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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 #1

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The authors evaluate the representation of the stratospheric equatorial zonal winds from nine different reanalysis products against each other and against radiosonde observations, in particular the FUB wind record. They focus largely on the inter-reanalysis standard deviation (which is generally largest in the deep tropics) and comparison with radiosonde observations as a means of evaluating the reanalyses.

In nearly all cases they find that this standard deviation is anti-correlated with the spatial and temporal availability of radiosonde observations. In the mid-stratosphere (10 hPa), the standard deviation is dominated by the zonal mean component which is largest during transitions of the QBO phase where the reanalyses tend to lag the observations by 2 weeks to 2 months, particularly in the easterly-to-westerly transition. The eddy component correlates with the QBO phase, being larger during the westerly phase, and is apparently associated with the representation of extratropical stationary waves. Lower

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in the stratosphere and upper troposphere the eddy component becomes larger and the correlation with the QBO phase weakens. This structure appears to be associated with the stratospheric extension of the Walker circulation.

The analysis is a very useful contribution to the literature given the broad relevance of the QBO and the reliance of many studies on reanalysis products. The discussion is generally lucid and concise; my main criticism is that there are too many figures and it seems to me some of them could be removed (or combined) without impacting the central messages of the text. I would therefore recommend that the manuscript be accepted with minor revisions, either with a shortened figure list or stronger justifications in the text for those figures.

The discussion could also be strengthened by including some comments regarding the implications of these results for (a) studies using the reanalyses to understand the QBO or its impacts and (b) reanalysis centres trying to improve the representation of the winds.

With regards to (a), the bias in timings of the phase transitions could be relevant for studies which composite based on these dates, as could (possibly) the weak westerly wind maximum. The magnitude of the standard deviation in the horizontal winds throughout the tropics would also seem to be worth highlighting for trajectory studies given that there is some inclination to assume that all the inter-reanalysis differences are in the vertical velocities.

With regards to (b), one hypothesis that is raised is that having a forecast model with an internally generated QBO might reduce some of the biases. Given that MERRA 2 is in this category it seems that this hypothesis could be more explicitly tested; since it seems there are still significant biases, it would seem this is not sufficient to guarantee improved representation.

Could the errors (particularly in the mid-stratosphere) be associated with the slow advective propagation of information from the Singapore winds during phase transitions?

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This would give you larger errors during transitions. Is it more likely the renalysis forecast models are systematically biasing the winds relative to radiosonde observations during transition periods?

Perhaps relevant to both (a) and (b), if reanalyses (or free running GCMs) are going to use existing winds to nudge towards a QBO, which reanalysis would the authors recommend (or perhaps another way to ask the question, are there any that should be cautioned against?)

Specific Comments:

Figure 1 and 2 contain much the same information, but Figure 2 is more useful; the former could be omitted without losing any conclusions. Also the latter could be improved if the labels and titles of each panel in 2 were removed (say, using a single labeled time axis and annotations within the panel) so that the lines were more easily seen.

Similarly Fig 6 contains the information in Fig. 5 and adds to it; Fig 5 can be omitted.

Fig 8: The time-dependence of the standard deviation would be much clearer if it was plotted on a different scale than the winds themselves. It also might be more informative to plot the standard deviations from each pressure level on the same axis so their temporal relationship can be more easily seen.

Fig. 9 and 18 could easily be combined.

Figs. 11 and 12 could also be combined and 11 (a-b) omitted.

p5 l21-25 It looks from Fig. 2d and e there are still significant anomalies in MERRA 2 at 50 hPa and 70 hPa. Are these really still likely to be be associated with an overactive SAO? This would seem to contradict the claim in l 26-27, though it is difficult to distinguish the MERRA and MERRA 2 curves.

p5 l29-30 What exactly was done with the tropical winds in NCEP-CFSR? Were they nudged towards ERA40 winds over this period? Is there a reference for this or will it be

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mentioned somewhere in the SRIP report?

p6 l1-2 The justification for omitting MERRA 2 from much of the rest of the analysis is unclear to me, particularly since the authors return to it in Figs. 18 and 19. Why not just discuss it with the rest of the reanalyses? It also seems that NCEP-CFSR could be included for simplicity of method, though the case for omitting it is stronger. From Fig. 18 it looks like it does a good, if not better job of the transitions than other reanalyses; are the errors stronger in the middle of the QBO phase?

p6 l12-13 One of the main conclusions from Fig. 3 would seem to be that the SD is improving amongst more modern reanalyses products (at least up to MERRA - I'm guessing this would change if MERRA 2 were included here?)

p8 l10-15 Fig 13 a, b suggests that some of the structure in the eddy component in the tropical lower stratosphere might be associated with an extension of the Walker circulation - this is an interesting possibility and is distinct from issues of data availability. Since the phase of the QBO has not been considered in Fig. 13 would it make some sense to move the discussion on p 11 l 3-14 here? Also, as a test of this hypothesis, is the 70 hPa standard deviation correlated with the strength of the upper tropospheric Walker circulation?

p8 l24-26: Is there any correlation between the QBO phase and the number of radiosonde observations available?

p9 l10: This underestimation of the of the maximum westerly winds is one of the clearest biases and should be brought out more clearly in the conclusions (and possibly the abstract as well).

p9 21-25: This hypothesis could be evaluated explicitly here if the MERRA 2 winds were included. If they are not a good test of this hypothesis for some reason this could be explained here.

p10 l3-16: It's not clear to me why the authors have chosen to focus on a single case

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here - surely more robust conclusions could be drawn by looking at the wavenumbers of the composited eddy component of the standard deviation? Indeed it might be interesting to see a zonal wave number spectrum of the standard deviation at several levels.

p12 l30: It would be interesting to test the importance of the satellite observations for the tropical winds by including JRA55c which only assimilates 'conventional' observations. This would seem to be a good way to strengthen many of the conclusions in this section, and is exactly the kind of question for which it is perfectly suited.

p23 l24-33: Given the close resemblance of the structures in Fig. 19 to other structures we've seen in many of the figures, I think these conclusions could be made without showing the figure explicitly.

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