Dear Professor Mudd

Thanks for the opportunity to review this paper. The author did a good job addressing all my previous comments and corrections. One of the major issues, the emphasis of the paper is now equilibrated; intro and discussion are matching pretty well and the focus was switched from seismogenesis to mechanisms of surface deformation. Deformation mechanisms are now discussed considering the wavelength of structures and patterns of surface deformation, which is a major improvement and fix one of the main problems highlighted in the previous versions. Minor problems like typos and repetitions have been corrected and the text considerable improved. I am really satisfied with this new version and I think is ready for publication.

All the best

Dr. Julius Jara
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Response to RC3:

This work studies the exceptional exposure of marine terraces along the Makran coast in Iran. This is a quite interesting study that attempt to integrate previous chronological constraints on these terraces with novel ages based on multiple approaches (OSL/14C/U/Th); in addition, the authors use detailed mapping and morphometry to estimate the patterns of surface deformation in this area, then used to discuss the source and mechanisms of such deformation in the context of the Makran subduction zone. The authors do a good work attempting to join the different ages obtained, which in some cases are not easy to interpret. One of the controversial points is the presence of MIS 3 terraces, which are apparently related to localized high uplift rates. The presence of MIS 3 terraces is rare, but they discuss all the pros and cons for this interpretation. I personally find the paper clearly written with some minor typos and some issues; however, in general they clearly explain the logical steps behind their interpretations, which is the good way to do science (e.g. Section 5.2). The quality of the figures and the fashion used to display the distribution of the terraces are excellent and quite original, also the final interpretation about the possible mechanisms of tilting are nicely explained in the corresponding figure.

My main critics comes from:
1) The authors refer to active structures (Section 5.2 and 5.3.1) to explain local variations in uplift rates but the description or reference to these structures are poor, most of them based on speculative faults not observed on the field or mapped by other authors, then this part of the discussion becomes light and not convincing it all, specially about the kinematics of these structures. I must highlight that in general long wavelength deformation patterns are usually associated to deeper sources of deformation, such as the subduction megathrust, instead short wavelength deformation patterns are usually associated to shallower sources of deformation like crustal faults, I think that framing the interpretations based on these concepts may provide a more convincing discussion on the sources of deformation (e.g. asperities or subducted seamounts are related to deeper sources of deformation, instead abnormal local high uplift rates could be related to crustal faults, etc.., this would also help to complement section 5.3.2).

First part. We agree that speculating on the presence of normal faults is an easy an unconvincing interpretation, and so, we have entirely removed it from the text.
We thank you for your valuable input regarding the interpretation of uplift patterns. We have taken your comments and suggestion into account and have thoroughly modified section 5.3 accordingly.

This solve the critic 1, nice addition

2) In sections 5.3.2 and 5.3.3 the authors discuss the deformation patterns and uplift rates of marine terraces in the context of the subduction earthquake cycle, I am aware that the historical records of earthquakes are scarce but I feel that the topics or paradigms mentioned in the introduction are weakly resolved, so I find their final interpretations and discussion a bit frustrating not fulfilling the expectation introduced at the beginning of the manuscript.

We have modified the text to make it clearer that uplift rates patterns obtained from marine terraces are averaged over multiple seismic cycles and therefore, only give relevant information on the long-term signals rather than on the scale of individual earthquakes. This was adapted in the introduction (p.2 l.5).

The critic 2 was correctly addressed

Minor/moderate comments:

Page 2 Line 28: "ten sequences"? Or ten levels of marine terraces?

We are not referring to terrace levels, but to 10 different terrace sequences (1 in Jask, 1 in Tang, 1 in Gurdim, 2 in Konarak, 2 in Chabahar-Ramin, 1 in Lipar, 2 in Pasabander, as shown by the N-S profile in the terrace maps), each with its own different succession of levels. In fact, we really end up looking at the global lateral variation rather than punctual profiles, but these profiles represent the different "independent" sequences observed.

Thanks for this explanation, the meaning of sequence was confusing in the earlier version

Page 3 Line 27: "different tomographical properties"? this is ambiguous, what they describe in the tomography?

We have edited the text to make it clearer that different seismic wave velocities between the two segments have been detected with tomography (p.3 l.29).

Well addressed

Page 4 Line 2: slope sedimentary rocks? Do you mean Sedimentary rocks of slope depositional environment?

Yes, we have edited the text to make it clear (p.3 l.3).

Correction accepted

Page 4 line 7: omega shaped?

It refers to the geographical shape of the bay similar to the greek letter omega Ω (a term often used to describe the Makran bays) (see fig. 1).

Thanks for this explanation

Page 5, line 27: "TanDEM-X (0.4 arcsec/_12m . . .." This is repeated in page 2, there are also several other repetitions along the text

We have edited the text to correct those mistakes as much as possible.

