Response to Comments by Referee #2 to “Last Interglacial sea-level proxies in East Africa and the Western Indian Ocean"

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1 Summary

We thank Anonymous Referee #2 for their thorough review of our manuscript. In the following, we answer the main comments for the manuscript and database, as well as their corresponding minor comments. The original reviewer comments are in italics while our response is in plain text and the adjusted manuscript text is indented.

2 Major Comments

One of the authors' stated goals is to standardize reporting of sea-level markers so that they are comparable. In practice, this approach means categorizing sea-level markers, quantifying uncertainties in measurements and indicative range, and establishing the elevation of modern equivalents. This undertaking is challenging as the authors note since many authors, prior to the advent of GPS, do not adequately report their height measurements. This goal is a good one, but it is unclear how successful the authors have been because their documentation of this procedure is inadequate and inconsistent. The companion manuscript for this excellent database needs to very clearly and methodically spell out what the authors did to generate the database. For example the authors state that "in the literature we surveyed, it was often unclear how most datums were established", but in the description of each site, there is rarely an explanation of how the authors established their own datum or relative water level (RWL). Although this information is provided in tables and in the database, it is often very difficult and time consuming to cross reference everything. For example, I often really struggled to ascertain how site-specific RWL and indicative range (IR) values are estimated based upon the description in Table 1. The authors should strongly consider including systematic descriptions and methodological information for every measurement in the text

In order to clarify and streamline the manuscript, we have expanded Section 2 Methods and have included a breakdown of how PRS are calculated in database.

> **Surveying Techniques**
>
> Very few studies within the EAWIO have the express intent to establish detailed surveys of Last Interglacial (LIG)

sea-level proxies. This is especially true with respect to elevation measurements. Most surveys conducted during the 20th century do not report a methodology used in measuring elevations. It is not until the advent of Global Navigation Satellite Systems (GNSS) and Total Stations that surveys on many of these remote shorelines could be accurately documented. The elevation measurement techniques used in the studies that we compiled in the database are shown in Table 1. When no accuracy was given for an elevation measurement in the original study, the typical accuracy of the technique was used. Any elevation measurement must be related to a specific sea-level datum (Table 3). Unfortunately, in the literature we surveyed, it was often unclear how most datums were established (e.g. how the highest tide level was calculated at different sites). Instead authors will often state that the elevation is relative to mean sea-level or the level of highest seas. This uncertainty is exacerbated by the large variance in tides within the EAWIO, specifically in the immediate vicinity of the Mozambique Channel (Farrow and Brander, 1971; Kench, 1998). In the database, we therefore try to reflect this uncertainty within the elevation measurement for each proxy.

Paleo Relative Sea Level Estimation

In order to extract paleo relative sea level (PRSL) from measured elevations, the IR and RWL for the measured indicator are needed (Shennan, 1982). The IR relies upon the measurement of modern upper and lower limits of the indicator in relation to an established datum. However, few studies have thoroughly documented the upper and lower limits of the site specific modern analogue to the indicator. To supplement missing IR and RWL values, Lorscheid and Rovere (2019) introduced a reliable empirical method that uses a global dataset of wave and tide model outputs in conjunction with the morpho- and hydrodynamic formation environment of the most common sea level indicators. This methodology was then packaged into a open-access stand-alone software, IMCalc, availible at: https://sourceforge.net/projects/imcalc/ (Lorscheid and Rovere, 2019). In the database we use the upper and lower limits when given by original authors, however, the majority of upper and lower limits for our PRSL points are calculated from the IMCalc software. Once the upper and lower limits are determined, WALIS automatically calculates the IR, RWL, PRSL, and PRSL uncertainty, based on the schemes from Rovere et al. (2016). All of the PRSL elevations in the following text have been calculated from originally published survey elevations using this methodology in order to standardize their comparison.

A second issue is that it is difficult to determine at times to whose PRSL estimates the author are referring, or indeed whether they are referring to PRSL estimates or simply a height above an (often unspecified) datum. This way of writing is very confusing but very easy to fix! I would strongly recommend that the authors return to the text and ensure that every description includes: 1. Reference to the type of sea-level marker, its accompanying RWL, IR and a clear justification based upon the measurements and observations made in the primary literature. 2. The height reported in the primary work and above which datum (if defined, and stated if it is not). 3. The authors' own, updated PRSL estimate based upon the measurements that have been clearly spelled out.

