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Climate of the Past Discussions

Interactive comment on "Inter-annual tropical Pacific climate variability in an isotope-enabled CGCM: implications for interpreting coral stable oxygen isotope records of ENSO" *by* T. Russon et al.

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We are grateful to the anonymous reviewer for their constructive review of the MS. The reviewer raises two substantial issues, which both overlap to some extent with the comments of Dr. Berkelhammer. The second of these points relates to whether, and if so how, the model results might inform the interpretation of existing coral stable isotope records. We propose that this point will be sufficiently dealt with by the proposed additional discussion paragraph given in the response to Dr. Berkelhammers review. The 'take home message' of this is intended to be that the model results probably should

not directly inform the interpretation of existing/future records, but should make us think carefully about the potential errors involved in using records from (some) locations for which long modern coral to SST calibrations may not be available. As the proposed additional paragraph also states, there is also scope for using this kind of model output as an alternative pseudo-proxy field, which could be used for investigation of more sophisticated field reconstruction methods for multi-site records, but as the current reviewer acknowledges, this would represent a very sizeable additional body of work and we consider that this is best referred to future work at present.

The other issue raised relates to better quantifying the extent of the spatial bias in the underlying model climate, namely in terms of the westward extent of the model cold tongue. We entirely agree with the reviewer that this presents a fundamental limitation to the study, especially as much of the most interesting model behaviour is seen to be in the region defined by the end of this feature (i.e. the 'WCT domain' in the terminology of the study). However, we also emphasise that this is an inherent feature of the free-running HadCM3 model (and indeed of the vast majority of CMIP3 and CMIP5 generation CGCMs that resolve any kind of 'ENSO-like' behaviour, as may be seen in, for example, the Guilyardi, 2006 reference) and that the only way to avoid this issue would be to either a) use a different GCM with a less severe cold tongue bias (such as is apparently seen in preliminary analysis with some of the CMIP5 models, although these do not contain stable isotope capacities) or b) to use a flux corrected version of the model to correct for climatological bias in the SST field. Both of these possibilities are now also mentioned in the proposed additional discussion section paragraph. The reviewer suggests an additional figure to quantify the spatial biases, presumably entailing replication of the panels of Fig 1 for the relevant instrumental fields. Our feeling remains that this would largely replicate existing work with HadCM3 (e.g. Collins 2001, Toniazzo 2006, Guilyardi 2006, Tindall 2009), but will be guided by the editor as to whether such replication would substantially improve the cohesion of the present MS. We do concur that some quantification within the text of the 'extent of the problem', at least in terms of the underlying SST biases in the specific regions we use as exam-

ples, would be appropriate. We propose to add/extend the text as follows in the results section:

Page 747: line 22 - . . . so-called CGCM cold tongue bias (Guilyardi, 2006). The extent of this bias in HadCM3 is such that the equatorial region for which inter-annual SST anomalies are positively correlated to those in the eastern equatorial Pacific extends right across the tropical Pacific domain (to 120° E) in HadCM3, rather than to 160° E as in HadISST. In consequence of this, inter-annual SST variability in the western equatorial Pacific is greater in amplitude than that seen in the real climate. For example, the inter-annual variance of the SST field averaged over the western equatorial Pacific NINO4 box (5°N to 5°S and 160°E to 210°E) exceeds that seen in the HadISST instrumental record (Rayner et al., 2003) by around a third. The relative magnitude of these bias increases as one moves westwards across the equatorial Pacific, attaining at a maximum value in the westernmost part of the model cold tongue, where the relative model inter-annual SST variance is over four times that seen in HadISST.

We agree that it would be desirable to offer some quantification of the extent to which these underlying biases may affect the current results (i.e. the metrics used in the study). However, short of performing the aforementioned flux-corrected experiments with the HadCM3 model (and even this would only go some-ways to addressing the issue, as discussed above), we cannot see any simple way to perform such an analysis at present. The strategy of assuming that all other factors remain constant and then scaling the models inter-annual SST variability at some given location to correct for the discrepancy seen with the amplitude in the instrumental record could be taken, but this is a very crude assumption and neglects the fact that the relationship between inter-annual SST and $\delta^{18}O_{sw}$ variability is demonstrably non-linear at many locations in the model. Nonetheless, we have undertaken this analysis and it shows that F_{sw} values in the NINO4 and WCT domains would increase by 0.05 and 0.15 respectively, values which are clearly consistent with an adjustment, rather than fundamental change, of the spatial pattern of Figure 2A. However, this result is somewhat circular, due to the

assumption inherent within it that inter-annual $\delta^{18}O_{sw}$ variability is independent of that in SST. Therefore, whilst the motivation for the inclusion of some form of sensitivity test is clear, we do not feel that the approach described here would add meaningful quantification of these uncertainties and could also lead to a loss of clarity in the MS. That said, further discussion of the possible impacts of these biases on a semi-quantitative and qualitative level is certainly possible and we propose to add the following text to the discussion section to make explicit the potential relationship between underlying spatial bias and predicted non-linearity in model $\delta^{18}O_{coral}$ - SST relationships.

Page 758: line 7 – The strongest model non-linearities are seen to occur in the region for which the cold tongue bias in terms of inter-annual SST amplitudes is the largest, namely the WCT. However, substantial El-Niño tail non-linearity in the $\delta^{18}O_{coral}$ - SST relationship is observed across much of the equatorial Pacific, including in regions for which the influence of the cold tongue bias is relatively modest (Fig 4B). For example, the $RMSE_{90}$ value for the averaged fields over the NINO4 box region, for which the inter-annual SST amplitude bias is much less pronounced in both absolute and relative terms than in the WCT, is 1.6. Whilst the case-study of the model WCT box may represent an upper bound to the extent of such behaviour that might be expected within the real climate system, the presence of such features is also unlikely to be a consequence of the underlying spatial biases in the HadCM3 ENSO realisation alone.

We would, of course, welcome any further input from the reviewer and/or editor on how we could deal better with addressing this issue. The remaining comments from the anonymous reviewer are primarily linguistic and will be addressed, as recommended, during final revision of the MS.

Interactive comment on Clim. Past Discuss., 9, 741, 2013.