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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.

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General Comments:

This is a very interesting paper that nicely demonstrates the utility of SABER data to document the gross statistics of larger scale equatorially-trapped modes and (perhaps) gravity waves in the stratosphere. As a non-specialist in stratospheric dynamics, I learned a lot from this well-motivated and nicely organized paper. I recommend that this paper be accepted with minor, but important, revisions that could be carried out by changing some wording and perhaps a figure or two here and there.

I have three substantial concerns. One has to do with distinguishing between the various "free" and the "convectively coupled" (here called "tropospheric") waves more

clearly, a distinction discussed by Randel and Wu (2004) in their Introduction. As an example, the stratospheric Kelvin waves discovered by Wallace and Kousky are for the most part "free" waves, and generally have relatively fast phase speeds of 20-30 m/s. These and various species of stratospheric inertio-gravity and MRG waves are considered to be forced primarily by convection and other sources such as orography, but in general once launched they propagate uncoupled to convection. As shown by Chang, and Salby and Garcia, these waves have preferred vertical scales determined by the depth of the heating, and they have correspondingly higher equivalent depths and subsequently faster phase speeds. In my opinion such free modes should be carefully distinguished from the stratospheric response to convectively coupled waves as documented by Wheeler and Kiladis (WK), Straub and Kiladis, etc.; these differ in that they move along with the at a phase speed determined in part by processes involved in convective coupling (e.g. the gross moist stability). For example convectively coupled Kelvin waves propagate at around 15 m/s, and so does their stratospheric signal, much slower than the speed for the free modes. As pointed out by the authors in linear theory the vertical scale of a given wave will vary depending its the equivalent depth (and thus its phase speed), as governed by eq. 3, which will in turn depend on whether such a wave is "free" or coupled to convection for an extended period of time. So, as alluded to in Section 4, I believe that the authors are considering here a combination of signals due to both convective coupling (especially in the lower stratosphere, where these signals are still large), and "free" modes. I suggest treating these issues with some comments in both the Introduction and in Section 4.

Another related concern has to do with the use of a 90 m equivalent depth "cutoff" to separate so-called "tropospheric" waves from the faster modes. At especially 21 km in Fig. 2, this curve cuts across the peak of both Kelvin and MRG signals, and for the reasons stated above I do not believe that the bulk of the power in these spectral peaks are necessarily associated with convectively coupled waves treated by KW, they more likely represent the free modes discovered by Wallace and Kousky and Yanai. It makes more sense to define the cutoff at a spectral gap rather than along the peak. So I sug-

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gest rethinking the motivation for this cutoff, and suggest using filters that corresponds more closely to the observed peaks at a given altitude. Since the equivalent depths vary with altitude, one solution would be to use only one broad filter (8-2000 m) and combine Figs. 2-3 and 5-8, but I leave it up to the authors to decide. In any case, I believe that the 8-90 m filtering for waves at 21 km should be reconsidered.

Finally, it seems to me that the gravity wave signal analyzed in Sections 3.2 and 4.2 may not be cleanly separated from the Kelvin wave signal (see below).

Specific Comments:

pg.11688: "The tropospheric values can be attributed to the vertical scale of convective processes"..it is probably more correct to say "attributed to the convective coupling " As discussed above it is likely the nature of convective coupling that determines the phase speed and therefore the vertical scale of the waves discussed by WK, whereas in the case of "free" waves it is the vertical scale of the heating.

Pg. 11689: "these analyses cover the source processes of the waves observed at higher altitudes" Actually based on the arguments above I would say that WK only discuss the largest scale, lowest mode convectively coupled waves which account for only a relatively small fraction of the free waves launched by "background" convection not organized into such waves.

Pg. 11693: Although it becomes obvious later, it should be stated here that the color scales for Figs. 2 and 3 are different for symmetric versus antisymmetric. I first thought that the antisymmetric spectrum had much more background power, but this does not seem to be the case.

"This means that the general spectral features..in the troposphere are also present in the stratosphere" This statement might be interpreted to mean that they are the same waves, I would not state it this way, since a large portion of the faster stratospheric signals are likely independent of the tropospheric waves studied by WK.

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pg. 11694: are the symm/anti background variances roughly equal?

