

Dear Editor,

We would like to thank you and Dr. Kim Cohen for the careful and constructive evaluation of our manuscript and the associated WALIS dataset. We particularly appreciate Dr. Kim Cohen's detailed reading of both the manuscript and the database, which helped us identify and resolve several issues, particularly regarding vertical land motion/uplift rate estimates calculated over different time intervals.

We have carefully considered all comments and revised both the manuscript and, where necessary, the corresponding database entries. We have also addressed all detailed corrections provided in a track-change version of the manuscript. In addition to addressing the reviewers' specific comments, the manuscript has undergone a comprehensive language and structural revision. Several paragraphs were thoroughly rephrased to eliminate redundancies, resolve ambiguous geological formulations, and enhance overall clarity and readability. We have ensured that the terminology, citation styles, and units adhere strictly to the ESSD guidelines, resulting in a more cohesive and fluid text. Below, we provide point-by-point responses to the main issues raised by Dr. Kim Cohen.

Reviewer comment:

This regards Line 22 (Abstract), Sections 3.1.1.x, 3.1.2.x and 3.1.3.x on series of sites along uplifting sections of Iran and Oman coast, and Section 4.1.1. (Discussion of uplift in Iran and Oman).

For some sites, the quoted long-term rates (also called VLM estimates in the database, unit m/kyr) appear based on the time 'from LIG to now' (e.g. 3.1.1.5 Lipar). For other sites, the uplift rates quoted (in text, or solely in dataset, e.g. 3.1.1.1 Haleh) it appears to have also included 'older-than LIG terraces', perhaps qualitatively only, perhaps quantitatively (pre-LIG MIS ages assigned to above-LIG preserved levels: explicit mention for 3.1.2.1 Kish Island). For yet other sites (Northern Oman) there is 'very long' uplifted setting record (twelve terraces, i.e. many terraces older than LIG). The point then becomes: how long is long in 'long-term'? For yet other site (Oman Sur Lagoon) there is no mention of uplift in the text, and a high value 'for during LIG' (op. cit. note field) entered in the VLM field. As I read it in the database note field, this would be a short-term uplift event all most, recorded during the highstand. This is a different entity than a long-term VLM rate.

I think the manuscript can be improved on this, by mentioning the differences in duration times for the uplift estimates consistently in the text (and signalled in the database note field). It is currently appropriately mentioned for some of the sites, and left ambivalent for others.

Thank you for bringing this to our attention. We acknowledge that using the term "long-term" without specifying the precise temporal frame can introduce ambiguity

To resolve this ambiguity, we have thoroughly audited the manuscript (both in the Abstract and throughout the Main Text) and replaced or defined all generic instances of "long-term". Depending on the context, we have updated the phrasing to explicitly state the relevant time scale.

Reviewer comment:

In line 32–34, early in the introduction, the manuscript mentions that Pakistan coast line has no records, where Iran and India have. Iran coast line is indeed reasonably covered. India coastline has long geographic gaps in it too (Fig. 1 shows). I recommend to move the remark from the Introduction to the Discussion section, and briefly mention and address also why along the India coast there are substantial data gaps. I understand this will have part access, national research tradition and such reasons, but there could also be physiographic reasons to discuss.

We agree with the reviewer. The original wording could give the impression that the geographic gap is restricted mainly to Pakistan, whereas Figure 1 also clearly shows substantial stretches of the Indian coastline without standardized MIS 5 records.

Following the reviewer's suggestion, we have removed the specific statement showing Pakistani LIG data gap from the opening part of the Introduction and moved the discussion of geographic gaps to the Discussion section 4.1.2. There, we now consider both Pakistan and the poorly represented eastern and western sectors of the Indian coastline.

We believe that relocating this discussion provides a more balanced description of the spatial coverage and limitations of the present compilation.

Reviewer comment:

Oman Sur Lagoon database entry (4501, 4502, 4541): check the recorded VLM rate there.

We thank the reviewer for drawing our attention to the VLM rate in Oman, particularly the Sur Lagoon data point. We rechecked the Sur Lagoon entries and the original source of information. The original database could indeed be interpreted as applying a high uplift rate reported for nearby localities directly to the Sur Lagoon SLIPs and as representing uplift since the LIG because the Sur Lagoon indicators themselves occur close to the expected elevation during MIS 5.

