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Interactive comment on "Holocene and Common Era sea level changes in the Makassar Strait, Indonesia" *by* Maren Bender et al.

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Dear Editor, We would like to thank you for the opportunity to answer the Reviewers comments and suggestions. While we acknowledge that both Reviewers are critical on several aspects of our paper, we also remark that Reviewer 2 made constructive suggestions and offered helpful ideas and recommendations. Reviewer 1 recommends that "a wholesale re-evaluation of the MS and underlying data are necessary". We think that re-writing some of the sections in light of the criticism raised by the two Reviewers and adding some accessory data and models will produce a stronger MS, that we hope will be acceptable in Climate of the Past. Hereafter, we try to summarize all concerns and ideas of both Reviewers, and we provide detailed comments to each of them. We hope you will give us the opportunity to revise the paper accordingly, as

we still believe that this could be a good contribution to Climate of the Past. If you choose to move forward asking us a revised version, we will send you the new MS with a detailed answer to each point raised by the Reviewers, including lines in the MS where the critical points are answered. Comments about how data are used in the paper Both Reviewers, in their opening statements, comment that this is a paper based on data. While Reviewer 2 comments the data characterizing the data as "interesting", "good quantity" and agrees that the presentation of data is "generally good", Reviewer 1 seems more negative about the fact that we present new data ("unfortunately, this paper is largely a description of some new data"). Both Reviewers agree in asking to explain better the rationale of this work, in order to give our data a larger context. In this paper, we present new data that was measured and dated as accurately as possible in an area of difficult access. A recent special issue in Quaternary Science Reviews reviewed hundreds of papers that reported, in total, more than 5500 data points. Each paper reported data exactly as we do in this MS. All together, these data are essential to validate GIA models at global scale. And validating GIA models in turn is important as GIA corrections are used, among other applications, to correct tide gauge data. We propose to restructure our introduction and rationale for the study to clarify this relationship between paleo sea level studies and current sea level, in order not to give the idea that our work is relevant only within the regional context. We can then take this point back in the conclusions to show what still needs to be done in SE Asia to reach a reasonable knowledge on Late Holocene RSL. In their comments, both Reviewers are asking for our raw data, specifically radiocarbon and tidal data. We commit to add all the original BETA Analytics reports on our radiocarbon ages and the water level data we collected in Indonesia. Tide Gauge data from Makassar tide gauge are private communication from BIG Indonesia, the National Geospatial Agency. The data have to be individually requested as per their rules, so we cannot put them as open access (The address to request the data is: Pusat Jaring Kontrol Geodesi dan Geodinamika, Bidang Jaring, Kontrol Gaya Berat dan Pasang Surut, Jl. Raya Jakarta-Bogor KM. 46, Cibinong 16911, Indonesia). We would like to remark, though, that these data were used only to refer our measurements to MSL and not to interpret the paleo RSL as Reviewer 1 seems to point out.

Rationale of the work Reviewer 1 asks to address several questions within the introduction. The first is “Why it is necessary to document the height and timing of a Holocene sea level highstand given that it has been done so in many places already?” We agree that we should have made this more explicit. The brief answer is that, as sea level is spatially variable due to a number of factors, studying paleo sea level changes at different geographic locations is very relevant to understand patterns and timing of land/sea level changes. The presence and magnitude of the Holocene highstand in tropical areas is the result of the combined effects of eustatic history and glacio-hydro-isostatic adjustment (GIA). Documenting the Holocene highstand (its timing and elevation) at different places serves to better constrain how GIA and eustatic forcings are intertwined in both space and time. Our study provides yet another constraint on these processes. We plan to expand our introduction to explain this point better. Then, Reviewer 1 proceeds to ask: “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?” We would like to remark that SE Sulawesi is not the area of interest of our study. Our Study area is located in SW Sulawesi. In general, southern Sulawesi is in the central part of Indonesia and thus is supposed to be a good region to study sea-level variability due to its central position within Indonesia. Furthermore, the study area we addressed is often reported as tectonically stable, while further north tsunamis and earthquakes are affecting the coast and hence there might be departures from eustasy due to tectonic activity. We propose to insert considerations on these matters in the Introduction, expanding on the relevance of our study area. Another matter raised by Reviewer 1 is that “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”. This matches a comment by Reviewer 2 asking to improve the description of the broader significance of our data. While our opening statement is about the importance of future sea level rise, we would like to

remark that we do not really frame our study around this topic. As briefly explained above, understanding late Holocene local sea level histories is indeed necessary to better analyze modern datasets. We can surely re-phrase the first paragraph of the introduction to better reflect the relevance of our study and its rationale. In the discussion, we propose to insert a new heading, “Implications on future sea level changes” to better explain what can be learned from our data in terms of future sea level rise.

