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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 #2

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This paper compares the structure of planetary waves between the coupled chemistry climate model ECHAM4.L39(DLR)/CHEM (CCM E39/C) and ECMWF reanalysis (ERA-15). The model's horizontal resolution is T30. The model has 39 layers and a model top at 10 hPa.

As summarized in the conclusion (first paragraph), the main goal of this paper is to investigate the ability of the coupled CCM E39/C to simulate planetary waves and to get a detailed insight into the forcing, propagation and dynamic effect of these long atmospheric modes. The main wave features shown and discussed are variance of

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geopotential heights of individual waves (1 to 8) at 500, 150 and 50 hPa, variance distribution and vertical structure of transient waves, amplitude and phase of normal modes, geopotential height amplitude at 50hPa, and latitude-height cross sections of both the variance of stationary waves (sum over wavenumbers 1 to 8) and the zonal mean zonal wind. In addition, refractive index for wave number 1, and the differences of geopotential height and heat flux between model and reanalysis are shown. All results are given for DJF. Zonal mean features are often discussed both for Northern Hemisphere winter and the Southern Hemisphere summer.

The authors conclude that the tools used are well suited to investigate and estimate the impact of various dynamic processes related to large scale waves (abstract, last sentences). The authors found a good agreement of variance distribution and vertical structure of transient planetary waves. It is suggested that quantitative differences are due to horizontal model resolution and modeled zonal wind. It is also argued that, in the model, a reduced heat flux is the main reason for a too strong stratospheric polar vortex and consequently an overestimated stationary wave 1.

I cannot suggest this manuscript for publication. In the following, the main problems are described and suggestions for further studies are given:

1. The authors show very selective parameters and do not provide a systematic comparison of wave features between modeled and observed waves. Therefore, the results shown do not allow to achieve an overall understanding of the wave behavior in the model. Only very limited conclusion can be drawn about the forcing, propagation and dynamic effect of waves. In my opinion, a systematic comparison needs to include latitude-height cross sections of amplitude and phase of stationary waves, heat and momentum fluxes for transient and stationary waves, information about the propagation characteristics, and EP-flux divergences. The authors discuss often differences between Northern Hemisphere winter and Southern Hemisphere summer. Here, a comparison between Northern and Southern Hemisphere winter will be more appropriate. A serious comparison of Southern Hemisphere summer between model

ACPD
5, S1113–S1116, 2005

Interactive Comment

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and reanalysis would be interesting by itself because of the well known problems with simulating a realistic zonal wind in the lower summer SH stratosphere.

2. The authors conclude that a model top at 10 hPa is not the reason for a too strong polar vortex and an overestimated wave 1 amplitude in the stratosphere. This conclusion is based on the results of the index of refraction for wave number 1 which is similar between the model and reanalysis (Figure 8). I cannot agree with this conclusion. Effective absorption of vertically propagating waves (especially wave 1) through the use of a sponge layer is difficult because vertical wavelengths involved are extremely long (˜100 km). In the real world, waves 1 propagates unhinderedly into the stratosphere. According to the similarity in the index of reflection between reanalysis and model, this seems to be the case also for this model. The fact that both the amplitude of wave 1 and the polar vortex in the model are too strong are more likely due to the fact that planetary wave activity is trapped in the lower stratosphere. These dynamical features in the model stratosphere seem consistent with previous findings of the effect of the upper boundary (e.g. Boville and Cheng, 1988, JAS).

The authors need to provide a more complete investigation of the effect of the model top and the sponge layer when discussing waves in the stratosphere. Are the waves damped effectively in the stratosphere? What are the consequences? We know that when a very strong damping of waves in the stratosphere is applied, the stratospheric results cannot be trusted. And, papers suggest even an effect on the troposphere (e.g. Shindell et al 1999). Or, is the damping too weak and waves get reflected back into the troposphere from the model top. What are the consequences in this case?

In summary, the manuscript would benefit strongly from a more systematic comparison and a critical discussion of the results. Such a study would be a valuable contribution because the model has been widely used. The authors should discuss whether the model can be used for stratospheric chemistry/climate interactions. What are the consequences of a too strong vortex and too strong wave 1 amplitude for modeling anthropogenic climate change conditions? Do we have to interpret with caveats the results of other studies using this model?

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