

Object: Revisions of the Manuscript “*Holocene and Common Era sea level changes in the Makassar Strait, Indonesia*” by Maren Bender et al.

27/12/2019

Dear Editor,

We would like to thank you for giving us the possibility to revise the MS in object. After careful consideration of the comments of two anonymous reviewers, we now resubmit a substantially reworked version of our previous manuscript. While our core results remain the same (i.e., 24 fossil microatolls dated in the Spermonde Archipelago, Indonesia), in comparison to the previous version we now extended the number of GIA models to which we compare our relative sea level observations. We believe that this gives us the possibility to discuss our data more in detail. Overall, we would like to draw your attention on six main points:

- 1) The MS has been largely rewritten, taking into account the reviewers suggestions and trying to better describe our data and their meaning.
- 2) In conjunction with the MS, we present a set of Supplementary Materials containing not only the data we surveyed and the models we used, but also the scripts we employed to make the key figures of our study. We hope this will be useful for other scientists who will work with datasets similar to ours.
- 3) We provide our GIA models for a large region encompassing South and Southeast Asia. We believe that this will be of interest for scientists working on Holocene sea levels in this region: to our knowledge, the only other models readily available for this area are the five we recently published here: <https://www.sciencedirect.com/science/article/pii/S2352340919309552#tbl1>. This new dataset enlarges the previous one tenfold (54 models iterations in total).
- 4) Besides the GIA models, we share everything we measured (e.g., water levels, elevations, radiocarbon ages) in the Spermonde Archipelago.
- 5) One colleague, Julia Illigner, helped us with a better definition of the survey errors and on the analysis of water level data against the Makassar tide gauge. For this reason, she was added to the co-authors.
- 6) The new MS contains four tables (one more than the previous version) and 12 figures (four more than the previous version). Overall, we revised almost all our figures to improve clarity. The new figures were produced to better illustrate our points and answer to the reviewer comments.

We would like to thank the constructive criticism by the two reviewers: their suggestions pushed us to be more critical towards our data and our approaches. We believe that this resulted in a much stronger paper and in a wealth of open-access data (e.g., our GIA models and water levels) that will surely benefit the sea-level community working in the broader S and SE Asia region.

We are confident that this round of reviews produced a better MS than the one originally submitted. Please find hereafter detailed answers to the reviewers, pointing to where, in the MS, we took their suggestions into account. Reviewer’s text is highlighted in gray, our answers are in plain text below. Central reviewer comments and our specific replies are in bold.

Best Regards, Maren Bender and the MS co-authors.

## Reviewer 1 (R1)

This paper develops 24 new sea-level index points using radiocarbon-dated coral microatolls from South East Sulawesi in Indonesia. The dates span the second half of the Holocene (past 6 ka) and capture a time period where Earth-ice models predict a highstand in sea level. To be suitable for publication in *Climate of the Past* (or elsewhere) I believe that **a wholesale re-evaluation of the manuscript and underlying data are necessary.**

We thank R1 for the time they dedicated to the revision of the MS. As the reviewer suggested, **we did a wholesale re-evaluation of the MS.** We rewrote several sections, also considering new data and new lines of discussion. We added new figures and revised the previous ones for clarity. While we revised slightly the error bars on our data, the underlying data is the same as the previous version and we stand by them. Nevertheless, we added 49 new GIA models to the previous 5 runs. This allowed us to better discuss our RSL data in light of other processes. All our data and models are attached to the MS in the form of Supplementary Materials. We make also available the scripts we used to make the main plots of our MS.

(1) What is the purpose of this study? **Unfortunately, this paper is largely a description of some new data.** The reader is not provided with a specific and compelling reason for why the work was undertaken in the first place, or why the study sites and region are important. Furthermore, there is no explicit take home message about the wider implications of the results. As such, the current manuscript is only of local interest to others studying coral microatolls and sea level near Makassar, Indonesia. In the introduction the authors should provide an explicit motivation for their work. Please explain **(1) why is it necessary to document the height and timing of a Holocene sea level highstand** given that it has been done in so many other places already? (2) What is the significance of SE Sulawesi? What can be learned by reconstructing relative sea level here that wasn't already known from existing studies? The abstract (lines 38-40) and introduction (lines 55-61) frame this study around the threat of future sealevel rise. **However, the paper makes no effort to use the results in this context (see conclusions for example).** Future sea-level rise is a convenient angle for making the topic of a paper appear widely relevant, but unless the paper leverages paleo results to produce better predictions this motivation should be removed.