Comments on Results section Other than asking to disclose our water logger data (which we will do, see above), Reviewer 1 asks to modify our Figure 4 to show also sea level datums together with the height of living microatolls. This is, according to the Reviewer, necessary as they are “used as part of the indicative range for dated corals”. We think that there is a bit of confusion here, and we apologize if this stems from our choice of wording. We will revise our methods section carefully to make sure that it is clear that we did not use the tidal ranges as part of the indicative meaning of our microatolls. We think that this misunderstanding may originate from the fact that, in the introduction, we summarize how microatolls are generally interpreted: using tidal datums. This is well established in literature, and we do not think we need to show (as Reviewer 1 asks) that this is true, as we provide several references for it. In our section 5.3 we discuss that using the height of living corals is a better option than using tidal datums because it allows to take into account small regional differences, such as the ones we propose for the strait of Makassar. Unfortunately, in this area there is only one tide gauge, in the city of Makassar, and the few days of water logging we have, do not allow to establish tidal datums rigorously. As Reviewer 2 also asks to plot tidal ranges in our MS, we can propose to enrich our results with a tidal model forced with offshore constraints and calculate tidal datums at our locations and see how they match with the height of living corals. As this would take a bit of additional work on our side (that we have the capabilities to do, but tidal models) and would require to model a few different sea level scenarios (e.g. higher sea levels during the highstand) we would like to ask the Editor if he thinks that adding this part is necessary. Another comment related to our data by Reviewer 1 is that we estimated the erosion thickness of some of our microatolls. Reviewer 2 also comments on this, saying that erosion rates might vary with time. We clarify that not all microatolls have been corrected, we will mark in the paper the ones that were (n=10). We also propose to mark them in the figures, so it will be immediately clear which microatolls might carry additional vertical uncertainties. We surely agree that using a single value (that was measured in the field by Mann et al., 2016) is a crude approach, but this is the only way we can take into account the erosion, that surely happened for some microatolls based on their morphology. Reviewer 1 suggests some interesting lines of discussion, that we propose to implement as caveats in our revised MS, expanding on them in the section where we discuss the erosion correction. Discussion – Abandoning conflicting sea level histories Both Reviewers express doubts about this section, also in light of the fact that Mann et al was in press at the time of the review. We shared a confidential copy with the Editor at the time of submission, and we are confident that the overlap with this MS is kept at a minimum. The paper is now published with this DOI https://doi.org/10.1016/j.quascirev.2019.07.007 and it is Open Access. This section is based on the comparison with older data in the same region, something that has been discussed previously by Mann et al., 2016 and Mann et al., 2019. In both papers, we have been cautious to reject these data, as we are well aware of the implications of doing this (i.e., eliminating a possibly high Holocene highstand from the Makassar Strait). Mann et al., 2019 write: Following the discussion about possible sources for RSL data inconsistencies in the SEAMIS database, site-specific discrepancies between [. . .] Tjia et al. (1972) (sub-region #5b) and de Klerk (1982) and Mann et al. (2016) (sub-region #6) must be resolved with additional high-accuracy RSL data before the existing datasets can be used to decipher regional driving processes of Holocene RSL change within SE Asia. Mann et al., 2016 already proposed that these data may represent storm deposits. Here we expand on this point showing wave heights in the region, opening also up to the fact that, despite the Spermonde Archipelago shelf is considered as tectonically stable, historical earthquakes have been recorded further north and waves may have propagated into the shelf. We checked if there is the possibility to model at least how one event would propagate

onto the shelf, but unfortunately, we miss realistic earthquake parameters to run our model. Again, we could propose to use a simple hydrodynamic model, forced with one historical storm, to show whether these high deposits can be explained. As we have no precise topographic data from the Tija and De Klerk studies we would have, though, to estimate a typical cross-shore profile (including bathymetry) and this might raise more questions than answers. Also on this matter, we defer to the Editor before attempting this modeling approach. Overall, we propose to expand the discussion in this section, also taking into account some comments on it by Reviewer 2 and making clear what is postulated by previous works and what is original here. Bottom-line, we feel that we have enough data from different islands to reject that the highstand was as high as 5-6 meters in this area. It is true that we are missing the highest peak (probably), but there it is very difficult to reconcile our data with a 5-meter highstand. Reviewer 2 seems to agree with this statement.