For each WALIS ID# we have provided PRSL elevations except for the two terrestrial limiting points. We have modified the text to more clearly outline that all PRSLs are our calculated PRSL values. Within the database we include all the metadata available for each WALIS ID#. We fear that inserting the information asked by the reviewer (which are readily accessible in the database) would have the effect of overcrowding the MS, diluting its descriptive aim.

A more detailed, general methodological description and explanation of general difficulties/uncertainties should be included. This change will mean moving Section 5.4 into Sections 2 & 3 and expanding. For example, there is no discussion of specific problems with U-series dating. This problem is extremely important! There should be a short description of how authors screen their samples (calcite %, original U ratio etc.). There should also be a description of the problems of open-system behavior. In general, this point is poorly addressed in the manuscript. There are studies cited which use open-system age-determination schemes which are not referred to (e.g. Stephenson et al., 2019). These issues should be highlighted in the detailed site descriptions as well. Again, much of this information is buried in the spreadsheet but it should be clearly spelled out in the text as it is vital for non-specialists.

This comment echoes the concerns of Referee 1 and have moved Section 5.4 up to Section 2 as well as expanded the discussion of issues found during age determinations. We have also added, where appropriate, whether U-Series ages are open- or closed-systems. Below is the revised Dating Techniques subsection and the Uncertainties and Data Quality subsection. To help be more transparent with how we have addressed quality control, we have included the rating systems for both elevations and ages.

Dating Techniques

Early observations of paleo-shorelines relied primarily on chronostratigraphic constraints to try and piece together a regional narrative. Two formations are primarily used in early studies: the Aldabra Limestone (Aldabra, Seychelles) and the Karimbolian Limestone (Antsiranana, Madagascar). The Aldabra Limestone is characterized by reef limestones with large corals in growth position. Similarly, the Karimbolian Limestone, first described by Guilcher (1956), refers to massive reefs overlain by red aeolianites. Both of these formations have since been chronologically constrained using U-Series Alpha-spectrometry (Thomson and Walton (1972) and Battistini and Cremers (1972) respectively).

As with elevation measurement techniques, dating techniques within the EAWIO have advanced dramatically since the first chronologies became available in the early 1960s, thanks to U-Series ages from coral samples (Barnes et al., 1956; Thurber et al., 1965). In general terms, U-Series ages are derived by measuring the disequilibria between ^{238}U, ^{234}U, and ^{230}Th radioisotopes (Edwards et al., 2003). The reliability of ^{230}Th-ages relies on a closed-system behavior that can be compromised by post-depositional processes that lead a re-mobilization of uranium –or thorium- within the coral skeleton. Several mineralogical and isotopic screening criteria are generally applied to detect any opening of the ^{230}Th-^{234}-^{238}U system. The coral samples should show no evidence of diagenetic alterations such as recrystallization or transformation of primary aragonite to secondary calcite. This is generally assessed by quantification of secondary calcite. In most recent studies, coral samples showing a calcite content of more than 1% are usually discarded. The uranium content of fossil corals should ordinarily be similar to modern ones (about 2.8 ppm). The back-calculated $[^{234}U/^{238}U]_0$ ratio that represents the $[^{234}U/^{238}U]$ ratio at the time of coral growth should reflect the $[^{234}U/^{238}U]$ of seawater. Due to the long oceanic residence time of uranium, this ratio is supposed to be similar to the modern seawater. For this reason, fossil corals showing a $[^{234}U/^{238}U]_0$ significantly different from modern seawater were generally discarded (Hamelin et al., 1991; Bard et al., 1991). This isotopic criterion is not always strictly applied since 1) there are many evidences that the $[^{234}U/^{238}U]$ ratio of seawater may have varied through time (see discussion in Chutcharavan et al., 2018) and 2) some models, assuming decay-dependent redistribution of ^{234}Th and ^{230}Th were developed to correct for the "open-system" behavior highlighted by anomalous $[^{234}U/^{238}U]_0$ ratios (Thompson et al., 2003; Villemant and Feuillet, 2003). Although these open-system ages are questionable, some of the ages reported here are calculated using such model (Thompson et al., 2003). Here, we state whether if ages were originally reported as closed- or open-systems. It is important to note that the application of the screening criteria presented here are quite recent and that most of the studies reported in this review were carried out before these criteria became common practice in the U-Th community.