To answer this comment, we would like to remark that both this version and the previous one contain **not only RSL data, but also GIA models at a very broad regional scale (beyond our study area).** We hope that the restructuring of the MS gives now more insights on why we embarked in this study. Departures between observations and GIA models at Holocene time scales are often used to gauge long-term vertical crustal movements due to, e.g. tectonics or subsidence, that need to be taken into account when extrapolating future sea level rise scenarios. **This was the reason for the hype on future sea level studies, which was removed from the introduction in this version.** While we removed the part of the introduction explicitly mentioning future sea level rise, we remark that, in general, the main purpose of studying paleo sea level changes is to leverage better future sea level predictions at either global or local scale. Therefore, we maintain that studies, such as this one, reporting accurately measured and interpreted data coupled with state-of-the-art models are, indeed, widely relevant. To answer the criticism of the reviewer more directly, **in the Introduction (paragraph 2) we now explain why it is important to understand the Holocene high stand at different locations.** The new section "Paleo to

modern RSL changes” shows why the data and models we present in this paper **are important at a very broad geographic scale.**

(2) Mechanisms of Subsidence (section 5.4). One of the five sites (Barung Lompo) records relative sea level that is anomalously low (Figure 3E). The authors propose that the reason for this trend is subsidence and go onto to propose that loading by approximately 4500 people living there, building concrete docks, and extracting water from wells is the driver. **This explanation seems unfeasible to me. The amount of subsidence is large** (order 1m per figures 3 and 7 and line 383), must be very fast (**order of 1cm/year assuming that the development occurred in the last 100 years**), and it doesn't seem unusual to me that this level of development would occur on a coral island in which case surely there would be lots of islands in many tropical places showing rapid subsidence at locations all around the world. Furthermore, the subsidence appears to be restricted spatially to just the island itself and doesn't extend onto the adjacent reef flat (line 398). Rather than offering an unsubstantiated idea, please **could the authors provide some evidence in the form of (for example) loading calculations or discussion of instrumented examples from other inhabited coral islands that are subsiding.** Figure 7 is not necessary. Its very easy to see from figure 3 that the difference is about 1m. Why doesn't the peak in the smooth curve line up with the peak in the histogram?

This line of evidence is now discussed in the section entitled “Mismatch of the record in Barrang Lompo island”. Here, as also suggested by R2, **we adopt a very cautious tone** for which concerns our interpretation of the mechanisms of local subsidence. The comment by R1 pushed us to do a bit more research on the population of Barrang Lompo. We found that the island is populated since at least 1720. This would translate into an **anthropogenic subsidence rate of 3-11 mm/yr** (depending on the assumptions on timing of population and uncertainties on the RSL observation differences). This subsidence rate is surely large, but it is **one order of magnitude smaller than anthropogenic-induced subsidence in coastal megacities in SE Asia** (up to tens cm/year). For which concerns investigating other lines of evidence, we tried to understand if InSAR data could be used to detect subsidence in Barrang Lompo, but unfortunately the island extent is too small to apply this technique. We did not find studies dealing with instrumental records of differential RSL changes at small inhabited coral islands, but the mechanism for subsidence proposed here is also reported by Church et al., 2006 (Global and Planetary Change), who state:

*“A thorough analysis of survey data at Funafuti (Kilonsky, personal communication) shows that the land adjacent to the UHSLC gauge is sinking relative to the land adjacent to the NTC tide-gauge benchmark (about 2.5 km away) by  $0.6 \text{ mm yr}^{-1}$ . This tilting may be caused by tectonic movement or (most probably) local subsidence (for example, due to groundwater withdrawal) and demonstrates that even on a single island, the relative sea-level trend may differ by as much as  $0.6 \text{ mm yr}^{-1}$ . In addition, the UHSLC gauge is sinking on its foundations by an additional  $0.6 \text{ mm yr}^{-1}$ , giving a total sinking rate for the UHSLC gauge of  $1.2 \text{ mm yr}^{-1}$ .”*

This reference is now discussed in the paper. In partial support to our hypothesis, **we also report some observations of intense coastal erosion reported for Barrang Lompo**, which might be related to exacerbated relative sea level rise rates. We close the newly designed section on Barrang Lompo by **suggesting strategies to test our hypothesis.** We hope that our work on this part provides a sufficiently balanced view on the data and on our interpretations.