In the original publications by Falkenroth et al. (2020) and Mauz et al. (2015), no specific VLM rate is reported for the Sur Lagoon itself. Therefore, we initially used the VLM uplift rate of 0.89 mm/yr for the Last Interglacial reported by Hoffmann et al. (2020), which was calculated from nearby localities, including Wadi Finn. We have clarified this distinction and revised the database entry accordingly. So, for more clarity, we have removed the uplift rate of 0.89mm/yr from the database entries of Sur Lagoon.

We thank the reviewer for identifying this issue, which helped us improve the consistency between the source literature, manuscript, and database.

Reviewer comment:

India Rameshwaram site (line 355–363): clarify the lateral/vertical facies/architecture relations between the two coral species patches.

Here we talk about lateral variation in the coral colonies.

We agree that the previous wording was unclear. In particular, the expressions describing the disappearance of the *Acropora* patch and the dominance of *Porites* could be interpreted as

implying a simple vertical ecological succession, although the description actually concerns the spatial distribution and preservation of different coral-bearing portions of the terrace.

We revisited the description and rewrote this part of the Rameswaram section with more clarity between the *Acropora*-bearing occurrence and the more extensive *Porites*-dominated coral deposits. We also removed phrases such as “gradually disappears” and “becomes dominated,” which did not accurately describe their spatial relationship. The revised sentence is:

“The 1 m² *Acropora* colony is absent 100 m to the south, where a large coral terrace (T2), approximately 10 m² in area, is present. This coral terrace is dominated by the coral genus *Porites* and is associated with *Diploastrea*, *Cycloseris*, and *Goniopora*”

Reviewer Comment

India Vettaikaraniruppu site (line 390): be neutral on calling this a highstand marker, as it may be transgression marker from before highstand.

We agree with the reviewer. The available chronology places the Vettaikaraniruppu record within MIS 5e, but this does not necessarily demonstrate that deposition occurred specifically at the peak of the Last Interglacial highstand. The deposit could instead record the transgressive phase leading toward the highstand.

We have therefore revised the terminology and no longer describe this record as a “highstand marker.” It is now referred to more neutrally as an MIS 5e sea-level/transgressive record, consistent with the chronological and stratigraphic information available.

Minor comments on the manuscript

Line 15: (critical revision = implies changing the original content; that is not what you intend to convey).

We thank the reviewer for this insightful comment. We agree that our intention was not to challenge or alter the primary field observations or original chronological data reported in the source literature.

Rather, to achieve complete consistency within the WALIS framework, we systematically re-evaluated the 54 sea-level index points by recalculating their paleo-RSL positions and associated uncertainties based on updated determinations of their indicative meaning (i.e., reference water level and indicative range).

To clarify our approach and avoid any misunderstanding regarding a “critical revision” of primary sources, we have removed the phrase “critical revision” and updated the manuscript text throughout (including the Abstract) to emphasize that the dataset was systematically compiled, standardized, and re-evaluated using the WALIS protocol.

Line 23: what is 'long term' is undefined/ambivalent in the abstract (ok) and, more importantly, in the main text. I posted a general remark on this.

We thank the reviewer for raising this point regarding terminology. To avoid any potential ambiguity or confusion regarding the specific timescales considered in each sector, we have systematically removed the generic phrase “long-term uplift rate” throughout the manuscript. Instead, we now explicitly state the exact time frame and reference period assumed for each

calculation (e.g., "uplift rate averaged since MIS 7 (~240 ka)" or "uplift rate calculated since the Last Interglacial (~125 ka)").

This adjustment ensures full clarity and precision regarding the chronological boundaries used for each uplift estimation across the manuscript and in the WALIS database.

Line 24 and 25: or near-zero VLM?

The wording has been changed in the revised manuscript from “low tectonic uplift” to “near-zero vertical land motion”.

Line 32 & 33: perhaps make this remark later on: in Discussion you can describe geographical gaps, and split that into 'for human reasons (limited availability of published data)' or 'for geological reasons' (tectonic setting, magnitude erosive impact Indus river system post-LIG). For large stretches of India coast (longer than the Pakistan coast), there is also a data gap (Fig. 1 shows clearly).