Discussion – Validation of GIA models Both Reviewers express their criticism on this section, asking in substance to expand our considerations on the underlying ice models. Reviewer 1 asks specifically "should we discard ICE5g because its fit to data in South East Sulawesi is worse than ANICE?" We would like to remark that nowhere in the paper we give the idea that one model should be discarded over another. We just note that, in our study area, one model matches data better than another does. This is clear from line 439 in our paper where we state "some iterations of ANICE seem to perform better" and we go on arguing that more ice sheet and earth models should be made available to "compare with RSL data in search for a better match". Given the Reviewer's comments, we decided to take on our own advice, also in order to expand this section as the both Reviewers seem to welcome. We now have 54 different ice-earth model iterations compared to the 8 we presented in the paper. We ran not only ANICE, but also ICE5G and ICE6G iterations with varying mantle viscosities. We share hereafter some preliminary plots to show the Reviewers that we are now in a better position to comment on the GIA points they raise. First of all, the figure below (Fig 1) shows the results of different earth viscosities associated with ANICE (red), ICE5g (blue) and

ICE6g (green). ANICE is still the one giving a lower highstand (left panel) and this is due to the ice history before the highstand itself (right panel). Using these new model runs, we first plan to use neighbouring areas from Mann et al., 2019 to gauge whether sea level indicators dating 10-12ka match better ANICE or ICE5g – ICE6g melting patters. Then, we will move on to compare our data to the model results. In general, we will maintain the notion that one single area cannot be used to say that ANICE performs better overall (we know well that it takes much more to choose a model over another for a given region). But it appears that our data (with the exclusion of Barrang Lompo, for which we discuss subsidence) fit better models with a low highstand (see below). Having these new models available is interesting, and grants some further discussion that we propose to add to the GIA section. For example, taking a snapshot at 5ka of the three models highlighted in the figure above (fig. 2) with full colors, we see that they show very different RSL histories in SE Asia, with ICE5G and ICE6G being essentially very similar and ANICE producing overall a very small highstand (see images below, respectively ANICE – ICE5G – ICE6G compared at 5ka and one mantle viscosity). We think that these new model outputs can be included in the paper, and producing a set of maps at regional scale such as those shown above (and at different times) (fig. 3) should help clarify. Overall, we propose to restructure the discussion of the GIA session giving a set of model maps that can be used by other workers to test whether the best fitting models in our area is compatible with other areas. Using tectonically stable areas in our database, we might also attempt to add to these maps points indicating when and how high was the highstand, taken from an update of Mann et al., 2019 (few data have been published since then).

Discussion – Local subsidence effects Both Reviewers are skeptical to the part of the discussion where we point to the fact that one heavily populated island might be subsiding due to local groundwater extraction and the weight of buildings. On this respect, Reviewer 1 seems more skeptical ("this explanation seems unfeasible to me"), while Reviewer 2 is more prone to consider it as a theory ("you should make more comments about this as a theory"). We remark that we left this as a hypothesis, as in the

discussion and in our conclusive point we use the conditional. In the revised version, we will advise further caution to interpret this result. We are currently trying to see if there is anything we can do to provide additional context, as suggested by Reviewer 2. One possibility we are exploring is to look at InSAR data with the help of a collaborator. We propose to report on this effort in the new version of the paper. We also plan to discuss further on the rates, comparing the subsidence of 0.8 ± 0.3 cm/year to other local subsidence rates in similar contexts. We are afraid, though, that there will not be many examples due to the lack of precise long-term surveying at small islands such as Barrang Lompo. Reviewer 1 is also questioning that Barrang Lompo data are really lower than the other areas, invoking "clustering" of our data. Reviewer 2 asks if it is possible that our data are wrong in Barrang Lompo. We would tend to exclude this latter possibility, as the survey methods we adopted are solid (levelling is among the most reliable surveying methods available). For which concerns data "clustering", we present hereafter a magnified version of our data (fig. 4) that might be the basis for further discussion. It appears obvious, to us, that the main clustering is the one we highlight in the paper, i.e. Barrang Lompo versus all the other islands. There is a second, minor discrepancy starting around 4700 BP between Panambungan/Bone Batang and Sanrobengi. This might be worth discussing, as these two islands are located in a different geographic setting, with Sanrobengi closer to shore (see Google Earth image below, fig 5.). Keeping in mind that these differences are, at best, 20 cm if we take into account error bars, it might be possible that these islands might have been subject to slightly different isostatic histories due to, for example, sediment loading or water loading of the shelf. The graph and brief discussion above also answers Reviewer 1 commenting that "new sea level histories to be conflicting within the study area". While Reviewer 1 comments that Bone Batang and Sanrobengi appear at odds, we remark that they seem perfectly overlapping within time and vertical error bars. We understand, though, that this comment originates from the way we chose to plot the data. We propose to insert, in the final version of the MS, a graph similar to the one above and discuss the inter-site discrepancies between entering the discussion of the

Barrang Lompo data.

