

Interactive comment on “Planetary waves in a coupled chemistry-climate model: analysis techniques and comparison with reanalysis data” by F. Mager and M. Dameris

Anonymous Referee #1

Received and published: 24 May 2005

The paper compares spectral properties (wavenumber frequency analysis) of planetary waves in a coupled chemistry-climate model with ERA-15 analyses as an attempt to pin down model deficiencies. This work is considered important, because the overall performance of such a chemistry-climate model is believed to be crucially dependent on a faithful representation of the dynamics in general and the planetary waves in particular. For this reason, a study such as this is well worth a publication.

However, I feel that the paper needs to be improved substantially before it can be published in ACP. Overall, the paper is not well written and I found a fair number of in-

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consistencies. The standard of the English should be improved. More importantly, the authors appear to be rather casual when interpreting their results. There is a substantial body of theory regarding planetary waves in the atmosphere and their interaction with the mean flow. The interpretation must take this carefully into account. In my view there is too much guess work and speculation. A sound interpretation of the presented analysis is a necessary prerequisite to identify the underlying model deficiencies, which (after all) is the declared goal of this work.

Below, I give examples where significant improvement is possible and highly desirable.

* Abstract: "The paper presents several analysis techniques...". This is somewhat misleading. The paper really USES analysis techniques. The term "PRESENTS" suggests to me that the authors actually invented the techniques (which is not the case).

* Abstract line 19/20 and several occurrences in the text: "heat flux... warms the pole": For linear conservative waves the effect of the eddy heat flux is exactly compensated by the effect of the Eulerian mean meridional circulation. So if there is a poleward heatflux, this does by no means imply polar warming. Instead, the argument should consider the EP flux and its divergence. Only to the degree that the EP-flux at the tropopause (TP) is refracted into the polar vortex at higher level can the heat flux at TP level (= the vertical component of the EP flux) be taken as relevant for the polar temperatures.

* Section 2.1.1, "For convenience we will use ω instead of $\Delta\omega$..."(page 2564 line 10): Well, ω is a frequency, $\Delta\omega$ is a frequency interval. This should be kept apart. It is actually ω (not $\Delta\omega$) which is needed later on.

* The paper talks about "standing waves", "stationary waves" and "quasi-permanent waves". What is the difference? If you mean the same thing each time, it would be better to use just one term and stick to that.

* similarly: "travelling waves", "transient waves", "eastward oriented modes"...

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* similarly: "wave amplitude", "wave variance"

* section 2.1.2: What is considered "noise" in this context (page 2566 line 10)?

* Equation (15): A formulation which gives amplitude as a function of pressure is not very suitable to show that the amplitude "increases exponentially with height" (page 2567 line 15,16). In figures 4 and 5 it would help to point out that both the pressure and the amplitude axis are logarithmic, such that one obtains a straight line if amplitude is ALGEBRAIC in PRESSURE.

* page 2567, lines 19-22: The statement in this section appears very strange to me. The amplitude behaviour from equation (15) refers to the external mode in an isothermal atmosphere. But in the real atmosphere there are other types of waves, too. For these other, non-external waves one does not expect a behaviour such as (15). For instance, in case of upward propagating undamped waves the increase in amplitude is stronger than (15). Thus, it is not clear to me why a deviation from the behaviour (15) necessarily implies conversion from available potential to kinetic energy.

* The index of refraction eq. (17) is taken as a measure for vertical propagation. Later in the section there are a few rather obscure sentences (page 2569, lines 4-9) suggesting that this interpretation is not really sound. At the end the authors quote the work of L. Harnik who developed a more refined diagnostic for vertical wave propagation. Why do the authors not use such refined diagnostics?

* Section 5.1: ".. sum over frequencies..." (page 2572, line 24): this implies that it contains both stationary and travelling waves. But the next sentence (same page, line 25) says that this measures travelling waves only. This seems contradictory.

* "Rossby waves come into being through conservation of PV" (page 2573, line 3): Taken at face value this is wrong. There may be conservation of PV but no Rossby waves! Instead, Rossby waves are generated by mechanisms such as flow over orography, instability etc.

