

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

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This is a very nicely written paper, but of course, I do have some suggestions for its improvement. I think this paper could benefit from a short discussion of the data assimilation process in the introduction. Such a discussion is not needed by those familiar with the data assimilation process, but many users of assimilation use the resulting products with out realizing that they are amalgams of data, the underlying model, and the statistical methods utilized. For this paper, I think it important at the beginning to indicate that the underlying model has its own climatology, and data, where present, nudges the resulting products toward observations. Also, unobserved quantities are adjusted to be consistent with the data being inserted together with the model climatology. To me, this together with the fact that as the Coriolis parameter tends toward zero the mass field constraint on the winds become weaker and weaker.

Another global comment is that reference should be made to Randel et al. (2004). Quoting from the summary section of that paper, “QBO variations in temperature and zonal wind are underestimated to some degree in most analyses, as compared to Singapore radiosonde data. The best results are derived from the assimilated datasets (ERA-40, ERA-15, METO, and NCEP, in that order) and only ERA-40 has realistic zonal wind amplitudes above 30 hPa. The use of balance winds in the Tropics (derived from geopotential data alone) is problematic for the QBO.” The authors may wish to state to what extent they are updating those conclusions.

The following are some more detailed comments.

1. Page 2, line 8: Perhaps the paper by Yoo and Son might appear in time to be cited. It shows that the QBO exerts greater influence on the MJO than does ENSO. The reference is as follows: Yoo, C., and S.-W. Son, 2016: Modulation of the boreal wintertime Madden-Julian Oscillation by the stratospheric Quasi-Biennial Oscillation**, Geophysical Research Letters, accepted.

2. Page 2, line 14: The authors may wish to add a reference to Naujokat (1986) who states, “The first three stations were used to produce a data set for the levels 70, 50, 40, 30, 20, 15, and 10 mb, which should be representative of the whole circumference at the equator since all investigations have shown that longitudinal differences in phase are small enough to be ignored.” The implication here, not specifically said, is that QBO amplitude differences among the stations are more substantial, and likely cannot be ignored. This statement seems to be consistent with the conclusions in Hemilton et al. (2004). The authors might then go on to indicate whether they feel the reanalyses can capture such asymmetries. They might if the extratropical planetary waves during QBO westerlies are well treated. Otherwise, I doubt they will.

3. Page 3 line 10: The situation is rapidly changing in that many models now produce spontaneous MJOs (i.e., GISS, CAM, etc.). Perhaps it would be better to say few GCMs used for reanalysis produce a spontaneous QBO.

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4. Page 7, line 18: I believe it also depends on the climatology of the underlying GCM.
5. Page 10, lines 30-31: Do the authors have any idea why this might be so?
6. Page 14, lines 27-30: To what extent do the authors think this might affect the FUB QBO data set, which is often taken to represent the zonally averaged QBO?

Again, I want to emphasize that this is an excellent paper. The figures are excellent, and clearly indicate the points being made.

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