Precision of U-Series ages relies upon the analytical method used to measure the isotopic ratios of the sample. U-Series Alpha-spectrometry dating was the first utilization of ^{238}U decay, detecting and counting the ejected alpha particles. The counting statistics are on the order of a few precents of the $^{230}Th/^{238}U$ ratio, resulting in a best-case 2σ internal errors of ± 10 ka for an age ranging between 70 ka and 150 ka and more often than not 2σ internal errors closer to ± 20 ka (Broecker and Thurber, 1965; Thurber et al., 1965). Therefore, the majority of early chronologies within the EAWIO have limited accuracy, and can only be generally assigned to one Marine Isotope Stage. It was not until the 1990s that mass-spectrometry, particularly thermal ionization mass spectrometry (TIMS, Edwards et al., 1987, 2003), began to bring down the 2σ error allowing MIS sub-stage discernibility. Additional advancements such as multi-collector inductively coupled plasma mass spectrometry (MC-ICPMS) have brought the 2σ uncertainties under ideal conditions down to ± 100 a at 130 ka (Cheng et al., 2013). Besides biogenic carbonates, two terrestrial limiting chronologies from lithified dunes were established through the use of Luminescence (OSL). All uncertainties are stated at 2σ and, when needed, they have been converted from the 1σ values reported in the original papers.

Uncertainties and Data Quality

As discussed previously (Section 2.3), $^{238}Th/U$ ages are reliant upon the technique and transparency of metadata. While many earlier studies briefly refer to the methodology used, they often provide little, if any, analytical metadata. Within the database, we have accepted all $^{238}Th/U$ ages as reported by the original authors and have only reported recalculated ages from Chutcharavan and Dutton (2020) which utilize ^{234}U and ^{238}Th decay constants from Cheng et al. (2013). Each chronological constraint has been rated using the common guidelines provided in the WALIS documentation (Table 2). Additionally, we have reported open-system ages for samples that are derived from mollusks (e.g. *T.Gigga*), which are widely accepted as providing inconsistent ^{238}Th/U age reliability (e.g. Ayling et al. (2017)) and therefore have been assigned a substage age rather than an outright age. Data with quality higher than 4 (good) (Figure 1c) are from the most recent studies within this region and are those who have adopted more rigorous sample screening procedures and have access to the most recent advances in mass-spectrometry (e.g. MC-ICPMS).

Table 1. Quality rating guidelines used for evaluating PRSLs. Exported from WALIS (Rovere et al., 2020)

Description	Quality Rating
Elevation precisely measured, referred to a clear datum and RSL indicator with a very narrow indicative range. Final RSL uncertainty is submetric.	5 (excellent)
Elevation precisely measured, referred to a clear datum and RSL indicator with a narrow indicative range. Final RSL uncertainty is between one and two meters.	4 (good)
Uncertainties in elevation, datum or indicative range sum up to a value between two and three meters.	3 (average)
Final paleo RSL uncertainty is higher than three meters.	2 (poor)
Elevation and/or indicative range must be regarded as very uncertain due to poor measurement/description / RSL indicator quality.	1 (very poor)
There is not enough information to accept the record as a valid RSL indicator (e.g. marine or terrestial limiting).	0 (rejected)

Table 2. Quality rating guidelines used for evaluating age information. Exported from the WALIS (Rovere et al., 2020)

Description	Quality Rating
Very narrow age range, e.g. few ka, that allow the attribution to a specific timing within a substage of MIS 5 (e.g. 117 $\pm$ 2 ka)	5 (excellent)
Narrow age range, allowing the attribution to a specific substage of MIS 5 (e.g., MIS 5e).	4 (good)
The RSL data point can be attributed only to a generic interglacial (e.g. MIS 5).	3 (average)
Only partial information or minimum age constraints are available.	2 (poor)
Different age constraints point to different interglacials.	1 (very poor)
Not enough information to attribute the RSL data point to any pleistocene interglacial.	0 (rejected)

3 General Comments

Figure Comments *Figure 2 – It would be useful to have this map labelled with places described in Section 4. E.g. I can't find Sanaag on the map! I think it is labelled as the Gulf of Aden.*, *Section 4.1.2 – Label Banaadir on map (Figure 2).*, *Section 4.4.1 – Add location to map.*, and *Please put Maputo on the map.*

Unfortunately, adding such data to the map would be impossible, due to the high clusters of points present. While adding one map for each region would be feasible, we fear it would sum up to a much longer MS. Therefore, we prepared a standalone HTML file, that we provide as annex, where the user can navigate data and samples, getting more information as they read. We hope this is an acceptable compromise.