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* "Ultralong waves come into being... at high latitudes..." (page 2572, line 6): In the NH the dominant mountains are Tibet and the Rockies, which are definitely NOT high latitudes. So this is a very poor (probably wrong) argument. "...shorter waves have the tendency to be refracted towards regions with a higher refractive index...": this argument applies equally to waves of all wave numbers, i.e. all waves have the tendency to be refracted towards regions with a higher refractive index.

* In various instances the agreement between ERA15 and the model is contended to be quantitatively good (which in my eyes is not always true). But later in the interpretation the quantitative differences are evoked as significant. This appears inconsistent.

* Section 5.1: "This is most probably due.... (Figure 6)" (page 2573, line 6) reference to the wrong figure, it is probably Figure 7.

* "Stronger subtropical jet ... induces higher shear instability..." (page 2573, line 6): What kind of shear instability? What is the basis for this speculation. A reference may be helpful.

* "wave activity... seems to propagate" (page 2573, line 9): Well, this cannot be inferred from the plot. It only shows different amount of wave activity at different levels. Direction of propagation cannot be inferred from this alone.

* "low pass filtered modes that have been forced by the stronger model jet" (page 2573, line 10-11): I plainly do not understand this.

* "eastern stratospheric winds..." (page 2573, line 14): easterly?!

* page 2574, line 14: it would probably be better to refer to "intrinsic phase speed" rather than only "phase speed".

* Discussion of figure 4: "characteristics of individual components were found to correspond closely to the sum" (page 2575, lines 20-22): This needs an explanation, because theory suggests otherwise (theory suggests a different vertical behaviour/decay for different horizontal wave numbers).

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- * A higher tropopause is given as a reason for increased baroclinic activity (page 2576, lines 17-20). Two sentences later the work of Lindzen is quoted in which it was argued that the opposite is true. The authors do not even try to resolve this contradiction.
- * Discussion of figure 5 (page 2577 line 7): these modes are better described as equivalent-barotropic rather than barotropic.
- * Section 5.2, page 2577, line 17-23): The modeled NH polar vortex is stronger than observed. This is explained by weaker wave 2 and 3. At the same time it is pointed out that wave 1 is stronger. So it is not clear without further analysis whether or not the increase in WN1 overcompensates the decrease in WN2 und WN3 (especially since WN1 is the strongest wave).
- * "Stationary waves forming in the troposphere..." (page 2578, lines 8-13): Most of this section is well known and should be acknowledged by references.
- * "signal seems to travel into the stratosphere where it is enhanced by the stronger model westerlies" (page 2578, line 29): this contradicts the work of Chanery-Drazin, according to which stronger westerlies imply reduced vertical propagation.
- * page 2579, lines 12 ff: "GCM's with sponge layers are susceptible to an imposed local force or diabatic heating...": (what does this mean?) "... they absorb upwelling waves realistically..." (certainly not) "... without causing a direct feedback on the dynamics below" (this statement, too, I would not trust).
- * end of section 5, page 2581, lines 1 and 2: "This interpretation suggests an interaction between transient and stationary waves through the transport of sensible heat." This is not clear to me.
- * conclusions, page 2581, lines 23, 24: ".. it has been shown that the forcing and propagation of [stationary waves] is determined by the strength and sign of the zonal wind": I think this has already been shown by Charney and Drazin in 1961, I certainly would not consider this as a result of the present paper.

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* end of conclusions, page 2583, line 2,3: "less idealized orography... " [in what sense is the orography used "idealized"?] "... should improve the orographic forcing of stationary waves...": I believe that wavenumbers 1 through 8 should be represented faithfully at T39 used in the model runs. So it is not clear to me to what degree the resolution of orographic small scales is a problem here.

The above is not an exhaustive list, these are only examples of sloppiness and carelessness. The substance of the work justifies a publication, but the presentation and interpretation must be improved significantly throughout the manuscript. If the revision were restricted to a slight rewording of the sections mentioned above, I would consider this as insufficient and recommend to reject the paper. Also, I am not willing to review this manuscript one more time.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2559, 2005.

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