Atmos. Chem. Phys. Discuss., 9, C3647–C3650, 2009
www.atmos-chem-phys-discuss.net/9/C3647/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Properties of the average distribution of equatorial Kelvin waves investigated by ray tracing techniques" *by* M. Ern et al.

Anonymous Referee #2

Received and published: 6 August 2009

In this work the vertical propagation of Kelvin waves is studied using a wave action conservation equation that includes turbulent and radiative damping effects. The results are compared with Kelvin wave diagnostics from ECMWF analysis. Launch amplitudes are determined from ECMWF analysis and then Kelvin waves are propagated in zonal mean temperature and zonal mean zonal wind profiles taken from ECMWF analysis. The authors examine the relative importance of turbulent vs radiative damping effects and source variability. The conclusions that they reach on the minimum height at which the spectrum appears to be dominated by Kelvin waves may depend on the technique.

I consider the paper contains interesting results and is suitable for publication provided

that they address the mayor comments given below.

Major comments

1. The authors claim to use a ray tracing technique for Kelvin waves, however there is neither horizontal propagation nor horizontal refraction so that ray tracing equations are not needed for this problem. As far I can see the only equations that are needed for the wave amplitude calculation are the wave action conservation equation and the dispersion relationship. Is this right?. In that case I suggest to change for a more appropriated terminology.

2. One of the assumptions that the authors are taking in the wave amplitude calculation is a zonal background wind that is uniform, in both zonal and meridional coordinates within the analyzed latitudinal range (this assumption must be clearly stated in the manuscript, see Minor comment (1) below). This could be an acceptable first order approximation in the stratosphere, but this assumption is not valid in the troposphere. This will pervade the conclusions taken for the minimum height at which the 'spectrum' is dominated by Kelvin waves.

3. The authors determine a minimum height at which the Kelvin waves are propagating freely since at lower heights the spectrum that they consider as Kelvin waves is contaminated with non-Kelvin waves. I think the only conclusion from their results is that the technique they are applying to identify the source spectrum is not valid below a certain height, other techniques that use other ways to identify the spectrum of Kelvin waves may reach a different conclusion. Furthermore the conclusion may depend on the data (e.g. zonal wind spectra vs pseudo-zonal wind spectra).

Minor comments

1. I agree that the wave amplitude calculation used for gravity waves in the works cited by the authors (Marks and Eckermann 1995, Eckermann and Marks, 1997) may be used as a first order approximation for planetary-scale waves but there are some

concepts and hypothesis that change their meaning. These must be stated clearly in the work.

2. The work assigns a part of the atmospheric spectrum to Kelvin waves, an examination of the polarization relations for ECMWF zonal wind and ECMWF temperature spectra may be useful to argue that those atmospheric disturbances found in the spectrum are indeed Kelvin waves. For example, the authors could compare ECMWF zonal wind spectra with pseudo-zonal wind spectra derived from ECMWF temperature (eq. 8).

3. The motivation to include SABER data is not clear in the paper. There is already a previous work that compares equatorial waves for SABER data and ECMWF analysis (Ern et al. 2008). In the present paper SABER data do not appear to add much to the conclusions.

4. Fig. 5 shows maximum Kelvin wave amplitudes are located at higher altitudes for the wave amplitude calculation at a fixed time (e.g. Jan 2004). Do the authors think that the differences are due to a poor radiative damping representation? Perhaps a comparison of ECMWF spectra and spectra from the wave amplitude calculation for a 36 h time window could be shown.

Technical comments

1. Section 2.3 Line 18. Are SABER data used in Section 5?

2. Section 6 Line 18. I suggest to refer as '*differential* radiative damping' to the process responsible of the shift in the spectrum. Furthermore, the conclusion might give credit to the work by Garcia and Salby (1987) who first suggested that process.

3. Section 3.3. What is the wave breaking limit that the authors are using?

4. There are a number of panels and figures that do not appear to add much information. Why do the authors show panels a and b in Fig 10-15? They have already shown in a previous section that the considered spectrum was contaminated with non-Kelvin

waves. This appear to support the Major comment (3) that the inferred minimum height will depend on the variable used to determine the source spectrum.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 13039, 2009.