(3) Common Era sea level (section 5.5). The authors have 8 microatolls that formed in the last 400 years and these form the basis for this section of the discussion. I see no purpose for this discussion and was unable to see its relevance to the paper or wider discussions about Common Era sea level. The database used by Kopp (2016) is not an exhaustive list of all sea level index points, it states clearly that the data are limited to detailed records. **There are likely 100s of sea-level index points that were NOT included in Kopp.** The authors claim that their new sea-level index points are the first from South East Asia for the Common Era because there is no data in Kopp (2016) from this region. This is simply not true and ignorance of the literature is an inadequate caveat. As one simple and widely known example see Horton et al. (2005; <https://journals.sagepub.com/doi/10.1191/0959683605hl891rp>). I expect the World Atlas of Holocene Sea-Level Curves would probably also provide some examples of Common Era sea-level index points from South East Asia.

**Why is the Common Era even discussed?** What is the importance of this time period (as distinct from the late Holocene which this paper focuses on), particularly given that the highstand occurs earlier? Figure 8C shows that the new index points have uncertainties that are too large to be useful in inferring much about Common Era sea level.

The section to which this comment refers to is now entitled “Common Era microatolls”. There was a long discussion, between the authors, on whether to toss this entire section. We decided to keep this in because, as it is true that there are several reports of Common Era sea level indicators in SE Asia, **but only one data point is reported for the time frame covered by our microatolls.** While we agree with the reviewer that the error bars are too large to gather anything significant on the eustatic sea level, we now use them to make a simple calculation of potential natural subsidence rates in our study area, which we then use in the following paragraphs. While we maintain that our calculations are only tentative, we believe that this section adds more context to our conclusions.

(4) Conflicting sea-level histories (section 5.1). This section of the paper (as written) seemed unnecessary to me. The authors discuss the Tija (1972) and De Klerk (1982) sea level reconstructions, but explain that a paper by Mann et al. (under review) contradicts the original interpretations of the two older papers. Why does this paper need to summarize the arguments made in Mann et al?

**To retain this section, the authors need to show the Tija (1972) and De Clerk (1982) data and provide a thorough explanation as to why those interpretations should be refuted.** There are major contradictions in this section. Line 316 states that “Makassar Strait being one of the most tsunamigenic regions in Indonesia”. Line 330 states that this study and others before it assumed that Makassar Strait was tectonically stable. How can the authors justify these two statements – they seem fundamentally opposed to one another. Is tectonics on or off the table for interpreting the new relative sea level curves?

I think that **figure 3 shows the new sea level histories to be conflicting within the study area.** The authors should focus on this rather than Mann’s (under review) of old data. Although Barrang Lompo is identified as an anomalous site, there are others that don’t match up particularly well with one another. For example, Bone Bafang (Figure 3F) seems to sit systematically lower than Sanrobengi (Figure 3B) by about 25-30 cm. Why is this the case? Its hard to evaluate the significance of this difference without knowing tidal range (not provided anywhere in the paper). Why is the relative sea level fall at Panambungan (Figure 3G) at 6 to 5 ka BP not seen anywhere else? None of these conflicting sea-level histories are discussed (and much less explained) by the paper. These discrepancies seem very large

given how close the sites are to one another (within about 100 km) and until this variability is explained the authors cannot justify their later interpretations of regional trends in relation to GIA models or Common Era trends for example.

We decided to retain this section. **As suggested by the reviewer, we expand on the descriptions by Tjia and De Klerk** (both in the text and Table 4 and Figure 7). We expand on our arguments for rejecting these data points, and we provide possible explanations for these higher deposits. While we still mention tsunamis, we clarify that the most active **tsunamigenic region is much further North**. We also point out that the fact that a region might get a tsunami wave does not preclude its tectonic stability: the Maldives are overall tectonically stable, but they were hit by the 2004 boxing day tsunami. Yet, we are very cautious in mentioning tsunamis in this region.

For which concerns the potentially conflicting sea level data within our islands, we now provide a new layout for Figure 3, which shows more clearly the RSL data in the region. We think it is clear that all our microatolls, except those from Barrang Lompo, **provide a consistent sea level history within their error bars**. It is true that there our data might indicate some high-to-low swings of few decimeters, but some of them are indeed present in our RSL models. The overall consistency of our record is now also shown in the newly produced Figure 9, that indicated that **there are only two clusters in our dataset: that of Barrang Lompo and that from all other islands**. The observation of the reviewer based on the differences between single points should be avoided, as every SL index point should be considered within their standard deviation.