Section 3 – There is no mention in this section of the effects of alteration of samples by diagenetic process etc. This problem is a significant one and can lead to much larger, and ill-defined uncertainties than those quoted. I think this section also needs some description of open-system modelling where there is evidence of open-system behavior (e.g. due to an original U ratio that differs from that expected for sea water).

As stated above, we have expanded the Dating Techniques subsection to include a brief explanation of open-system behavior as well as referenced the respective open-system model individual authors used for site specific age calculations.

Section 2 and Section 4 have pretty much the same heading but one is introductory and the other includes the detailed site descriptions. Is there a way to rationalise this structure? Section 2 maybe should be called "Paleo Relative Sea Level Determination"?

To address this confusion we have renamed Section 2 (now subsection 2.5) "Paleo Relative Sea Level Estimation".

What is the logic behind which study sites get a Figure? I think these Figures are great to include, but it seems a little bit random which ones are included and which are not. For example, Why are figures not included for Stephenson et al (2019) and Dutton et al (2015) if these are the two high-quality sites, as presented on Figure 2b&c? Similarly why are photographs included for some locations and not others? Obviously photographs may not be available for some sites, but it seems sensible to include photos from Stephenson et al (2019) and Dutton et al (2015) since they are the high- quality locations.

We did not include sketches from Dutton et al. (2015) and Stephenson et al. (2019) because these two studies particularly focus on sea-level reconstruction and when incorporating their data into our database, the morphological nature of the respective outcrops is not as relied upon as with many of the earlier studies where the PRSL proxies are very much "extracted" from geomorphological descriptions of outcrops. As for the photos, we have added only photographs taken by the authors of the MS, for which we have the rights to reproduce. The two papers cited by the reviewer are widely available and contain high-quality photos, therefore we decided not to include any photographs that might require copyright clearance.

Section 4 - For this paper to be an excellent companion to the database much greater description is needed. At the moment the reader has to dive into each paper to find the details of the field work. A few sentences of concise and consistent description for each study would help enormously. In general the data are often only partly reported. The reporting system needs to be more systematic in the text so that the reader can extract all of the information that they need without looking up the sample numbers in the spreadsheet all the time, which I found quite frustrating. If a user is looking for why a particular datapoint might be an outlier, it is going to be a torturous process at the moment when all the information could be in the text. Sometimes ages are reported but not elevation. Sometimes elevation is reported as recorded by the original authors and sometimes it is the authors' updated PRSL estimate that is reported. This chopping and changing makes it quite difficult to follow what is being referred to and I would strongly suggest that the authors try and make their reporting approach more consistent. This change shouldn't be hard but would help enormously!

Within the manuscript we do not focus on the field work element because the majority of RSL proxies are derived from papers that do not describe surveying techniques (summarized in Subsection 2.2 Surveying Techniques). We therefore try to focus on the standardized data in order to enable the comparison of data. As for the confusion between originally stated ages and the calculated PRSLs, we have gone through and modified the text to more explicitly tie the ages, and therefore the WALIS ID#s, the calculated PRSL values.

Additionally, it needs to be clear where the authors are using indicative meaning based upon the published work's modern analog data, and where they are using IMCalc. If they are using IMCalc, what are the inputs?

As stated above, we have expanded the section where we introduce the calculation of PRSL values. This includes an introduction to the IMCalc app and its inputs.

Section 5.1 – I wonder if there is an opportunity for the authors to conclude anything from their impressive database on these points? As the first compilation of these data it seems a shame for the authors to leave it to others to find paleo sea level signals? It is not essential in a data publication such as this one, but it seems like a little bit of a missed opportunity.

While it is tempting to draw conclusions from the data standardized in this database, this is beyond the scope of the project. It is the hope that future studies can take advantage of our database (as stated in our Future Directions section) and apply GIA corrections to draw inferences on regional or eustatic sea level.

Section 5.3 – This section is extremely cursory! Woodroff et al (2015), Braithwaite et al (2000) etc. report Holocene data from the Seychelles; Stephenson et al (2019) and Battistini (various) report a few Holocene dates from Madagascar; Camoin et al (1997) report a whole suite of U-series dates from Reunion, Mauritius and Mayotte. The authors should either remove this section or add in significantly more data. The equatorial location of this region means that Holocene terraces at 1–2 m elevation are very common indeed.

