Clim. Past Discuss., 11, C1480–C1482, 2015
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Climate of the Past Discussions

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

Anonymous Referee #1

Received and published: 26 August 2015

In this paper, the authors combine seven/five continous sea-level records over 0-430 ka/0-798 ka, produced in the literature using a range of techniques (benthic foram oxygen isotopes + Mg/Ca paleotemperature, planktonic foram oxygen isotopes + Mg/Ca or alkenone paleotemperature, hydraulic modeling of oxygen isotopes of semi-cosed basins, inverse modeling of oxygen isotopes, and regression of oxygen isotopes against coral sea-level records). They perform a principal compoment analysis on these sea-level records and find that the dominant principal component is approximately their average, which they scale to -130 m at 24 ka and 0 m at 5 ka to produce a composite sea-level record. They then make some observations about the spectal analysis of the composite record compared to the spectral analysis of the Lisiecki Raymo 2005 oxygen isotope stack.

A comparison and a meta-analysis of the continuous sea-level records analyzed here are highly valuable. However, the current meta-analysis suffers from two significant

flaws, one critical.

The critical flaw is that there appears to be no treatment of the uncertainty in the underlying records. These uncertainties are not negligible (indeed, the authors state that one of their goals is to reduce the signal-to-noise ratios seen in the individual records). For example, as the authors note, the sea water oxygen isotope-derived records uncertainties have 1σ errors up to about 20 m and the inverse ice volume model derived records has a 1σ error of 12 m. (These errors are, more over, not fully uncorrelated and should not be treated as such, when they are treated.) But the authors appear to be working with simply the mean estimates of each of the underlying records. It is therefore impossible to assess the robustness of their composite curve. If they retain their current meta-analysis methodology, a bootstrap assessment of errors would seem like a minimal necessary statement.

The second significant flaw, which I view as serious but not critical, is that PCA is a bit of a slightly odd methodological choice for this analysis, as it ignores a key piece of prior information. All of the records are (supposedly) independent measures of a common signal. There are reasons to think that, say, the relative sea-level records will be less correlated with total ice volume change (which I think may be what the authors actually mean by 'eustatic sea level') than measures of ice-volume derived from open-ocean $\delta^{18}]O$, but that relationship is more complex than the simple scaling provided by a weighted average. So why do the authors think that the scalings associated with PC1 provide a better estimate of their target than an unweighted mean of the records? If they don't, why are the throwing out the prior information that tells them they are all noisy measures of a common underlying signal?

A minor note (p. 3711): the MIS5e sea-level estimate is usually (and appropriately) quoted as 6-9 m. The analysis in Kopp et al. (2013) of the well-resolved post-129 ka highstand stated, "within the LIG period, it is extremely likely (95 per cent probability)/likely (67 per cent)/unlikely (33 per cent)/extremely unlikely (5 per cent) that the highest peak GSL well resolved by observations exceeded 6.4/7.7/8.8/10.9

m", and is in agreement with a coral-record from the Seychelles, corrected for GIA and fingerprint effects, indicating a peak of 7.6 ± 1.7 m (Dutton et al., 2015, doi:10.1016/j.quascirev.2014.10.025).

Interactive comment on Clim. Past Discuss., 11, 3699, 2015.