Discussion – Common Era Both Reviewers point out that our discussion of the Common Era is not well constrained, and it appears not well related to the rest of the MS. Reviewer 1 is correct in saying that we did not investigate the existing data properly, also in light that, when we wrote this paper, we had the Mann et al. database in our hands. In the latest update (that contains data that were not published before the Mann et al., 2019 paper was out) we found out that there are 200 data points dating between 0 and 3000 BP. The co-authors are currently debating whether to delete completely or expand, as requested, the Common Era part of the discussion. Regardless of the outcome of the discussion, we will obviously re-write our discussion and focus it on SE Asia, explaining better the meaning of our data in the regional context. Minor points We plan to carefully consider and wherever possible accept the minor points raised by the Reviewers.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2019-63/cp-2019-63-AC1-supplement.pdf

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Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-63, 2019.

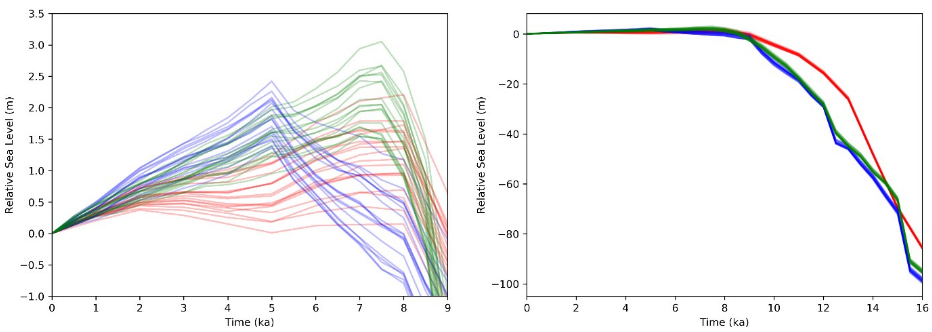

Fig. 1. Results of different earth viscosities. ANICE (red), ICE5g (blue, ICE6g (green). Right panel: ice history before the highstand and left panel: ANICE giving a low highstand.