In deed, this section was far too cursory and had focused more on controversial deposits that have been attributed to both Holocene and the LIG. We therefore removed the section from the manuscript. Other database projects, such as HOLSEA (Khan et al., 2019), are more appropriate repositories for standardized Holocene sea-level data.

Section 5.4 – this section should be removed and the discussion added to Sections 2 & 3. I think it is important the the reader has a sense of where the uncertainties come from before reading the results. This explanation also needs to be significantly expanded to describe the procedure for determining the authors' standardised PRSL, which is quite opaque at the moment – see comments above and below!

Again, we have moved this subsection up in the manuscript to Section 2.6 as well as expanded the explanation of our methodologies used in determining standardized PRSLs.

There is a data point from Mayotte in the Comores that I think is missing from the database that the authors should consider including. See Camoin et al. (1997) "Holocene sea level changes and reef development in the southwestern Indian Ocean". Coral Reefs, 16, 247-259. and references therein. There is no U-series date but I think these islands should be mentioned for completeness.

Thank you for pointing Camoin et al. (1997) out. We have reviewed the text but cannot include it in the database as no chronological constraints are available. We do however mention it in passing as evidence to the lack of other MIS 5e RSL proxies on Mayotte.

Can the data in Table 4 be in numerical order? It is incredibly difficult and frustrating to find WALIS ID# in this table. I ended up sorting the spreadsheet numerically which not all readers may have immediately to hand.

This is a very good point for quality of life. We have reordered the table to reflect WALIS ID#s.

4 Detailed Comments

We feel that many of the below comments have been answered by the responses provided above. We therefore in many instances state, “Please see responses above."

L24 – You state that Battistini's (1984) Tatsimian is “MIS 11 or 7?” yet on Figure 1 you have only MIS7. Is there a reason for this difference? Consider standardising.

We have standardized the Tatsimian to reflect the usage in Figure 1.

L54 – The authors quote ages here but haven't done so for any of the previous locations. Is there a reason for this difference? It might be best just to introduce and cite the authors here and then quote ages in the detailed description later.

We agree with this comment and have removed references to ages from the introduction.

L65 – If this database is to be used by non-experts, then it would be helpful to have RWL, IR and indicative meaning defined for the reader/user.

Please see responses above.

L73 – just the latter half of the 20th Century or also in the early half? In my experience there is very little information from either.

We agree and have modified the text to reflect that.

L103-104 – More description of methods needed here. Since this manuscript is a data publication, it is useful to have all of the data processing information in the text alongside the database. How does IMCalc work? A short paragraph stating your approach and what this software does would help hugely in interpreting the updated PRSL values that presented in Section 4!

Please see responses above.

L115 – What type of transect? Topographic? How were these transects collected? From satellite DEM? Or from a ground survey? More detail needed.

We have added additional text clarifying that these transects were gathered from a ground survey.

> Mapping of the area was conducted using aerial photographs in conjunction with a series of four transects using altimeter measurements from the field (Brook et al., 1996).

L115-6 – Do you mean for this study they were derived from Google Earth or in the original study?

We have modified the text to clarify that we used the Google Earth to estimate the coordinates based on the original published map.

Coordinates of samples and terraces in the database were estimated in Google Earth from the original published maps.

L120 – State that this age is a U-series age – don't make me have to look up the dating method in the table every time!

This was an oversight by us. We have gone through the manuscript and added the type of chronological constraint when missing.

L121-123 – Is this difference in height because the authors have altered the height based upon re-interpretation of the indicative meaning? It isn't currently clear from the text so the authors should state what has caused the change in height and re-reference the original publication.

We have tried to clarify this confusion by expanding on the methodology, particularly we state reference survey elevations (and their respective datums) from the original publications as well as our calculated PRSL.

L153-155 – This outlining of the open-system behaviour needs to be mentioned earlier in Section 3 and its importance discussed for interpreting dates. It is good to mention it again here but the open-system problem and original U ratio needs to be introduced in Section 3 where dating is described. It is a primary problem in U-series dating.

Please see responses above.

L166 – The authors should state what the PRSL is that is concluded in the database. This would save the reader having to go and find it!

We have modified the text to clarify this misunderstanding.

