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Interactive comment on "Last interglacial sea levels within the Gulf of Mexico and northwestern Caribbean Sea" *by* Alexander R. Simms

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*Reviewer Comment: The author has made a meta-analysis of 62 papers investigating RSL indicators in fossil beach-dune systems and coral reefs with LIG U-series or OSL ages around the Gulf of Mexico and the eastern Yucatan Peninsula. In general the scholarship is good and the manuscript reads well. However there are issues with consistency in determining valid estimates of RSL, methodological problems, and unstated assumptions that need addressing, as outlined below. For the LIG beach-dune systems rimming the Gulf of Mexico, he finds that few of the studies have quantitative estimates of RSL and attempts to piece together RSL indicators based on a comparison between the average elevation of modern beach-dune systems and the fossil systems (drawing from his own work in the area). For the NE Gulf he estimates that

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RSL was +1 to +5 m above present, and for the NW Gulf +2 to +2.75. The stated errors on these estimates however are large, and in some cases exceed the amplitude of the estimated highstand. In addition to this uncertainty, there are several unstated assumptions in this analysis which I think could be addressed. First, comparing the elevation on modern beachdune systems directly with their LIG counterparts ignores any subsequent erosion, both during the initial SL downdraw when these deposits first became inactive but were still composed of mobile sands, and later when they became part of the inshore during the 125 m lowstand. In active modern systems there is a balance between deposition and erosion, but once they became inactive, the dominant process would be erosion. Similarly the second assumption, that the elevation variability of modern beach-dune systems is representative of the LIG systems downplays the differences between transgressive vs regressive systems. For example, transgressive systems tend to be sediment starved compared to (forced) regressive systems where a slight fall in SL can expose large unlithified sediment sources and thus contribute to higher sediment flux.

**My response: I thank the reviewer for their careful reading of the manuscript and many insightful and helpful comments. Yes, I have made many assumptions in comparing the average elevation of the modern and LIG paleo barrier islands. I have added a paragraph acknowledging more explicitly these assumptions including those brought up by the reviewers. Concerning the transgressive versus regressive nature of the barrier islands, I tried to compare similar features to similar features. In general the modern aggradational and progradational barrier islands are wider than their transgressive counterparts (Anderson et al., 2014; Otvos and Carter, 2013). For example compare the width of the transgressive Follet's Island with the aggradational Mustang Island (Fig. 4). The Ingleside segments of the South Texas coast are wider with preserved beach ridges and thus likely represent regressive barrier islands. But so are their modern equivalents (see new Figure 4). Conversely, the lower elevation and thinner segments of the Ingleside near the Brazos-Colorado River Delta (e.g. Hoskins and Chocolate Bayou) are similar in width to their analogue east-Texas barrier of Follets

Island (see new Figure 4). With the exception of the Vidor segment this holds true for most of our analogue pairs (See new Figure 4) for the Texas LIG shoreline segments. This does not however, hold true for the eastern Gulf of Mexico LIG features. We have acknowledged that (lines 500-505) and hope the larger error bars applied to the estimates derived this way account for those differences. I have doubled the error estimates from the Simms et al. (2013) study to help account for the larger uncertainty given our assumptions.

*Reviewer Comment: For the LIG beach-dune systems running along the NE Yucatan coast, the author ignores the elevation of the beach ridge (and does not attempt to compare it with modern systems in the area) and focuses instead on the more reliable boundary between the cross-bedded beach facies and fore/back shore dunes which is reported by Ward and Brady (1979) to be +4.8 m. However the interpretation of the RSL highstand by these authors is +5 to +6 m, which is inexplicably accepted and a RSL of 5.5 $\pm$1.5 m is assigned. There is no analysis of stratigraphic evidence to support this interpretation or it's uncertainty. Is this beach-dune system representative of the RSL highstand, or its subsequent regressive stage?

**My response: Thank you for pointing out where my analysis could be better. I have revisited the estimates based on the beach ridges and used the contact between the upper shoreface/foreshore instead. Using the IMCalc program (Lorscheid and Rovere, 2019), I approximate the "breaking depth' of the waves and apply that value when assigning an indicative meaning to the stratigraphic contact. We do assume that the beach ridges and their related underlying shoreface/foreshore deposits represent the highstand and I added a statement about that assumption.

