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Interactive comment on "Dating and morphostratigraphy of uplifted marine terraces in the Makran subduction zone (Iran)" *by* Raphaël Normand et al.

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Article contents This work focusses on marine terraces as uplift markers along the Makran coast, in SE Iran/SW Pakistan. The Makran is an actively deforming emerged accretionary prism, developing over the north-dipping subduction of the Arabian plate. The work is based on ∼13 study sites distributed over hundreds of kilometers along the E-W coast. The data is made of geomorphic, tectonic and stratigraphic observations of marine terrace succession and organization. Dating attempts are presented with various methods (OSL, C-14 and U/Th).

General comments This is a valuable detailed work on marine terraces along the

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Discussion paper

Makran's coast. Unfortunately, the dating constraints are not always clear, but the authors made an interesting work in explaining quite clearly how these apparently confusing dates may be coherent, as evidenced by constant uplift rates through time. A striking point is the presence of unusual MIS3 terraces, supported by two scattered OSL ages (34 and 51kaBP). The data is not enough to convince me, but I have no alternative hypothesis. Thus I advise the authors to explore the possibility OSL dates are erroneous, in order to strengthen their conclusions. My main criticism comes from tectonics. The Makran is an active accretionary prism, probably hosting important thrusts. The authors only write about normal faults with limited Pleistocene offsets. The current tectonical framework is too light to understand the relationships between faults (either normal/inverse) and the uplift rate distribution. I think this problem could be addressed through mapping the main structures in figure 1 (both parts) and improving the background and discussion. I have some other comments about the timing (see thereafter). Apart to these criticisms I have been impressed by the quality of illustrations. There are lot of information reported: this paper must become a reference for future studies in the area. I support the publication of this manuscript.

Major comments. About tectonics. My experience at the western edge of the Makran is that there is probably active deformation, not extensional. My feeling is that your normal faults correspond to minor expression of the system. For example, uplift would be more easily explained under compressional deformation rather than through normal faulting (part 4.3, p14.line 7). This would agree with what you state p15, l. 9-10: "Nevertheless, our results indicate that the internal part of the accretionary prism is still accommodating significant internal deformation and uplift, as also shown by Haghipour et al. (2012)". Also, the Jask terrace is nearby an active fault (Peyret et al., 2009). A map of active deformation is mandatory, with maybe the position of the trench (or the Makran frontal thrust). About MIS3 terraces. I am doubtful about your scenario (Beris-Pasbander area). OSL is not always perfect, and If in your scenario MIS3c corresponds to a true highstand, this is not the case for MIS3a. Considering that a large majority of the marine terraces documented on earth developed during a highstand, and that this age leads to uplift rates largely higher than those shown by older terraces, I suspect a wrong scenario. Are you sure this does not corresponds to a MIS3c terrace?

Moderate comments. P.3 l.3-4. Uplift variations along strike is sometimes the consequence of the subduction of asperities/aseismic ridges (see the extensive literature on the subject). P3. Lines 16-24. There is probably along-strike variation in the convergence accommodated across the Makran, due at least to the East Lut fault (for example Walpersdorf et al., 2014). You must evoke this fact. P6 l14. “Platform carving usually happens at the beginning of the highstand, until the platform gets too wide and the wave energy too dissipated to carry out effective cliff erosion”. I agree with this ‘theoretical’ statement. I say ‘theoretical’ because it has only been observed on numerical models. Actually, there is very few data about the nature, but cosmogenic nuclides contradict this view (Regard et al., 2012; Hurst et al., 2016). There is no consensus. . . P6 l17-19. Here again, this assertion is not always true. One counterexample may be found around the Somme Bay in France (50°10’N and 1°30’E). To the southwest (Ault) it passes to the Normandy chalk cliff coast. The area between Cayeux and Ault is made of sand ridges (former sand spit) sedimented over a rock shore platform. The sedimentation begun after the Holocene sea level rise at ∼5-6 kyrs BP, as observed on others lidos (like Venice). OSL Dating. I understand the technique but I am not a specialist. I have not reviewed this part. When there is no shoreline angle, you used the back of the terrace. I agree with you but I do not understand that you do not use it as a minimum value, instead of using it as an approximation? (p9, l20). P10 L15. You claim the radiocarbon are limited to 20ka. I do not agree! There are good calibration curves and also good radiocarbon results for more ancient periods. P11l2-5. You cannot write about Jask without mentioning the local active tectonics! P11 l25. I do not understand your argument. The lateral shift comes from the relief. Here, if the terraces are close, it indicates the relief is high enough to carve different terraces in a narrow zone. P14 l16-20. If terrace location and uplift are driven by the limits of earthquake rupture (Saillard et al., 2017), then, as earthquakes occur generally at the same depth on the subduction interface, the uplift distribution is expected to be function of the distance to the trench. Is it the case in your dataset, as suggested by the high uplift rates of the areas closest to the trench (Pasbander)? Supplementary material Figure C1 and tectonics. I do not find your diagnostic of normal motion is convincing. The Figure shows a fault that morphologically looks like a thrust, and you do not indicate what are the lithologies (and ages of them) affected by the fault.

Minor comments. TanDEM-X DEM (P5 l26). I used one in Peru and I observed a systematic offset. Have you verified your sea level corresponds to the elevation of 0? Digitization (p5 l27). What is the resolution to which you digitize? Normand et al. 2018. I had not been able to access to the dataset. Also, I am not sure it is at the right place in the reference list. Uncertainties p8 l23-27. Although it is clear in following parts in the manuscript, you might explain why you differentiate min/max where a correct formulation would be ∆U=√(ãĂŰ((∆E+∆e)/(E-e))ˆ2+(∆A/A)ãĂŮˆ2) P9,l7. Due to the age uncertainties of the δ18O records, the timing of the chosen sea-level curves is not well constrained (Shakun et al., 2015; Spratt and Lisiecki, 2016).My experience revealed that timing uncertainties are much less influencing the overall uncertainty than uncertainties on e. Sea also the interesting paper by Caputo (2007). P12 l24. I would not qualify MIS3 of lowstand. MIS2 and MIS4 are lowstands. Odd MIS must be highstands, even if MIS3 is a particular case. Figures: the graphs showing uplift rates and shoreline angle altitudes lack horizontal scale and cross section labels. Figure 3. They are not Uplift max and min but uplift rates max and min. Figure 4. “Pre-Holocene”

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