(5) Validation of GIA models (section 5.2) This section of the paper offers minimal insight. The new reconstructions are compared to some GIA models. Unsurprisingly, there is a spread of highstand predictions (height and timing) among the different GIA models and therefore the agreement to the new data also varies among models. The authors proceed to identify models that fit the data better and worse. **What should a reader take away from this section?** Should we discard ICE5G because its fit to data in South East Sulawesi is worse than ANICE?

**Why does the fit in this region offer more insight than the fit in other regions** (particularly those with much bigger and longer datasets such as the British Isles or South East Asia, where databases on index points have long been compared to GIA predictions). Line 344 states “The better match of ANICE to our data has a meaning for which concerns ice melting patterns. In fact, the lower highstand predicted from ANICE stems from a very different behavior of the Antarctic Ice Sheet component”. Please could the authors provide the meaning that they refer to and explain what behavior specifically in Antarctica is different between the two ice models and why this has the effect in Indonesia. The final sentence of this paragraph is wholly unsatisfactory at explaining the difference in behavior. This is an example of the paper failing to offer insight beyond their study area and specific topic.

Taking into account the comment of Rev.1, we heavily edited this section, which is now titled “Comparison with GIA models”. First of all, **we now provide a much larger set of models** that are compared to our data. To our knowledge, **this is one of the largest GIA model ensembles published for S and SE Asia, and we provide NetCDF files** that can be used to compare observation to modeled RSL in a broad region, together with the scripts needed to reproduce the figures we present in this paper also for other areas.

We also modified the text in order to **make it clear that we are not favoring one model over the other**. Also using some considerations on tectonics and subsidence from different hypotheses, we point out that different models fitting in our region have different meanings also for current sea level studies. As we moved away from the search for a “single fit”, we also deleted the discussion related to the ice sheet patterns of ANICE vs ICE5g vs ICE6g that we agree were out-of-scope.

(6) Assumptions and approaches to reconstructing sea level I have several queries about the specifics of how relative sea level was reconstructed. (a) In the methods section, I would like to see plots of the water logger data and the correlations to one another and the tide gauge in Makassar.

The water level logger data are now reported in SM1. We cannot report the tide gauge data of Makassar as these data are, unfortunately, not in the public domain. Data can be requested to Badan Informasi Geospasial (BIG), Makassar. Unfortunately, we have no control over their public availability.

(b) What is the tidal range at these sites? This key piece of information is missing.

Unfortunately, we would need 19 years of water level data to gather tidal ranges at the sites studied. This is why the information is missing in the paper. Nevertheless, in the “Regional Setting” section, we inserted what we can derive from the Makassar Tide gauge: *“In the Spermonde Archipelago, the tidal cycle is mixed semi-diurnal with a maximum tidal range of 1.5 m (data from Badan Informasi Geospasial, Cibinong/Indonesia).”*. This is the same information contained in Mann et al., 2016.

(c) In figure 4, there are very large differences between sites for the height of living corals. Presumably, this is caused by similarly large differences in tidal range, **but since tidal ranges for the sites are not presented anywhere in the paper it is impossible to confirm**. Please could the authors provide this information and a supporting explanation as to why tidal range varies by so much over distances of less than about 100 km (Figure 1D; there is nothing in the figures to indicate that tidal range should vary dramatically among the sites). Alternatively, tidal range doesn’t vary much between sites, in which case the authors need to explain why modern corals have different relationships to tides over the same small distances.

Unfortunately, as explained above, **we cannot present the tidal ranges for the sites as this would require very long-term water level measurements**. What we do is to point out the difference we find in the living microatolls, and advise against using a single tidal range to derive the indicative range also as such small spatial scale. The comment of the reviewer, though, pushed us to investigate more this onshore-offshore gradient, and we found out that a similar pattern is **discernible in our water level data**. We plotted Figure 4b to show it, but caution must be adopted in comparing directly Figure 4a and 4b: the water levels of 4a represent, possibly, MLLW/LAT ranges over 50-100 years (living microatoll time), while 4b presents only a snapshot of a couple of days.

Figure 4 must also show the height of other important datums (particularly MLW, MLLW, and LAT) that **are used as part of the indicative range for dated corals**. There must be explicit statements if these datums have the same or different relationships to mean tide level (and one another) at each site. The reader needs to see the data that supports the authors assertion that coral microatolls live between MLW and LAT (Line 84).

