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Interactive comment on "A Late Pleistocene sea level stack" *by* R. M. Spratt and L. E. Lisiecki

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Referee comment:

"I thought this was going to be a great study to consider, but in the end felt disappointed. This study to me seems to be just another example of taking good records that have taken many years to perfect, smear them together in a fairly arbitrary manner, and then running some basic statistics over the top, to try and produce a 'synthesis' with an 'improved signal to noise ratio'. Earlier, work by Kopp et al. 2009 (which by the way was not, at all, an only-coral-based assessment, as suggested in the final paragraph of section 4) did something similar, albeit in a more sophisticated manner and for a shorter time interval, but even that study is blighted by the problem of arbitrary choices of chronological alignment between records, pulling some around to the limits of, or beyond, their stated uncertainties."

Authors' reply:

"Thank you for your in-depth critique. First, we will correct our assertion that Kopp et al. (2009) is an only coral-based assessment. That compilation also included sedimentological facies, non-coral biofacies, erosional features (e.g., raised beaches), and oxygen isotopes."

Referee comment:

"There have been several other 'syntheses' of late that all use versions of this approach; perhaps it is because of the lure of avoiding the hard graft of working up something original, in favor of writing yet another easy compilation with some statistics to get a potentially well-cited paper."

Authors' reply:

"We appreciate the hard work that has gone into each individual sea level record. However, synthesis of these records is also valuable because each record has its own assumptions and errors. If these records are all well-constrained measures of sea level, then synthesis will reveal their respective levels of agreement or discrepancy.

As the referee notes, publication of syntheses of paleoclimate data is common and these syntheses often provide value to the paleoclimate community as evidenced by their citations. Some specific examples of other recent synthesis studies are: Huybers and Wunsch (2004); Clark et al. (2012); Milker et al. (2013); Gibbons et al. (2014); Shakun et al. (2014)"

Referee comment:

"The original LR04 stack was a game-changer in synthesizing benthic delta-O18 records, but was later found to be blighted by assumptions of synchronicity between records that are made implicitly by use of the Match software (e.g., Skinner and Shackleton 2005 QSR). One of the authors of the current manuscript even had their own paper in 2009 (Lisiecki and Raymo, Paleoceanography 2009) in which this implicitly assumed synchronicity was demonstrated to be flawed. Yet here we see it again, and again without any attempt at proper propagation through all methods and conclusions of the uncertainties and limitations."

Authors' reply:

"It is true that benthic d18O records are not synchronous by as much as 4 ka during terminations, so there is potentially some smoothing by neglecting these potential differences. We mention age model uncertainty in the manuscript (page 3707, lines 13-14; pages 3710-3711, lines 28-1), and we will add more discussion of this source of uncertainty in revision. However, our conclusions are not overly sensitive to this uncertainty because the manuscript focuses on highstands and lowstands, rather than the timing or rate of glacial terminations. For example, note that the highstand and lowstand estimates in Tables 2 and 3 were identified individually in each record (page 3710, lines 9-14) and, therefore, are not based on the assumption of synchronous d18O change. Nonetheless, these tables reveal large discrepancies in estimates from different records. For example, the standard deviation of MIS 11 sea level estimates as compiled in Table 3 is 26 m. This level of disagreement is an example of the kind of valuable information a synthesis study can provide."

Referee comment:

"The signal matching approach needs to be relegated to history, if it is not backed up by a strong physical rationale, and/or rigorous independent testing, and/or proper uncertainty propagation. Certainly in the way applied in the current study, it is an antiquated approach that is known to be flawed. I suggest that it would be time better spent for the researchers to instead start working on developing independent and testable chronolo-

gies for each of the records."

Authors' reply:

"Testable chronologies for each of the records would be a valuable contribution to science. However, basic comparisons can be done in advance of such work. In some cases, such as Bintanja et al, (2005) a locally developed chronology would not be possible because this record is based entirely on the LR04 benthic stack (Lisiecki and Raymo, 2005). Although not perfect, the Lisiecki and Raymo (2005) benthic d18O age model is currently widely used in the paleoclimate community: For example, it has been used in a variety of ways for basic comparison to local climate records (e.g., Melles et al, 2012) and as a general measure by which to compare models of 100-kyr glacial cyclicity (e.g., Abe-Ouchi et al, 2013)."

Referee comment:

"In this context, I was surprised that the Red Sea record used is not the most recent version that I have seen (Grant et al. Nature Comms 2014), which has an independent chronological assessment and full probabilistic assessment using the age uncertainties as well as the method uncertainties. That should be used, and then any chronological adjustments needed would need to remain within the limits of that assessment."

Authors' reply:

"All records in the stack need to have comparable age models. Comparing records on independent age models would create larger errors by systematically smoothing (and hence, underestimating) highstands, lowstands, and orbital power. If we could have correlated benthic d18O to Grant et al. (2014), we would have. Although we consider this age model better than LR04, the Red Sea sea level curve does not have an accompanying open-ocean benthic d18O time series to which other records could be correlated. Also, Grant et al (2014) does not span the full 800 kyr."

