Comments by Reviewer #1: Vincent Regard

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 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.

> We included more information on the placement of swath profiles in lines 321-323, and a second site at Punta Galera in Ecuador to show the placement of swath profiles and sensitivity tests to swath width.

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?

> We included the exponent to adjust the QR value according to the observed natural distribution of distances from referencing points, which follows an exponential relationship (Fig. 4D). For clarity, we extended the explanation of the exponential term in text (lines 399-400).

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?

> To account for the potential uncertainties in the age of a terrace level, we use the distance to the nearest referencing point and the confidence of its age constraint, both of which are included in the QR. We extended the explanation of these topics in lines 298 and 301.
>
> Le Roux et al. (2000) proposed subsidence to have occurred during the Pliocence and early Pleistocene. However, it is difficult to extrapolate this subsidence to Late Pleistocene time scales. Furthermore, the study of Le Roux et al. (2000) is a local analysis that cannot be easily extrapolated to regional scales. Other authors such as Macharé and Ortlieb (1992) proposed steady-state conditions or subsidence based on qualitative observations. We are aware that terrace mapping is challenging in north-central Peru and referencing points are less dense compared to other areas farther south; however, our estimates account for the uncertainties related to reference-point distance in the quality rating.

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 indicative meaning is taken from Lorscheid and Rovere (2019) and consists of the range between the lower and upper limit of sea-level formation – the indicative range – as well as its estimated position – the reference water level. Instead, "referencing points" are previously published terrace heights and age constraints. For clarity, we extended the explanations on these terms in lines 333-336 and 298-301, respectively.
>
> We added a short explanation of the abbreviations in the text (lines 472-473) and in the caption of Fig. 5 (lines 478-479), and we followed the reviewer's suggestion of adding the nomenclature in Fig. 6-9.

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.

> To account for the uncertainty between current shoreline-angle elevation and sea-level position, we used the indicative meaning (Fig. 11F, WALIS database, lines 74-76, 333-336 and 724-726). The indicative meaning consists of the range between the lower and upper limit of sea-level formation – the indicative range – as well as its mathematically averaged position, which corresponds to the reference water level (Lorscheid and Rovere, 2019). All these values are incorporated in the WALIS database and the reference water level is included in the discussion figure (Fig. 11). We changed the corresponding description in the methods (lines 333-336).

Specific comments

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

> Flat and steeper subduction angles are now introduced in lines 99-106.

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

> We are convinced that an introduction to the major faults and fault systems is important because we refer to them later in the results and discussion sections (see also Fig. 6-9, Fig. 11). We added more information to this paragraph to make it clearer and added the references indicated (lines 141-147).

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

> We agree and rearranged this part and added some information to make it easier to understand (lines 375-400).

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.

> The point of this study is to review previous studies and providing an almost continuous and methodologically uniform terrace mapping. Our study allows for different regions along the South American margin to be compared. Terrace-elevation estimates are denser and more precise in areas with well-preserved terraces and age constraints from previous studies. Information on marine terraces in north-central Peru can be found in the results section (4.1.2.). Their less-well developed morphology and sparse availability of age constraints is reflected in the quality rating.

Technical corrections

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

> We included two more references.

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

We moved the reference Regard et al. (2010) from line 55 to line 50.

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.

We changed the references from Pedoja et al. (2014) to Pedoja et al. (2011).

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

The increase in coast-trench distance. We added this to line 190.

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

We briefly mention this ongoing discussion and added this reference to lines 196-197.

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?

We rewrote this part for clarity as "The DEMs were converted to orthometric heights by subtracting the EGM2008 geoid and projected in UTM using the World Geodetic System (WGS1984) using zone 19S for Chile, zone 18S for southern/central Peru, and zone 17S for northern Peru/Ecuador.".

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

We changed the caption of this figure (now Fig. 4).

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.

The factor of 1.2 in the 3rd coefficient is added to maintain the possibility of QR = 5. We added an explanation in lines 396-397.

We rearranged this part to make it more coherent with the QR equation, its range (1 to 5) is now indicated directly before (lines 375-376).

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

We do not mention a correlation. The higher number of measurements results in a more accurate representation of the measurement errors, since less measurements sample a smaller part of the topography. We extended the explanation in line 631.

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

We included this reference.

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

We added the reference Anderson et al. (1999) and Trenhaile (2002).

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

Since we observe a positive correlation between wave height/tidal range and the number of terrace measurements (Fig. 11), we infer that wave energy predominantly controls coastal erosion and marine terrace formation rather than rock resistance or other characteristics – this is in line with most geomorphic concepts on this subject.

Additional References

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Trenhaile, A.S., 2002. Modeling the development of marine terraces on tectonically mobile rock coasts. Marine Geology 185(3-4), 341–361. doi:10.1016/S0025-3227(02)00187-1.