Thank you for the suggestion. We have moved the suggested part to the discussion part 4.1.2, and revised the manuscript, where we have added “In addition, a substantial geographical gap in LIG sea-level records still exists along much of the eastern and western coasts of India, primarily due to the limited availability of previously published data. A similar data gap exists along the approximately 1,050 km-long coastline of Pakistan, highlighting the need for further investigation and the development of well-constrained LIG sea-level indicators across the northern Indian Ocean region.

Line 49-51 You could add a remark, whether 'in all cases' you took over the original published age results - or whether you attempted a recalculation from the original lab data (because of procedural changes, older U-series dates are sometimes an issue).

The suggested remark has been added to the introduction part in the revised manuscript.

Line 79: How does this work? I would think an absolute error is more appropriate to apply. If I report a value of +0.1 m, 20% is just +/-0.02m (trivial, odd) I I report a value of +5.1 m, 20% is +/-1.2m (reasonable) I recommend to report the absolute error that the elevations got in the database. Table 2 reveals it is +/-5 meters for indicators found ca. 20-25 m above present MSL

We believe that this error is quite reasonable for the range of elevations we have in the paper. Obviously, this method should not be used in case the elevations are too close to zero, as the error will assume unrealistic low numbers.

Table RSL_4540: this is the only date that has no uncertainty reported

The RSL_4540 entry is now being updated according to the original paper (Brückner, 1989), where uncertainty is not clearly discussed, but the range of age is available. This is 95 (72.6-123.2 ka)

Line 107: This is very generally phrased. It can be phrased more precisely, and more linked to the LIG data target of the paper: it records repeated high stand phases (both from LIG and from older interglacials) with relative elevation differences between.

The sentence has been rephrased in the revised manuscript and connected to LIG.

Line 123; check the use of this word 'groups of exposures' You also list 4 groups, not 3

Thank you for this observation. We have revised the terminology throughout this section to ensure consistency. The term "orders" has been replaced with "terrace levels", and the total number of identified levels in the Haleh site has been corrected to four.

Line 126: I think in this particular case, the overlap with MIS 5e (ending) is so much larger than with MIS 5c, that I would only put MIS 5e here. (also given uplift rate notion and 5e reaching higher than 5c barystatically) 'most likely to MIS 5e'. MIS 5c is only inferred from geochronological uncertainty reporting 'in isolation'.

We agree that MIS 5e is much larger than MIS 5c. However, in the original interpretation by Reyss et al. (1998), the authors did not clearly distinguish between MIS 5e and MIS 5c. Following your suggestion, I have revised the age, and it now corresponds to MIS 5e

Line 128: For this site, you do mention older interglacial terraces at higher elevation than LIG, but there NO 'long-term' uplift rate is mentioned in the text. The Excel sheet DOES give a value of +0.2 m/kyr VLM for that entry. See also the comment further down, for site 3.1.1.5.

Yes, indeed. Older marine terraces were reported at Haleh by Reyss et al. (1998), but the authors only discussed the LIG uplift rates across the region. That is why we entered a VLM value of 0.2 m/kyr in the Excel sheet. I have also included the LIG uplift rate for Haleh in the main text in the revised manuscript.

Line 130 : see above terrace units

Replaced “order” to “levels/marine terrace levels”

Line 136: This site shares the same VLM rate of +0.2 m/kyr in the database. Not mentioned here in the text

We have added the VLM for the Jazeh MIS5e unit in the revised manuscript.

Line 145: you do not call this a 'long term' uplift rate here. This is more a mid-term uplift rate, the average rate over the time since LIG, not a for longer period with the LIG in the middle of it.

The entire sentence has been rephrased to be clearer:

“In WALIS, this terrace is catalogued as RSL_4532, with a shoreline angle elevation at 32 m a.s.l. Optically stimulated luminescence (OSL) dating of feldspar from the terrace T3 yielded a fading-corrected age of 141.3 ± 12.1 ka, which falls within the broader range of MIS 5e (Normand et al., 2019). In addition, the combined morphological expression, facies, and chronological evidence provide strong grounds for assigning T3 to the Last Interglacial highstand. The Konarak T3 terrace elevation allows Normand et al. (2019) to calculate an uplift rate for this terrace is 0.27 mm/yr since the Last Interglacial.”