Referee comment:

"Similar independent age assessments need to be developed for the other methods; this is where the real challenge lies, and where advancement of sea-level understanding will come from. It is only once that is done, that we come into a position to consider putting the records together (each on their own proper timescales) to evaluate common signals and differences."

Authors' reply:

"We disagree that basic comparison has to wait for this. It may not be possible to produce independent age estimates for each marine core due to a lack of ways to date each record individually back to 800 ka."

Referee comment:

"Even if we accept the chronological matching as done (though I don't see why we should; see above), then I still remain very worried about the lack of propagation of the legion uncertainties that arise from assumptions and adjustments in the chronologies, through the method and into the final conclusions. I am convinced that the uncertainties around the end product, and any further manipulations based on it, will increase greatly when this is done."

Authors' reply:

"At the moment we don't provide uncertainties because there are too many poorly quantified sources of uncertainties (e.g., amount of lead/lag between benthic d18O at individual sites, amount of difference between sea level and d18O of seawater at specific locations). An estimate that does not include them all would be misleading. Therefore, we must rely on metrics of the amount of agreement between different records,

such as the standard deviations of (age-independent) highstand and lowstand estimates in Tables 2 and 3, and a bootstrap assessment of errors which will be added upon revision, based on the suggestion of reviewer 1."

Referee comment:

"I am particularly worried that the difference between linear and non-linear regressions in section 6 may not be robust when considered relative to fully and properly propagated uncertainties."

Authors' reply:

"The difference between the linear and nonlinear regression is 10-20 m during highstands and lowstands (pg 3714, line 4). One potential concern, therefore, is that the scaled PC1 highstand estimate for MIS 9 differs from the mean of the seven highstand estimates by $\sim$12 m (Table 2). However, for MIS 5e and 7c, the two estimate methods agree to within 5 m, and the standard error of the highstand means is 4.6 m (calculated from Table 2: sigma/sqrt(n) = 12/sqrt(7) = 4.6 m). Additionally, Kopp et al (2009, 2013) estimated that the MIS 5e sea level maximum was very unlikely greater than 10m, whereas the mean sea level for 5e must be lower. Thus, the linear regression is not consistent with uncertainty estimates for 5e and is also significantly too high for the Holocene. Ice core evidence also suggests a change in interglacial characteristics between MIS 11 and MIS 9 (pg 3714, lines 20-23). Uncertainty is a larger concern for glacial lowstand estimates, as explained in the manuscript (pg 3715, lines 2-6). This creates uncertainty about the exact functional form of the change after MIS 11, but the interglacial data are sufficient to demonstrate that some kind of shift occurred in the relationship between benthic d18O and sea level."

Referee comment:

"A further concern is that the various methods underlying the different sea-level records that are used, are not independent of each other. As such one could wonder if straight PCA is an appropriate analytical tool. After all, we're not just looking at covariances between independent estimates with a common signal, but at covariances between methodologically (partially) related estimates with a common signal. This is not discussed, and there is also no assessment of how the methodological dependence might affect the answers (and their uncertainties). I think this would need some serious thought and discussion too."

Authors' reply:

"We briefly touch upon one advantage of using PCA in the interpretation of PC2 and 3 (page 3715, lines 18-19). However, in revision we will add more justification for the use of PCA in the methods section. (See also response to reviewer 1). For valid reasons our reviewer mentioned that the records are not 'unrelated'. This is exactly the reason why we use PCA, which can identify similarities and differences between the seven records used. PCA allows us to discern the common signal (PC1) as well as detect where the biases in the individual records are being replicated: our PC2 and PC3 scores elucidate differences between Atlantic vs. Pacific, and surface vs. deep, respectively. To address concerns about sensitivity of the result to the PCA method , the revised manuscript will include a comparison of PC1 and the unweighted mean of the sea level records (see reply to referee #1)."

Referee comment:

"Finally, there are some unsupported manipulations, such as the 2 ka lag to the smoothed LR04, as applied in section 6."

Authors' reply:

"The 2 kyr lag (line numbers 24-26, page 3713) was computed as the lag which maxi-

mized correlation between the two records. Therefore, we use this as the characteristic lag between benthic d18O and sea level. We will add text to the manuscript to clarify this step."

Referee comment:

"I don't think that this study as is does anything to advance understanding and to improve the state of the art. It's the sort of exercise that one might expect from an MSc student, perhaps, but it is not going to help us understand sea-level variability any better than the individual input records. The study would only introduce a false sense of 'understanding' that is flawed because of the (often unspecified) underlying assumptions, uncertainties, and questionable manipulations. It is evident that the real challenge is to get the different records onto their own independent chronologies, and to then compare them statistically (using appropriate statistics and proper uncertainty propagation). That may not be so easy to do, but true understanding doesn't need to come easy. Certainly not when 'easy' is using a known flawed approach. I recommend rejection of the study as is."

Authors' reply:

"It is apparent that the reviewer has a different viewpoint about the best way to advance sea level studies; however, a variety of study approaches is healthy for the advancement of science. Although there are still many uncertainties (particularly with respect to age models), there is potential benefit to the community to performing initial comparison and synthesis as long as the manuscript is transparent about the study's limitations and sources of uncertainty."

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