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

Anonymous Referee #1

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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.

(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 sea-level 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.

(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?

(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.

(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 through 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.

(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 that 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.

(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.

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

(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.

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).

(d) Tables 1 and 2 must also show the original radiocarbon results (radiocarbon age, error, lab ID, d13C 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.

(e) The equation used to reconstruct relative sea level (section 3, page 6) includes a term ("Er") 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 the same as fossil microatolls that lived when sea level was rising more slowly, stable of falling across the mid-Holocene highstand?

The value of term Er should be presented in Table 2.

(7) Minor Points In figure 3, time should run from left to right.

Line 72: My understanding is that the eustatic sea level curves in ice models such as ICE 5G show the fastest rates of melt/sea-level rise during the Holocene occurred earlier than 6-3 ka BP.

Line 91: Why are index points from the Maldives, India and Sri Lanka mentioned here? This data is not used anywhere in this paper.

The structure of the paper would be improved by presenting modern coral surveys before the fossil samples (in methods, results, discussion and conclusions).

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