*Reviewer Comment: This is followed by a strange section on dated corals found in lagoonal units associated with the beach-dune systems. (Again the significance of the underlying/adjacent lagoonal unit is unclear in terms of transgressive vs regressive stage). The author attempts to use dated, non-depth-specific coral genera (Montastrea, which is now Orbicella) to constrain the RSL highstand, following the protocol in

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Hibbert et al 2016. The author states that using Hibberts strict interpretation a Montastrea (species unstated) at +2 m gives a RSL interpretation of 11.7 +8.6/-7.3 m. This makes no sense. You cannot precisely constrain the RSL highstand using an unidentified coral, with a low-precision age, and a large habitat range. And you cannot say that an in-place coral found at +2 m could have grown 7.3 m deeper! The only option is to determine their consistency with respect to the most reliable RSL indicators (like an intertidal beach or reef crest). This is a problem with the Hibbert et al (2016) protocol and should be addressed in the section on 'uncertainty and data quality'.

**My Reponse: I removed the discussion related to the Hibberts et al. (2016) applications for where other data is available (everyone but Belize) and simply use the ages as limiting data, which agrees with the revised overlying beach ridge-based estimates. We included a statement about the uncertainty introduced by using the Hibbert et al. (2016) protocol in the section on "uncertainty and data quality."

*Reviewer Comment: For the LIG reef systems in the same area, the author details the elevation of reliable RSL indicators such as reef crests, before dismissing these in favor of individual corals. This time the coral species is the depth-restricted reef-crest coral A. palmata, which is only a reliable RSL indicator when found in a monospecific assemblage. Using the coral's total depth range (as suggested by Hibbert et al 2016) clearly dilutes its utility as a RSL indicator. So instead of using the most precise indicator, the reef crest itself, the elevation of in-situ corals from the 0-5 m depth zone are used to determine the RSL highstand, giving a +6.4 (+1.2/-7.9) value for an in-situ coral at 4.9 m. Again this makes no sense. You cannot claim that an in-situ coral found at +4.9 m actually grew 7.9 m below this level. The only thing you can say is it grew at a maximum of 1.2 m below SL. When evidence of a lower reef-crest unit is assessed, the level of the reef crest is used as the RSL indicator, not the elevation of its corals. This is correct, but completely the reverse of what was accepted for the highstand reef. This inconsistency is the problem.

**My response: I thank the reviewer for pointing out the shortcomings of simply applying the results of the Hibbert et al. (2018) zonation of individual coral species to the well-documented reef facies at Xcarat. Thus I have changed the LIG RSL based on the Xcaret data to fully utilize the stratigraphic information at the site. I struggled to find a reference citing specifically the reef crest elevation for Carribean corals but did find one for the reef flat. A study by Cubit et al. (1986) found that the reef flat forms 6 cm below MLLW. I added to that $\frac{1}{2}$ the tidal range (15 cm) to arrive at the reef flats indicative meaning of 20 cm below mean sea level. Thus RSL at the LIG based on the higher reef tract at Xcaret is +5.7+/-0.2 m, the error being the root sum of the squares of the indicative meaning (1/2 the tidal range) and the measurement error. This indicative meaning assignment for the reef flat places the related reef crest at about mean sea level, which is what I think the reviewer was alluding to for its indicative meaning.

*Reviewer Comment: For LIG reefs from Belize, the same problems occur with the age and elevation of individual corals being used to define RSL highstand estimates that are significantly below those in the Yucatan. These may be a result of subsidence, as the author suggests, but it could be that reef development during the LIG occurred at a lower stand of SL before the highstand was attained, and that the other non-reefal deposits developed further inland along the unstudied coast of Belize. (i.e., that reefal deposits might equally be transgressive systems and do not represent highstand units). Clearly without a precise and reliable chronology different stages of development cannot be identified. And regardless of what geochronologists claim, the present system of correcting LIG ages for open-system behaviour has yet to provide a well-constrained SL reconstruction, or even stratigraphic consistency between and within sedimentary units.

**My response: Yes, I agree that the analysis on the Belize reefs is also fraught with the limitations of the Hibberts et al. (2016) approach (which I have now acknowledged in the section as already suggested). Therefore for the Ambergis Cay site, which Mazzullo (2006) describes as a reef flat facies, I used the same approach as with the Xcaret data. However, as the elevation datums and facies architecture are not as well characterized

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as they are at Xcaret, I provide a larger error of ±1.0 m. However, the other Belize reefs are not as well described in terms of their sedimentary characteristics and indicative meaning (we added a statement to that fact on lines 387-388). Thus I was only left with two options for my interpretations of those deposits: a generic, shallow marine reef with an arbitrary error assignment or use the approach of Hibberts et al. (2016). I favor the latter. Nevertheless, I point out these uncertainties including the reiteration of the point that the Belize reefs might not necessarily represent the highest MIS5e deposits but could represent earlier (transgression leading up to MIS5e) or later reefs (e.g. post MIS5e) in line 418.

*Reviewer Comment: Details: Line 383: the tidal range stated by Blanchon et al 2009 and Blanchon 2010 is 0.3 m with any data point having an uncertainty of ±0.15 m.

**My response: I changed this, thank you.

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