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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Representation of the Tropical Stratospheric Zonal Wind in Global Atmospheric Reanalyses" *by* Y. Kawatani et al.

Anonymous Referee #3

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This paper assesses the tropical stratosphere variability in contemporary (and not so contemporary) climate reanalyses. Focus has been restricted to the effects of observational inhomogeneities going into the datasets, primarily from radiosondes, and how they constrain the observed features of the quasi-biennial oscillation and their potential role in the differences seen between datasets. It is found that the inter-model disagreement coincides to data poor areas, especially in the lower stratosphere. The reanalyses show good agreement over Singapore and show a progressive improvement at more recent times. Consistent and sizeable biases remain in the timing of the phases of the QBO especially during the easterly-westerly transitions at 10hPa.

This is a timely and well-written paper which will well-complement forthcoming science-focussed papers on tropical stratosphere variability. The number of figures is perhaps a little large, but they are generally of good quality. If the number of figure panels were

to be reduced, the ability to see fine features in the data would be really improved. I recommend publication pending due consideration of the points outlined below.

Main points:

I am unable to understand why it is difficult to establish what observations have gone into the various reanalyses. I would imagine the information is likely to be conspicuously posted on the individual Reanalyses Centres' websites or have been collated by other groups participating in the SPARC S-RIP project. I would have thought the Reanalysis Centres would find it particularly informative where (inter-model) differences are potentially coming from. This information should be included (or pointed to) in the paper, in some convenient way.

There is an inconsistent use of MERRA-2 data within the paper. As stated by the authors, MERRA-2 represents the ONE dataset whose forward model reproduces a QBO. This reviewer for one, would be especially keen to see that dataset assessed more throughout the paper. Is there a reason why 10hPa was chosen to assess the QBO phase timings? Outside of those times, there actually appears to be a better correspondence between MERRA-2 and the balloon record.

Conclusions referring to the wavenumber structure of the reanalyses' tropical wind need to be tempered a little. Figure 10 identifies a period where the high latitude stratosphere was particularly active. Other figures indicating wavenumber structures in longitude over a longer period of time refer to reanalyses differences (i.e. SD)

Finally, there are a lot of details going into the (numerous) figures. Some of these details were difficult to pick up even when blowing the figures up on screen. Can the authors make sure the figures are not lossy and try to improve the clarity between different models. The authors may also consider looking for a colour-blind-friendly contour scheme. Perhaps the authors should assess if they really need to include all 19 figures (>100 panels) in the main article.

Other points: (L28, P4) "...well-known summary..." (?)

(L9, P4) Presumably the interpolation to the FUB/IGRA data is done from the native reanalysis model resolution and not from the common ERA-I resolution mentioned in the previous sentence?

(L29, P5) Can the authors please find a suitable reference for the statement that NCEP-CFSR uses ERA40 winds in the tropics at and above 30hPA from 1 July 1981 to 31 December 1998. That is extraordinary!

(L10, P6) As the tropics are the focus for the paper, it would make sense to limit the latitudes in figure 3 to something like 20-30 degrees, for example. I do not think the extratropical SD differences show anything interesting anyway.

(L20, P6) Why have the authors chosen levels below 100hPa in figure 4? They should explain the reasoning behind this. Not much is written about this figure.

(L25, P6) Perhaps refer here to 'Indonesia' or the 'maritime continent' rather than the 'warm pool'. Also, the central (through to western) Pacific is where the most conspicuous SD values reside. This clarification may be important in pointing to other sources of model disagreement: in particular modes of (ocean-)atmosphere variability (e.g. El Nino-Modoki).

(L32, P7) Bogota data is on panel j not h. Also the authors should consider doing a an F-test to compare the differences in variances with and without IGRA data.

(L21, P9) It is a pity that MERRA-2 is not shown here, whilst perhaps attempting to minimise the strong SAO signal (at 10hPa) afflicting a couple of years early in the time record. As that model has a spontaneous QBO it would be very interesting to see the timing of the E/W and W/E transitions. It would clearly be less reliant on analysis increments and sufficient observations to constrain the QBO phase progression.

(L8, P10) It is evident that all 5 reanalyses shown do actually show a positive anomaly near the maritime continent, although there is evident a local maximum in 3-4 of the

datasets (probably highlighted due to contouring). Might also highlight (and reference) the fact a wave-1 warming occurred during December 1998 (and a wave-2 in February 1999)

(L17, P10) subtitle: "Dependence of the ? Difference..."

(L29, P10) "The overall larger SD in the westerly QBO phase, as compared to the easterly QBO phase,..."

(L7, P12) "...has reported stratospheric..."; reference, Pers. Comm.? The sentences following this need to be looked at. It is mentioned that a change in SD occurs around 1998, but then it is mentioned that there was a bias in the forward model of JRA-25. But presumably the bias in the forward model will not be responsible for changes around 1998 (forward model should not change during the reanalysis period - unlike operational analyses)

(L25..., p14) The authors should relax the statement referring to wavenumbers 1 and 2 dominating the 'eddy' zonal wind. Figure 10, mainly shows the zonal anomaly of zonal wind for January 1999 (a month sandwiched between two SSWs), so may not be representative of conditions at other times (e.g. 1979-2001)

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