Line 150 : an inset lower part

The entire sentence has been rephrased to be clearer:

“The Ramin coastal site, situated east of Chabahar on the Makran margin (Fig. 2), features a prominent marine terrace (T1) preserved at an elevation of 59.5 ± 2.0 m a.s.l., recorded in the WALIS database as RSL_4531. At this site, terrace T1 is composed of trough-cross-stratified marine deposits. Luminescence dating of sediments associated with the T1 platform yielded an age of 140.2 ± 12.5 ka, placing it within the MIS 5e (Normand et al., 2019). Conversely, a lower inset bench 3 m a.s.l. yielded an OSL of 78 ka, attributing this lower level to MIS 5a (Normand et al., 2019; Thompson & Creveling, 2021). The WALIS record (RSL_4531) and the geomorphic, chronological, and stratigraphic evidence at Ramin T1 together support its use as a significant but tectonically upward-displaced RSL indicator of Last Interglacial.”

Line 150 : ('inset' to signal architectural complexity of the Ramin T1), otherwise it would be strange to have the lower part of a sedimentary body younger than the top.

Thank you for the comment. We added the word “inset” in the manuscript.

Line 151: +3 m a.s.l.

Revised in the manuscript.

Line 154: mention the direction. upward displaced

The upward direction has been added to the manuscript

Line 172: are these 'long term' uplift rates? (perhaps reserve that term for uplift calculations also fed with older-than-LIG Quaternary terraces) or more like 'mid-term' uplift rates (based on LIG MIS5e, LIG MIS5a and present), For this site, it is the latter, as there is no mention of

further higher terraces. Earlier on, for the site 3.1.1.1 Haleh, you do mention older interglacial terraces at higher elevation than LIG, but there NO 'long-term' uplift rate is mentioned in the text. The Excel sheet DOES give a value of +0.2 m/kyr for that entry.

The entire sentence has been rephrased to be clearer:

Based on the combined elevations and chronological constraints of MIS 5e (T3) and MIS 5a (T1-T2) terraces, Normand et al. (2019) calculated an uplift rate from 0.25-0.57 mm/yr for the Lipar area since the Last Interglacial.

Line 180: you use the same phrasing for Qeshm island below as you do for Kish island here. Say it ONCE that it these two islands together are the best, and do it in line 175-177 (Section 3.1.2) not in island specific sections.

Thank you for the suggestion. Following your suggestion, we have discussed once the best documentation of LIG sea-level record of both islands under 3.1.2 heading.

Line 194: 'however' not needed to use

Removed “However”

Line 196: You can use stronger wording for this. Your database HAS it attributed to tectonic overprinting, as it registers an uplift rate.

We thank the reviewer for this observation. To streamline the narrative and avoid over-interpreting specific tectonic mechanisms without local structural data, we opted to remove the explicit discussion of "tectonic overprinting" from this section. Instead, we have refocused the text directly on the quantitative uplift rate estimates (0.13–0.24 mm/yr) and the observed terrace elevations/ages as recorded in the WALIS database.

The text in Section 3.1.1.1 has been revised accordingly to present these empirical observations clearly and concisely.

Line 199: here you DO mention that the rates are calculated/intended as an average over longer time than 'since the LIG'. To me, this is a long-term uplift rate.

Thank you for this comment. As detailed in our general response above regarding uplift rate terminology, we have removed the generic expression "long-term uplift rate" throughout the manuscript. To prevent ambiguity, we now explicitly state the exact reference period used for the calculation in each specific case

Line 202: you use the same phrasing for Kish island above as you do for Qeshm island here. Say it ONCE that it these two islands together are the best, and do it in line 175-177 (Section 3.1.2) not in island specific sections.

Thank you for the suggestion. Following your suggestion, we have discussed only once “ the best documentation of LIG sea level record” of both islands under 3.1.2 heading

Line 222: (i.e. in the same range as nearby Kish, where there is supporting pre-LIG highstand record too)

We thank the reviewer for highlighting this similarity. We agree that these values fall within the same order of magnitude as those reported for nearby Kish Island. In this synthesis, we chose to report the original values as calculated and presented in the primary literature to maintain strict fidelity to the published sources while ensuring consistency across the WALIS framework.

