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Atmospheric Chemistry and Physics Discussions

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 #3

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General Comments:

In this paper the authors use a variety of wavenumber-frequency spectral analyses (WFA) to diagnose the planetary wave behaviour in a T30L39 chemical climate model (E39/C) and validate this behaviour against ERA-15 reanalysis data. This is an interesting an worthy pursuit. Far too often, coupled chemical-climate models (CCMs) concentrate too exclusively on the chemistry with little reference to the properties of the underlying dynamics. As the authors point out, the interactions of chemistry and dynamics are manifold and poor dynamics (e.g. transport) can easily undermine the

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validation of chemistry packages.

The authors' direct comparison of the WFA of E39/C against equivalent WFA of ERA-15 reanalysis data is a useful as a validation tool. This simple comparison would on its own provide a means of documenting problems in the dynamics with respect to the reproduction of planetary waves in CCMs. In this regard the study is of interest and suggests a diagnostic that could be employed more regularly. Whether this is enough to justify publication, and publication in this particular journal, is not so obvious. Questions that remain unaddressed are: What if any is the impact of these WFA biases on the chemistry? Are some more important than others?

As the authors point out these are not new diagnostics. Their novelty arises from the insight they may provide in the present application. The interaction of chemistry and dynamics is quite complicated and the prospect of a diagnostic that might speak directly to one or several key connections appears to be what is promised in the Introduction and by the particular journal selected for the paper's publication. However, this paper provides no such insight into the interaction of dynamics and chemistry. The paper focuses on dynamics only. Consequently, I would suggest that this is not necessarily the appropriate journal for its publication.

One of the reasons that the WFA has not been used more widely is that its value is not obvious. Given that this study focuses exclusively on dynamics, an important question is then whether the WFA has provided a new or a deeper understanding of the model dynamics. I'm not sure that it has. Instead of the WFA providing say a new explanation for a known biases in the model response, most of the papers seeks to explain the planetary-wave biases revealed by the WFA as consistent with known biases of the model response. Granted these are only diagnostics and consistency relations are all that can be expected, but the authors are in a position to test some of these explanations and this would provide new insight.

For the reasons outlined above, I would recommend that this paper not be published

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in the journal of Atmospheric Chemistry and Physics, but rather in a more dynamical journal that is better suited to its subject matter. If the authors decide to pursue this approach, then I have some suggestions as to how they might improve their analysis.

Specific comments:

Sources of WFA biases relative to ERA-15:

1) It is suggested that low horizontal resolution is an important source of WFA biases in E39/C relative to ERA-15 (i.e. reduced tropospheric transient wave activity) and that a number of improvements might be realised if higher resolution were employed (e.g. better orographic planetary-wave forcing). While this seems plausible it is also expected (e.g. Senior 1995 reference). The authors make no effort to validate this claim or explore this issue. This sort of follow up would be new and helpful to the community.

The authors are in a position to use the WFA to test this explicitly and also comment on how much resolution may be required. The same model used in this study has been extensively exercised over the past few years for the IPCC Fourth assessment. A variety of runs (e.g. "Climate of the 20th Century") employing a T63, 31 level version of the model used for the current study have been available for some time. A WFA performed on this run (and perhaps one of intermediate resolution) would explicitly validate the authors explanation and possibly identify a minimum horizontal resolution required to obtain reasonable levels of transient wave activity. This would provide a new and useful application of the WFA and provide valuable information to the community.

2) It is also suggested that many biases in the WFA are consistent with documented biases in the zonal-mean seasonal-mean basic state. Are the planetary-wave errors the cause of the basic-state biases or are they an effect of the basic-state biases? If latter were true and the source of the basic-state wind biases were alleviated (e.g. through improved physics), then many of the planetary wave errors would also be corrected. If the former were true, however, one would have to focus on mechanisms of planetary-

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wave forcing in the model to correct the basic-state biases. Trying to sort out which is true is a valuable exercise.

The authors should be able to use the WFA to make some informed comment in this regard. For example, there must be a number of applications of E39 (i.e. no Chemistry). I do not have access to Land et al. (1999) but surely the model wind biases look different than Fig.7 when the model is run in AMIPII mode with prescribed O3 and greenhouse gas forcing. A WFA of these previous runs could identify more clearly the impact of these changes to the basic state on the planetary wave errors.

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