The samples were taken from on top of the coral reef terrace within an elevation range of 8 - 15 m above mean sea level and have an open-system age of 120 $\pm$ 8 ka and a PRSL of between +14.5 m and + 21.5 m (MIS-5e, WALIS ID #s 189-192; Table 6). Groups B and C are taken from the face of the limestone cliffs 0 and 6 m above mean sea level. Group B samples come from the central to northern section of coast between Kalifi and Manda Island, and have an open-system age of 118 $\pm$ 14 ka and a claculated PRSL of between +9.5 m and + 14.5 m (MIS-5e to 5d, WALIS ID#s 193-198; Table 6). Finally, Group C has an open-system age of 100 $\pm$ 8 ka and a claculated PRSL of between +8.5 m and +12.5 m (MIS-5c to 5d, WALIS ID#s 199-201) and is located along the same section of coast as Group A (Schimoni to Kalifi).

Section 4.3 – Are there no elevation estimates or dates in Tanzania? If not I think this should be stated.

The individual subsections of Section 4.3 Tanzania introduce the different PRSL estimates in country.

L193 - "We extract PRSL..." - how do the authors extract this PRSL? What are the geomorphic features that are used to calculate this sea level? Again, I know this is partly in the database but it needs explaining and the sea-level markers describing in the text for completeness. Often I have to take the authors' word for a lot of things at the moment.

We have modified the text to better clarify the relationship of the PRSL to the geomorphic observations.

Kourampas et al. (2015) provide a descriptive transect of the Jambiani marine terrace from which we calculate a PRSL of +11 $\pm$ 5.1 m for MIS 5 (WALIS ID# 212).

L200 – How is this value calculated? More details needed.

We have modified the text to reflect that this PRSL was derived from a coral reef terrace.

> From this the coral reef terrace we calculate a PRSL of +7.9 ±1.1 m for (WALIS ID# 724).

L208 – "See below" – where? Section cross-reference needed.

We have added Section cross-reference here

L212 – Please add in the elevation estimates that are in the spreadsheet (5.5 +/- 1.37 m I think)

This is a terrestrial limiting point and therefore there is no PRSL estimate. We have added this explanation to the manuscript to prevent this confusion.

> There is no PRSL for this formation as this is only a terrestrial limiting point.

L220 – This is the same WALIS ID# as reported in the previous section (L213) for sample AR-06-003-001. Is this correct!? I can't check because there is no field for original sample number in the database – maybe this would be a useful addition?

Thank you for catching this. The WALIS ID# has been updated to reflect the correct ID, 184

L242 – How does the elevation determined by Stephenson et al (2019) translate to the new PRSL? What has been changed in the current publication? It looks like the authors are using a RWL of -1.44 m according to the database, where does this value come from? These details need explaining systematically for every site. Additionally, the database says that the PRSL = 10.74 +/- 1.36 m, but the manuscript says 10.3 +/- 1.6 m. Is this a mistake?

This RWL value comes from the upper and lower limits derived from IMCalc, we have used this system to recalculate the PRSL. As for the difference between the database and manuscript, thank you for catching this. This is a mistake and we have corrected the value in the manuscript.

L243 – this is the value reported by the original authors, but what is the value that has been determined in the present work? I think 8.22 ± 1.38 m according to the database. Please be consistent in reporting these data in the companion paper because it is very difficult to understand what the elevation estimates are referring to.

We have added a following sentence with the appropriate PRSL value.

> Moving south along the eastern shoreline, near Baie des Dunes from Battistini et al. (1976), a coral reef terrace elevation from Cap Miné is recorded at +6.8 ± 1.2 m above MLWS with an age between 125.5 ± 1.8 and 136.9 ± 2.3 ka (ST18-003-001 and ST18-004-001, WALIS ID# 159, Table 6). We calculate a PRSL of 8.2 ± 1.4 m from Cap Miné.

L244 and onwards – The ages quoted here from Stephenson et al (2019) are open- system ages which should be noted! The authors also report conventional ages. Please check reporting of all other studies for whether they are using open-system or conventional U-series methods and highlight this in the text and in the database.

As referenced above, we have gone through the manuscript and noted when open system ages are used and when they are not.

