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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Equatorial wave analysis from SABER and ECMWF temperatures" *by* M. Ern et al.

Anonymous Referee #1

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General comments This is an interesting paper describing the behavior of equatorial waves and internal gravity waves in the QBO in the equatorial stratosphere based on temperature data from SABER observations and ECMWF analysis over 4 years. With the aid of global distribution and fine time resolution of the data, the equatorial waves and internal gravity waves are carefully extracted using a two-dimensional power spectral analysis. The analysis covers the whole stratosphere, and is regarded as a nice extension of previous studies in 1990s using conventional radiosonde observation data in the lower stratosphere. Moreover, comparison between the SABER and ECMWF data sets shows the potential and limitation of the ECMWF data for the equatorial wave analysis which is useful information to the community. The manuscript is well organized and the discussion is mostly clear.

However, there are several important points that need to be considered and clarified to improve the manuscript before its final publication. First, the review of previous studies using radiosonde data should be more properly made in order to clarify the originality of this paper. Several works captured essential characteristics of short-period (or small-scale) waves in the QBO, although used radiosonde data are limited regionally. The works that need be referred to are described in the specific comments below. Second, it seems that the Doppler effect of the mean wind is ignored through the analysis of this paper. The QBO and SAO associate with the mean wind having vertical shear. Thus, the vertical wavelengths of upward propagating waves and their equivalent depths vary in the vertical by the Doppler effects. Moreover, the Doppler effects also modify the intrinsic frequency. Thus, strictly speaking, it is not appropriate to apply the theoretical dispersion relation (2) to the observed spectra because $\hat{\omega}$'s do not equal to observed frequencies in nonzero mean winds. The significance of the Doppler effects needs be clarified and the ambiguity from this effect should be discussed quantitatively. Third, some discussion about the effects of analyzed waves on the QBO is weak and not beyond speculation (e.g. 2nd sentence of page 11694 and 3rd paragraph of page 11696). The temperature amplitude is "not conserved" for conservative waves propagating vertically in the mean wind having vertical shear. Thus, it is generally difficult to discuss the wave dissipation only from the temperature data. This point should be more carefully taken into account in the discussion.

The other specific comments are listed below.

Specific comments

p.11688, l.14: Figure 1 shows the dispersion curve assuming that the background wind is zero.

p.11689, 3rd para (from l.13): Sato et al. (1994) focused on short-period (<3days) waves which are considered mostly due to waves with higher zonal wavenumbers. Sato and Dunkerton (1997, JGR, 102, 26247-26261), an extension of Sato et al, showed the

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momentum fluxes associated with the short-period waves and discussed possible significant contribution of these waves to drive the QBO. These papers should be properly referred to in this paragraph.

p.11690, 3rd sentence: Since the critical level is the level where the *phase velocity* matches the mean wind, the filtering behavior depends not only on the frequency but also on the zonal wavenumber. Thus, this sentence is not correct. Moreover, the critical level filtering and wave breaking are not the only process of wave dissipation. The radiative relaxation is sometimes important as discussed by Holton and Lindzen (1972, JAS, 29, 1076-1080).

p.11692, ll.11-12: It is generally difficult to examine the characteristics of waves having about 30 days using the 31 day time windows.

p.11694, ll.2-3: I do not understand the meaning of this sentence. The temperature amplitude can be modified by the background static stability and Doppler effects of the mean wind as well as wave dissipation and generation. Similar weak discussion is made on the 3rd paragraph of page 11696.

p.11697, 2nd paragraph: The discussion is not clear. Please describe how the spectral densities are obtained. How are they “normalized”?

p.11698, 4th para (from l.13): The QBO modulation of equatorial waves observed in Figs. 5 and 6 are consistent with results for long-period Kelvin (Wallace and Kousky) waves and Rossby-gravity waves by Sato et al. (1994).

p.11704, 4th para (from l.18): Comparison with studies using radiosonde data should be made in this section. Maruyama (1994, JMSJ, 72, 423-432) and Vincent and Alexander (2000, JGR, 105, 17971-17982) discussed the QBO and annual variation of gravity wave activity.

p. 11704, Section 5: I understand the importance to examine that phase and amplitude structures as well as the spectra. However, it is not clear why the authors chose the

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time period of this particular campaign of SCOUT-O3, although there are many observation campaigns in the equatorial region performed so far. There is little description of SCOUT-O3. It may be more interesting to show the wave structures in the QBO easterly and westerly phases, for example.

p.11707, discussion on Fig. 10: Since satellite data in the stratosphere such as TOVS are assimilated in the ECMWF analysis, the consistency between ECMWF and SABER data as shown in Figs.10b and 10c is not surprising for Kelvin waves with 10-15 day periods which usually have long vertical wavelengths. The comparison for shorter period waves is more informative for the readers.

Technical corrections

p.11702, l.23: Singapur -> Singapore

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11685, 2007.