We remark that **we do not use tidal ranges to calculate the indicative range for dated corals**. In the new version of the paper, we tried to state this more clearly to avoid this confusion. The reason why we

used HLC to calculate the indicative range is, in fact, that **tidal datums are not available for each site within our study area**. We only have few days of water level data, which is now attached as SM1.

(d) Tables 1 and 2 must also show the original radiocarbon results (radiocarbon age, error, lab ID,  $\delta^{13}C$  etc) because calibration curves and marine reservoir corrections change, but the original results will not. Adding this information will make the paper more useful in the long term.

We totally agree. In SM3, we share all the data received from the radiocarbon analysis laboratory.

(e) The equation used to reconstruct relative sea level (section 3, page 6) includes a term (“ $E_r$ ”) for the amount of material eroded from the top of a coral microatoll. The authors assume that all coral microatolls had a pre-erosion thickness of  $0.48 \pm 0.19$  m (line 175) based on a survey of modern corals in Mann et al. (2016). **This assumption seems tenuous** because the structure of a microatoll depends on the pattern of relative sea level change. For example, if sea level is rising then microatolls grow vertically (presumably getting thicker rather than wider). In contrast, if sea level is stable they grow laterally (presumably getting wider, but not thicker). Can the authors justify why the thickness of modern microatolls surveyed when sea level is rising rapidly (line 163) would be the same as fossil microatolls that lived when sea level was rising more slowly, stable or falling across the mid-Holocene highstand? The value of term  $E_r$  should be presented in Table 2.

The value of  $E_r$  is now reported in Table 2 and clearly marked in Figure 3 (grey error bars). **Modern microatolls thickness is the only proxy we have to account for how much material has been eroded. We put a caveat in our methods section** to reflect this very relevant consideration offered by the reviewer. Here is the sentence we inserted:

*We remark that this calculation does not take into account the fact that modern microatolls are thicker rather than wider because of the current rapidly rising sea level. In contrast, under Late Holocene falling or stable sea level changes, they were presumably getting wider, but not thicker. Hence, in our calculations, the added  $E_r$  might be overestimated, as it is based on modern microatoll proxies.*

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## Reviewer 2 (R2)

This paper presents interesting new mid-late Holocene coral microatoll data from the Makassar Strait and produces a new regional RSL curve which is compared to GIA models. Although there is a good quantity of new data and the presentation of this data is generally good **the broader implications for understanding past and future eustasy, or regional RSL are not well made**. Each section of the discussion either falls short of making important new insights or reiterates statements from other papers (e.g. Mann et al, in review). As it stands this paper does not provide convincing reasons for the study taking place other than to document local RSL during the mid-late Holocene.

We would like to thank R2 for the time they dedicated to assess our MS. We re-wrote large sections of our MS, including the discussions and conclusions, in order to **make more evident what are the broader implications of our study**. We hope that this answers their comment. We also tried to be specifically clear on what was already published in Mann et al. 2016/2019 and this study. Overall, we present more data and more models that we surveyed following the works of Mann et al.

Discussion – section 5.1. Absence of evidence for high RSL (over 1 m above MSL) **does not categorically rule out the fact that RSL could have been higher in the Holocene**, particularly at the start of the high stand when you have fewer index points. Your data is not continuous, and in all cases has clustered SL index points from individual islands with millennial-scale time gaps. It is highly unlikely that the earlier (De Clerk/Tija) data is in situ but you cannot categorically rule out a slightly higher high stand earlier in the Holocene. You should therefore **be slightly more cautious in describing your data**. You also state that the re-analysis of the earlier work was largely undertaken in Mann et al. (in review) and therefore this discussion surely just repeats this analysis? **How is section 5.1 in this paper different to what is discussed in Mann et al., in review?** As that paper is in review I am not able to look at it for information.

We tried to be more cautious in describing our data. In our results section we only provide an overview of the data and model results we have, with no interpretations. We point out, though, that we now have a very widespread amount of data compared to Mann et al., 2016 (who had two islands only), and the bulk of data points to the same conclusion: we find no evidence of higher RSL than our microatolls in the Spermonde Archipelago. Our models indicate that the highstand might pre-date our RSL index points, but we find no evidence for it.

The paper Mann et al, in review, is now published (2019). In this paper, we quote exactly what Mann et al., 2019 concluded and we use this as the starting point for further discussion. Basically, **the data presented in this paper were not yet available when Mann et al. compiled their review**. These new data give us **more confidence in rejecting Tija and De Klerk data** and in looking for alternative explanations.