Are SABER and ECMWF data completely independent, i.e. data from the SABER instrument are not assimilated into the EC analysis? In either case this should be stated.

Pg. 11695: just to clarify: SABER residuals are calculated from the zonal mean, but here you are using temporal residuals? This is not quite clear.

There is a strong, time mean component spread through the lowest wavenumbers in the antisymmetric spectra of Figs. 2 and 3 that the authors do not discuss. Part of this signal cuts across the ER band, but there seems to be equal variance on the eastward side too, perhaps it's some sort of standing component? Do the authors have an explanation for this signal?

Bottom: I do not understand the argument that the EC "background is spread over a much larger spectral area". For the same space- time regions both SABER and EC should have the same backgrounds if all were equal. So it seems to me that EC is simply missing some variability here, as the authors conclude next. One thing I wonder, if the EC sampling were decimated to replicate the SABER sampling, would their backgrounds be more similar? In other words, do the authors have an idea of what the overall effect of SABER sampling might be on variance? I suppose if anything it would be to decrease the overall variance, but I'm not sure.

Pg. 11696, bottom: "the average peak Kelvin signal" is this for only the one wavenumber-frequency bin that has the highest value?

Pg. 11697: Once again, in Fig. 4 does this represent the one bin with the highest value?

Pg. 11698: What kind of windowing is used to produce the time- variance plots of Figs. 5-9, are these just the same 31-day windows used to obtain the spectra?

Pg. 11699: For those (like me) who are less familiar with the SAO, it might be good to

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distinguish this signal in the text from the annual cycle and QBO in Figs. 5-9.

Pg. 11700: I am not sure what you mean by a "median" value, is this over all 31 day segments or bins? Please elaborate on the technique here. In any case, I am concerned that the estimate for what you are calling gravity waves is still contaminated by Kelvin and other eastward modes, there is overall more eastward power beyond wave 3 in Figs. 2 and 3. Maybe it's because I don't quite understand the "median" technique, but it seems that you haven't effectively isolated the background here, since the pattern in Fig. 9 looks suspiciously similar to those in Figs. 7a and 8a, which both contain an annual cycle and downward propagation related to the QBO. Also, don't the QBO and SAO relationships in Fig. 9 imply that if you have indeed isolated gravity waves they have a preferred eastward phase direction? I don't understand why this should be so, especially since it is suggested that Wu and de la Torre did not completely remove Kelvin waves either. So if the authors really wish to be convincing that Fig. 9 reflects primarily gravity wave activity, I suggest some more detailed discussion of why the authors think that they have isolated just gravity waves (maybe they need to clarify what their definition is?), and why eastward waves should be systematically favored, otherwise I suggest eliminating this Figure and the accompanying discussion.

Pg. 11701: at the start of section 4.1, already at 21 km the equivalent depths associated with the waves are substantially higher than in the troposphere, and this discrepancy becomes greater with altitude, so I don't think it's quite correct to say that your results are "similar" to WK and Cho et al.

Pg. 11703 line 4 from bottom: Since the Preusse et al. 2006 analysis concerns only one month, this statement is confusing.

Pg. 11705: I assume "91-day analysis windows" refers to the length of the series used for spectral filtering?

As I understand it EC stratospheric data assimilation are heavily dependent on satellite measurements. The comparison between SABER and ECMWF is remarkably good. If

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EGU

these data sets are truly independent this should be pointed out here and also at the start of Sections 2.3 and 6.

Pg. 11706: "..one reason why residual temperatures are maximum at.." I do not follow the logic here.

Technical Corrections:

Pg. 11689, bottom line: "in the stratosphere" used twice.

Pg. 11690, middle: "some kind of" suggest "a" instead.

Pg. 11692, top: I suggest saying something like "and each averaged over a latitude band (e.g. 15S-15N) after first calculating the power at each individual latitude".

Pg. 11694: a minor point, but you are using pressure level ECMWF data yet results are plotted by altitude, how is this conversion made?

Pg. 11696: suggest "This likely reflects the increase of the amplitudes"

Year is missing on Preusse et al. 2006 reference.

Krebsbach and Preusse reference should be 2007, not 2006 throughout.

Signed,

George Kiladis

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