Yes, I notice the current version much more improved

Page 7, line 13: "OSL dating", as I understood, you tried with quartz but then decided to use IRSL technique in feldspars, maybe is better state IRSL dating method instead of OSL, here and along the text as the results presented comes from IRSL.

IRSL is one of the methods of optically stimulated luminescence dating; hence, the terms are synonyms. We use the term OSL, because it is more commonly used and recognizable by the scientific community, but we state in the methods that we used IRSL (edited and emphasized, p.7 l. 23).

Not addressed.

Maybe thermo luminescence would be a more general term, but OSL or also know as "quartz OSL" is a method specifically for the analysis of quartz using the full light spectra. IRSL or post-IRSL are not a type of OSL.

Page 8, line 15: "nearest sea-level highstand" and also refer Jara-Muñoz et al., 2015. This is not correct it all, usually we use the age of the immediately preceding sea-level highstand, as the deposits are accumulated during the sea-level drop that follows the higstand.

We apologize for this mistake. We have corrected the text (p.8 l.18).

No need to apologize, nice additions in text

Page 8, line 25: the author use equations to estimate the minimum and maximum uplift rate, why do not propagate the errors like have been classically done by other authors before? (e.g Gallen et al., 2014)

We use the same method as in Pedoja et al. (2018a, 2018b). This method slightly overestimate the error compared to the standard error propagation. We have edited the text (p.8 l.32) and added a tab in the supplementary data B (B.4) where we calculate uplift rates and errors using the standard error propagation. The uplift rates profiles from the map figures was left the same.

This is well addressed

Page 9, line 18: The concept of eroded shoreline angle sounds weird, and also the way to estimate its elevation, is not clear which part is used to estimate the elevation of this feature, as the paleo-platform can extend seawards for long distances its elevation can display wide variations.

We are aware that it is an uncommon (unique?) feature, and it is not straightforward to describe it. We have modified Fig. 2a and emphasized the reference to field pictures (Fig 2h, and Figs H in dataset) that illustrate the concept (p.9 l.23).

This correction was correctly addressed, but I never heard about a terrace feature like this before, would be nice to see it on the field.

Page 9, line 21: "their altitude might underestimate the reality"? do you mean their altitude might represent minimum estimate?

Yes. We have edited the text to make that clearer (p.9 l.27).

Correction accepted

Page 9, line 24: ". . . we calculated uplift rates for each sample. . ." but before you mention that uplift rates are not calculated using the sample elevation but the shoreline angles, this is contradictory.

We have changed the text from: (p.9 l.29)

We calculated uplift rates <u>for</u> each sample using the terrace shoreline angle situated directly northwards of the sample (Table 3) (i.e., perpendicular to the trench) …

To

We calculated uplift rates <u>at the longitude of</u> each sample using the terrace shoreline angle…

Ok, this more clarifying and corrections are well addressed

Page 9, line 25: this is contradictory, here it says ". . . we subtracted the sediment thickness observed on the field. . ." and in line 29 it says ". . . we subtracted a general value for the thickness of the sediments..." so, what you really did?

We did both. For the samples, we were able to visit the shoreline angle on the field and measure the exact thickness of the sediments. However, for the large scale calculation that we do for the whole ranges of shoreline angles extracted from the DEM (>>100 km), we only use a general value because we could not physically go and measure the sediment thicknesses everywhere. We have adapted the text to make that clear (p.9 l.30-32).

Ok, this more clarifying and corrections are well addressed

Page 10, line 11: "results of radiometric. . ..." this definitely is not the best way to start a paragraph, please use topic sentences here and in some other paragraphs of the manuscript. Page 11, line 26: remove "mostly"

We have edited the text accordingly.

Addressed

Page 14, line 5: "The observation of normal faulting in a convergence context is intriguing". Actually this is very common in the fore arc of the Andes where normal fault are a result of crustal bending and several other processes, not suggesting subsidence (e.g. Lowless et al., 2010; Melnick et al., 2012; Melnick et al., 2009).

We agree that normal faults actually are observed in many subduction zones (not only in Chile, which is the typical example, but also in Japan, NZ, Greece,…), it nonetheless remains an intriguing fact not always correctly understood. We have adapted this part in section 5.3.

Addressed

Page 15, line 9: "Those few terraces that are not tilted. . .. Might provide insights on the uplift component directly linked to subduction dynamics. . .." I disagree, as mentioned in the major comments is the pattern of deformation which may provide insight about the mechanisms of deformation either long or short wavelength may provide insights about deeper or shallower sources related to crustal structures or megathrust deformation.

We agree, see comments to the first part.

Yes, I notice that your discussion is now more solid, good work!!

Finally, I must say that this is an interesting paper with high potential to be a great contribution after improving and correcting some of the issues described before.