Line 244: This is a true long-term uplifting setting, with twelve levels preserved. See the above comments.

Thank you for the comment, yes indeed this is a long term uplifting setting.

Line 348 : for me a new word, but I get where it comes from alluvial-fan cemented-gravel 'conglomerate' deposit.

Thank you for the information related to the Fanglomerate.

Line 255-257: The text does not mention long-term uplift (or stability or subsidence) for this site (different from 3.2.1 Northern Oman). The database entries for Sur Lagoon mention a very high uplift rate of 0.89 m/kyr - which does not match the +4 m a.s.l. encounter position of the site. The note field in the database mentions "The nearby locations to Sur lagoon have an uplift rate up to 0.89mm/y during the MIS5 stage (Hoffmann et al., 2020)" It may be that a rate 'DURING' is misread as 'also SINCE'. This particular VLM database entry should be checked

We thank the reviewer for identifying this issue, which helped us improve the consistency between the source literature, manuscript, and database.

In the original publications by Falkenroth et al. (2020) and Mauz et al. (2015), no specific VLM rate is reported for the Sur Lagoon itself. Therefore, we initially used the VLM uplift rate of 0.89 mm/yr for the Last Interglacial reported by Hoffmann et al. (2020), which was calculated from nearby localities, including Wadi Finn. We have clarified this distinction and revised the database entry accordingly. So, for more clarity, we have removed the uplift rate of 0.89mm/yr from the database entries of Sur Lagoon.

Line 301 U-Th and U/Th in some other instances in the manuscript. U-Th is the more common.

Thank you for the suggestion. We kept U-Th uniform throughout the revised manuscript.

Line 340: recorded at

The “recorded” word has been added in the manuscript.

Line 356: see above U-Th or U/Th

We kept U-Th uniform throughout the revised manuscript.

Line 358: use a different word. Was it eroded? Was it a local mini-reef/local stand of these corals?

The 1m² Acropora colony was no longer recorded 100 m south, so the sentence has been rephrased in the revised manuscript for clarity.

Line 359 : use other words. Porites superceded the Acropora? Porites neighboured the Acropora? Dominant at the time? Dominant in what eventually preserved?

A large coral terrace (T2) is present 100 m south of the Acropora colony, approximately 10 m² in area, This coral terrace is dominated by the coral genus *Porites* and is associated with *Diploastrea*, *Cycloseris*, and *Goniopora*

The paragraph has been revised with more clarity in the revised manuscript.

Line 390 : remove as it could also be the Termination II /transgression part of MIS 5e

The word “Highstand” has been removed from the sentence.

Line 408: a considered reliable

The word “considered: has been added.

Line 413 : encountered

The word “positioned” is being replaced with “encountered”

Line 425 : registered

Thank you for the suggestion. The word “archived” has been replaced with “registered”

Line 441: show an elevation

The sentence has been revised

Line 453-454: signal if this is once more based on LIG terrace modern elevation (+assumptions), or if it is (also) based on tectonic modelling/reconstruction/geophysical measurements etc

This is the same LIG uplift rate which is calculated for the Haleh and Jazeh area by Reyss et al., 1998.

Line 461 : for MIS 5e highstand (i.e. +2 to +9 m a.s.l., Ref, Ref)

The sentence has been rephrased and the references have been added.

“This mechanism also explains why coral units of different ages are found at elevations significantly above global eustatic benchmarks for the MIS 5e highstand (i.e., 2 to 9 m a.s.l.; e.g., Kopp et al., 2009; Dutton and Lambeck, 2012). “

Line 478: forebulge of the Makran subduction

Sentence has been revised.

Line 480 : check this with the actual VLM entry in the database for this site.

From the direct publication on the Sur Lagoon, no actual VLM rate is discussed. Therefore, we used the VLM uplift rate of 0.89 mm/yr for the Last Interglacial reported by Hoffmann et al. (2020).

Line 502 : this is a bit vague use of the word system. You mean the Indian Ocean LIG shoreline record

The sentence has been revised