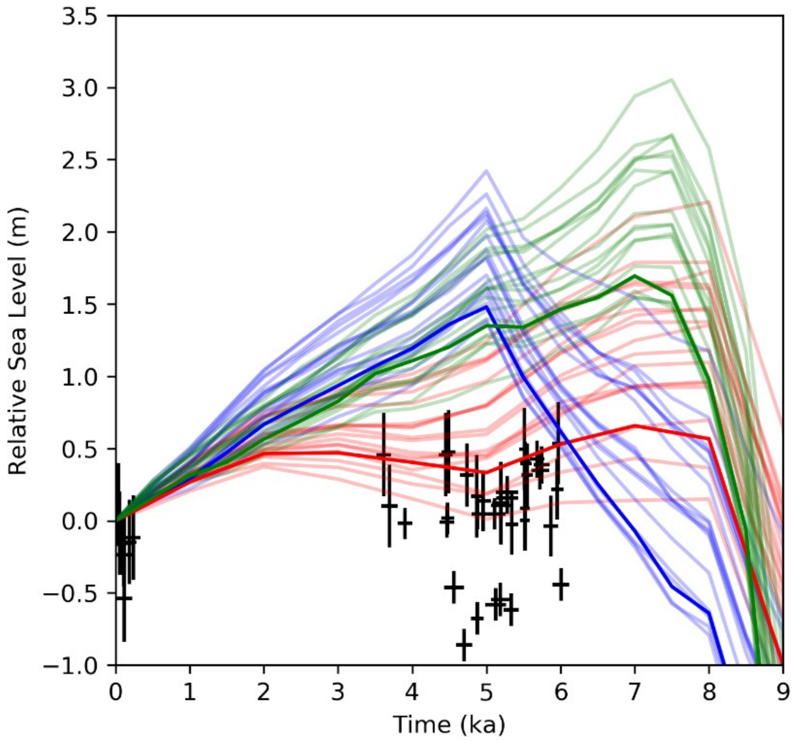

Fig. 2. The data fit better with a low highstand

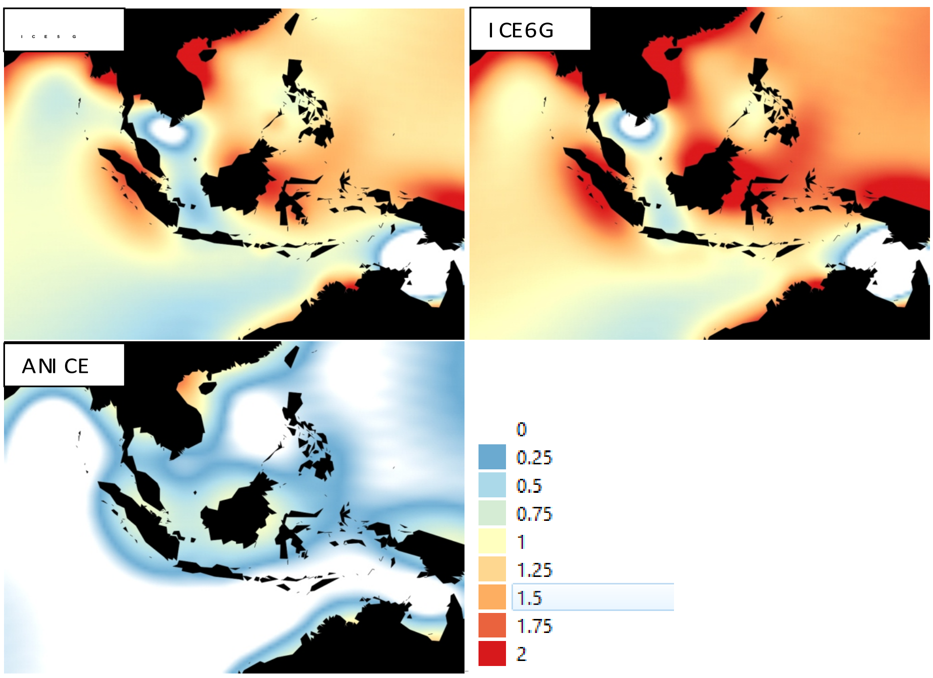

Fig. 3. Maps of different model outputs. Please find further explanation in the text

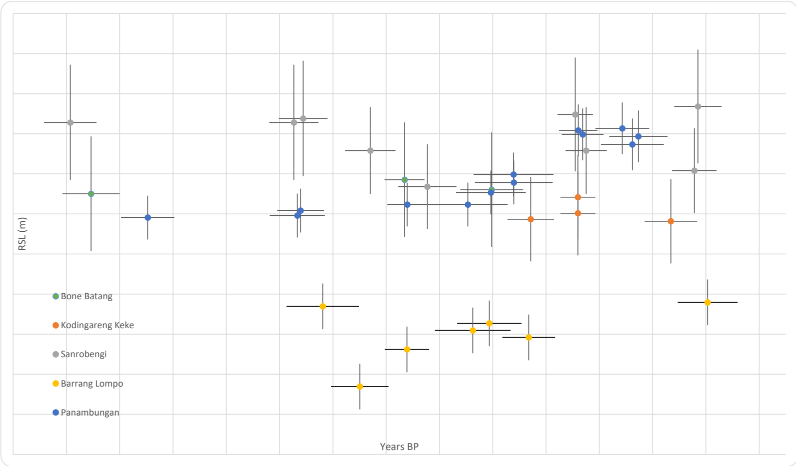

Fig. 4. Close up of our data. Barrang Lompo provides deeper positions than the other islands

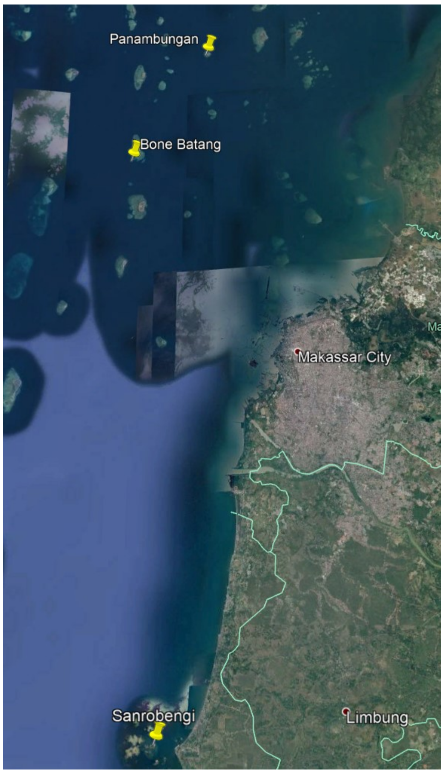

Fig. 5. Google maps picture showing the locations of Sanrobengi and Bone Batang