Discussion – section 5.2. Can you be clearer about **what the Antarctic fluctuation during the Holocene is, that is causing the ANICE model to better fit your data?** Does this model fit better elsewhere in this region? What larger implications of the model-data fit can you make using this dataset? I feel as if the broader significance of this section is not well explained, but this data-model fit may not be region-wide, so it may have no significance at all. If it is not this raises more serious questions about your data.

Also following the comments of R1, **we now deleted this part**. We now show fits/misfits of our data with a larger set of GIA models under different tectonic uplift/stability/subsidence scenarios, and discuss what would change in terms of modern GIA rates. We believe that this gives a better frame to show how our data may be useful within a broader context.

Discussion – section 5.4 **Is there a chance that the elevation data is incorrect for Barrang Lombo** rather than there being subsidence on this island? If you are arguing that subsidence of 0.8 m has occurred since the mid-Holocene on this island only, is there any other geomorphological evidence of subsidence (lower elevation reef flat compared to other islands, or tilting of the reef flat surface?). Surely if water extraction and buildings are causing this subsidence it should be ongoing and therefore should be seen in the surface morphology of modern microatolls (or is the rate too small)? **You should make more comment about this as a theory**. Why is it significant that the modern microatolls are ‘a few hundred meters away from the island’? Where were the fossil microatolls that were sampled in relation to the island? Are you arguing that the modern microatolls are not affected by subsidence because they are located further from the centre of the island? If you are indeed suggesting subsidence you need to substantiate this with other evidence beyond that derived from the microatoll data.

We carefully re-checked our data following this comment, and this led to some changes in how we propagated errors. While we believe that the new error bars are now more “solid”, the overall dataset did not change much. Standing our measurement methods, **it is highly unlikely that there is an error in elevation data from Barrang Lompo**. Following this comment and that of R1 on the same section, we now made it more hypothetical, presenting the “human-induced” subsidence **more as a theory that needs testing**. One way to test it was suggested by R2: one might look at growth patterns of modern microatolls at different islands and see if there are differences. In re-writing the section in object, we tried to take into account all the questions raised by the reviewer on the previous version.

Discussion – section 5.5 **I don’t think it is sensible to compare single-dated microatolls to the Common Era SL curve from Kopp et al (2016)**. Two SLIPs don’t fit with the curve and there is no explanation for why this might be. Why are these data not corrected for GIA? Given the large error terms on the SLIPs and the lack of explanation for why some data fits and some does not, is this kind of data suitable for assessing Common Era RSL in this region? I’m not really clear where this discussion is going or its value to the manuscript.

**We deleted from the MS the comparison with Kopp et al.** Instead, we now use the microatoll data to gauge whether it is possible to calculate rates of natural subsidence, that are then used to discuss Late Holocene “stability” of the area. We refrain from any ESL consideration from these few scattered records, but we kept them in the paper as this particular time frame (the last 300-400 years) is almost not reported in SE Asia.

Tables 2 and 3 – be clear that Age and RSL uncertainties are +/- errors. As you plot the data from De Klerk (1982) and Tjia (1972) on Fig 6 it would be helpful to include the data for these index points in a table, probably table 3. Also please include the raw 14C data and lab codes for all dated index points so that they can be recalibrated in future if/when new calibration curves are developed.

We clarified that age and RSL uncertainties are +/- errors. We also included the data from De Klerk and Tjia in Table 4. The original lab analyses are available as SM3.

Fig 3 – why have present to the L of these plots? I would like to see present on the R of these plots. It would make sense to me to have the X axis scale the same for each one, even though it will squeeze the data points on some of the graphs.

Figure 3 was re-drawn to better show the data points. We feel that present to the Left or Right is a preference rather than a convention, and for the Holocene we have the feeling that many authors adopt the present to the left (see Khan et al., 2015 Quaternary Science Reviews, summarizing the Holocene sea level Atlas).

Fig 6 – marine limiting data points would normally have the horizontal bar at the top of the vertical distribution, not at the base. Your terrestrial limiting data point should be drawn with the horizontal line at the base of the vertical distribution. It would help if these data were included in table 3 so it is clear how you have plotted them.

We now have the same data plotted in Figure 3, and we changed marine and terrestrial limiting symbols to follow the convention highlighted by R2. All data plotted in figure 3 is now included in Table 2, 3, 4 and in the SM1 as spreadsheet.

Why is tidal range not stated for each island location? This would help in interpreting the modern HLC data. I am not clear why you have used a constant erosion variable where erosion has occurred, as this is likely to vary over time.

See answers to the same points raised by R1.