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Interactive comment on "Last Interglacial sea-level proxies in East Africa and the Western Indian Ocean" *by* Patrick Boyden et al.

Anonymous Referee #2

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Boyden et al. present a compilation of published last interglacial sea-level markers as a component of the larger WALIS project. I have a high degree of admiration for this project and it should prove an extremely useful resource for sea-level scientists and those in other fields in future. Boyden et al. do a good job of mining the literature for references to sea-level markers in a region in which sea-level markers are under-documented. My main concern is that the methods and data are insufficiently and inconsistently documented in the manuscript, leading the reader having to work out how values were calculated and interpret justification. This criticism notwithstanding, the database is very thorough and I am very supportive of this kind of work. It falls clearly within the remit of the journal, making for a very useful contribution for researchers across the Earth Sciences.

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Discussion paper

Major Comments

One of the authors' stated goals is to standardise reporting of sea-level markers so that they are comparable. In practice, this approach means categorising 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.

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.

A more detailed, general methodological description and explanation of general diffi-

culties/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 behaviour. 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.

Below are general comments that I wrote as I read the manuscript and explored the database, which are followed by detailed line-by-line comments in the text.

General 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 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 behaviour (e.g. due to an original U ratio that differs from that expected for sea water).

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"?

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.

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! 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?

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.

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.

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!

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 competeness.

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.

Detailed Comments

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.

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.

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Discussion paper

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.

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.

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!

Section 4.1.1 – Label Sanaag on map (Figure 2).

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

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

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!

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.

L123 – maximum age – why maximum? Is this stated because ages generally get older with alteration in open-system conditions? It would be nice to have this point clarified. If it is due to this open-system issue then it would be worth talking about why these ages are maxima in Section 3.

Section 4.1.2 – Label Banaadir on map (Figure 2).

L135 – Again, do the authors mean, “we calculate the PRSL to be...”? I think the active voice should be used for the parts where the authors have altered the PRSL since it clearly demarcates what is their work and what is in earlier publications. Where PRSL values have been calculated or updated, I think it is important to explain why they have changed. It is not clear why the authors think that “4 m” is not a correct value of PRSL from the text. The reader needs to know why the value presented here of 3.8 /- 3.9 m is better and how the uncertainty was calculated. This issue is addressed in the database, but I think the point of this companion publication should be to make these important methodological points crystal clear and interpretation of the indicative range and relative water level justified. Is it useful to report PRSL to a greater degree of precision than the initial authors’ height estimate? (1sf vs 2sf)?

L138-139 – How is it determined if there is no description? Is this determined by the earlier authors or by the current authors? What height measurement from the publication, RWL, IR etc. were used to calculate this value?

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.

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!

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

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.

Section 4.3.3 – Location of Dar Es Salaam needs to be on the map in figure 2.

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

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

Section 4.4.1 – Add location to map.

L210 - "is comprised of"

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

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? Please put Maputo on the map.

L229 – please put Antsiranana/Diego Suarez on the map.

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?

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.

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.

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)?

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?

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.

L300 – what is the chronological limit?

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

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

L348 - ditto

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?

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.

Comments on Database

This is an excellent resource and is extremely thorough. I have a few suggestions that may help improve the presentation.

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.

Minor points

L4 – comprised of/composed of

L14 – remove comma after spreadsheet

L31 – prevent many of these early studies from being included...

L46 – emergent, well developed. . .

L60-61 – Incorrect citation style, needs paraentheses.

L74 – the studies that we compiled

L91 – puzzingly high... - consider rephrasing.

L109 – However, only. Change to “Only”. References for these reports?

L147 – incorrect citation stayle, remove parentheses.

L179 – reference for recrystallisation.

L184 - “Missing citation”(!)

L194 - “datings” - change to ages or similar. Sentence generally needs rephrasing, missing subject pronoun.

L221 – indicate

L226 – Emergent

L251 – Irodo.

L257 – Emergent

L263 – Tidy up referencing so that year is not in double parentheses.

L282 – 7.4 +/- 0.2 m above MLWS.

L309 – disconformibly

L350 – Remove “to the far extent of the island group...”

L368 - “in the literature”

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