

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2022-14

Anonymous Referee #1

Referee comment on "The tropical route of quasi-biennial oscillation (QBO) teleconnections in a climate model" by Jorge L. García-Franco et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2022-14-RC1>, 2022

This work by Garcia-Franco et al. looks at the relationships between the QBO and tropical climate in observations and centennial pre-industrial CMIP6 simulations with one coupled climate model. The connections are difficult to diagnose from observations so long simulations are useful.

The paper is overall interesting and covers many topics, but the authors should check the consistency of the symbols and names used (see comments by line). It can be confusing to read different acronyms for the same quantities. The units reported in the plots should be verified.

Given the central role of model simulations, more information on its skill at simulating QBO and ENSO should be provided. For example, how realistic is the QBO amplitude at 70 hPa for this specific model? Apart from composite differences, some climatologies should be discussed. In the introduction reference to Geller et al., 2016 on gravity wave changes would fit. Model-dependence of the results should be stressed, since different configurations of a single model are analysed and QBO/SST biases may play a big role.

The causality analysis on how the QBO influences ENSO is not very convincing as it stands.

I guess the authors should also say something about the frequency of LN/EN events during neutral QBO (QBO-N).

The section about monsoons should be revised and maybe shortened, since QBO surface impacts may be very dependent of any QBO bias. For example, Giorgetta et al. 1999 (cited) nudged to QBO, so it was realistic in their case.

The data description should be modified to provide pertinent information.

Specific comments by line

L52, maybe 'on the convective process'?

L55, define 'CMIP', rephrasing L62

L63, are GWs tied somehow to sources?

L76, both monthly means?

L82, is there a reason for not using the standard 0.25x0.25?

L83, it is a bit strange to put the (generic) link only for ERA5;
I would move to data availability with direct links for all datasets
(for ERA5 <https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-pressure-levels-monthly-means?tab=overview>) and proper citation
<https://confluence.ecmwf.int/pages/viewpage.action?pageId=197704114>

L90seq, define 'N' and 'ORCA' for the components resolution

L91, UKESM or UKESM1?

L94, So 3 simulations in total? Would be good to state that you have two models with lower resolution and one with better resolution, which is the main interest.

L96, I would move to 'data availability' or similar

L98, Here or later I would add something about some relevant model properties (e.g. both models have more spectral power in 2-3 years compared to observations). Also ocean resolution seems to be important for mean biases, and the realism of the ITCZ should be mentioned as well.

L105, What about UKESM1? Not sure why only HadGEM is mentioned.

L111, years or months?

L117, Which levels? Above you just mention 70 hPa.

L119, '1' and '2' are subscripts

L124, The product you use (GPCP?) for this index is providing convective and stratiform precipitation separately? Or is it a total precipitation? If not, remove convective (here and also in all instances following).
Can you explain why using a precip-based IOD index rather than the standard SST-based one?
Please add a reference if it was used before.

L125, I'd use same style for EN3.4, with []

L133seq, This symmetry seems strange (given the ENSO asymmetry and QBO stalling) can you provide numbers?

L135, This is 'observed' for ERA5? Can you provide the values for HadSST? It is useful to compare model/observation statistics.

L140, Maybe start with 'When estimating correlations, they are...'

Fig 1, 'mm day⁻¹' in brackets, or move 'pr' to title

L159, Please comment on the ITCZ realism.

L162, Add reference

L206, I guess would be useful to have a table in the method section with the

different numbers for ENSO and QBO. Why 120, does it have a special meaning?

L209, But the wet anomaly in the Pacific and dry in the Atlantic are more marked with ENSO included.

This is also seen in Fig5.

Fig5, If regression coefficients are re-scaled (caption), then a prime is missing in a&d titles.

See Supplement as well.

L214, (1) -> (Fig. 1)

L216, it was EN3.4 before

L221, why no significance in FigS3?

L225, mention Gray et al., 1992

Fig6, I'd use E and W for QBO in (b). Moreover I would define once all the acronyms (EN, LN, E, W) in the methods and be consistent throughout (no 'ea', EN3.4, etc.). Suggest NE or NN for Neutral ENSO. Moreover, would it be easier to read the plot ordering the boxplots as LN/NE/LN ? Why not showing E and W phases separately for the amplitude?

L238, Have you stated which level are descent rates for? From the methods I got that the amplitude is integrated in the 10-70 layer, but descent rate is by level.

L246, See Geller et al 2016 about GW variations.

L252, So the frequency would be for example (# months EN) / (# months W)? Maybe mention that IOD will be considered later?

L260, ENSO3.4 -> EN3.4 (or maybe ENSO)

Fig 7, [] missing around mm day-1 (check other plots as well). I guess IOD-prc is same as IOD?

L266, write months in full. Can you elaborate on how the difference model/obs depends on the ENSO evolution in the model (e.g. Lengaigne et al., 2006)? Also worth noting how the model index amplitudes are 2-3 times smaller than obs.

Fig8, as before, why 'convective'? Why now using a higher confidence level?

L275, but could this be model-dependent?

L280, Please avoid the mix of abbreviations and months in full

L286, Maybe the Indian Ocean sector, rather than IOD?

L293, why '.'?

L295, atmospheric circulations. However, the model biases should be noted.

L300, How are these longitudes selected?

Fig9, Only convective, stratiform rainfall removed? Is panel (b) indicating a double ITCZ bias?
Can you comment in the text?

L317, remove 'rate'

Fig10, define acronyms MSD, NAM. For more direct comparison you could mask values over oceans? Do you know why the regions show very net boundaries in some cases? Compare with Lee and Wang, 2012 their Fig4

Fig11, I am confused by vector sizes. They are 3 or $0.3 \cdot 10^{-2} \text{ Pa s}^{-1}$, but their lengths do not differ by a factor 10. Please clarify. Also the plots are quite busy, can you try improving them?

L330, Mention the QBO biases which may be important

L335, If you integrate to the top, then the integration bounds are swapped and $0 \rightarrow p_{\text{top}}$ (or p_{surf})? Gravitational acceleration (g) rather than constant (G)? How do you compute the divergent component of zonal wind?

L351, To me some QBO/ENSO superposition can also be seen from the plots.

L406, or role of QBO bias...

L416, have you ever mentioned TRMM in the text?

L420, 'observations' \rightarrow 'variables'

L422, revise. you speak about days, I understand the input data is monthly mean, so is this weighting already built in? Does the MOHC model have 360_day calendar?

L435, I guess the 'i' subscript is redundant with one predictor? Same in Fig S3

L440, State that summation is ' $j=1 \dots N$ ', as X_0 appears already

L446, Is there a stray A3?

L513, why uppercase?

Additional references

Geller et al JGRA 2016

<https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2015JD024125>

Gray et al., JMSJ, 1992

https://www.jstage.jst.go.jp/article/jmsj1965/70/5/70_5_975/_article

Lee and Wang, CD, 2012 <https://link.springer.com/article/10.1007/s00382-012-1564-0>

Lengaigne et al., JC, 2006

<https://journals.ametsoc.org/view/journals/clim/19/9/jcli3706.1.xml>