L251 – Again what explains the difference between the original authors' height estimates and the PRSL estimate? I presume the consistent 1 m difference in the central estimate is due to the difference between MLWS used by Stephenson et al (2019) and some other datum, but in the database the RWL is stated to be -1.45 m, not -1.0 m. . .? Again PRSL in the database is 4.13 +/- 1.4 m, but in the text is 3.8 +/- 1.56 m. Why? I am confused! What accounts for the different (and variable) uncertainties between this work and that of Stephenson et al (2019)? is it just the extra uncertainty in IR? What creates the uncertainty in IR? What is the merit in reporting an updated PRSL to greater precision (3 sf) than the primary authors (2 sf)?

Thank you for catching this. We have updated the manuscript with the correct PRSL from the database. The inclusion of 3 significant figures is a mistake and has been corrected. As for the use of a difference in sf between the PRSL and the primary authors, this is a byproduct of the database itself and how it handles inputs for elevation as floats. We have therefore not changed these values to reflect a standardized comparison within the whole database.

L261 – is this the value given by the original author or in the database? In the database it seems to be 3.28 +/- 1.68 m? How is this value arrived at and why is it not written in the text while it is in other sections?

This was the original elevation provided by Battisitini, we have added the newly calculated PRSL from the database to clarify this.

> The emerged reef, with large in-situ corals in growth position, was described at 1 - 2 m above MSL with an age of 85 $\pm$ 10 ka (WALIS ID# 949, BA76-001-001). We have calculated a PRSL of +3.3 $\pm$ 1.7 m.

L278 – I am a bit confused here, because I thought Dutton et al (2015) used MLWS specifically because that gives them the best estimate of PRSL? I understand that they also state that their corals can grow at up to 2 m below MLWS, but they deliberately pick MLWS because many corals grow up to this height on the reef flat. It is fine to add in this extra -1.0 m and the IR estimate associated with this value, but it needs to be explained! Is this range chosen because Dutton et al (2015) quote it, or is it chosen because this value is a standard value used for all of these types of data when calculating the updated PRSL? E.g. Stephenson et al (2019) also use MLWS for reef- flat corals but the RWL used for those data is about -1.44 m (in the spreadsheet at least, it is -1.0 m in the text – see above) Why? I can't marry these differences up with the RWL and IR quoted in Table 1. Is it because of tides/weather or something else? These questions apply to all data – I am just picking up on it here because these are papers with which I am familiar.

The difference between the RWL in the Dutton et al. (2015) dataset and Stephenson et al. (2019) are different because the tidal range of the two localities are different and therefore the indicative range of coral reef terraces are different (upper limit of the IR is mean lower low water). This difference is furthered because Dutton et al. (2015) provide modern analogue data where as the PRSL data from Stephenson et al. (2019) is derived using IMCalc. As responded above, we have added a subseciton for how PRSL elevations are derived in the Methods section.

L300 – what is the chronological limit?

This was a misleading sentence and has been reworded to make it more clear that this is the chronological limit from the proceeding sentence.

L301 – More information is needed, it is not clear how this 8 m estimate translates to the PRSL estimates that the authors report here.

Please see responses above.

L345 – add U to 234/238 ratio. This section talks about U ratios but this wasn't ad- dressed in Section 3. please add discussion of this important issue to Section 3.

Please see responses above.

L351 – is this a screening process established by the referenced authors, or in this contribution? It is not clear from the text. Is it based upon XRD or upon original U ratios?

We have added clarification that this was the original publication's screening process.

L387 – how do these best judgements work? Where these judgements are applied they should be written down and thoroughly described in the text as well as in the database if this report is to be a useful and more verbose description of the methods that will accompany the database.

We have moved this subsection up to the methods section and have looked to clarify the elevation.

5 Comments on Database

Why are some description fields empty? What is the recalculated U-series age? Explanation of this value is essential! I presume this is recalculated from the U/Th concentrations reported by the authors? Please state in the manuscript text what this recalculated age is, it is not mentioned currently I don't think. It is also essential to report where original publications quote conventional ages, open-system ages and where they report both open-system ages and conventional ages. I think the README could be expanded so that users can better understand the various columns. E.e. the sheet "U-series (Corals)" extends from column A to column DJ, but the README tab has only a sentence of information.

This comment was also brought up by Referee #1. We have not recalculated any ages, the only recalculated ages we record in the database are from Chutcharavan and Dutton (2020). We have also added within the manuscript where open-system ages are used and where closed-system ages are used.

6 Minor Points

We have followed the Referee's recommendations and modified the manuscript accordingly.

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