

Interactive comment on “Hemispheric ozone variability indices derived from satellite observations and as diagnostics for coupled chemistry-climate models” by T. Erbertseder et al.

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Received and published: 31 July 2006

I note firstly, that this is an independent review: I have not read the first reviewer's contribution.

The paper produces harmonic analyses of model and observed total ozone and constructs indices based on the wavenumber 1 and wavenumber 2 components. The paper is generally well-written, except for the conclusion, with good English, which is a credit to the non-native speakers. Also, the paper appears to be free from scientific errors and is comprehensive as far as it goes. Therefore I recommend that the paper is

accepted for publication in ACP, subject to response to the specific comments detailed below.

Overall, however, I think that the paper would benefit from a stronger justification for the diagnostics presented. As it is, the paper comes over as an exercise in harmonic analysis and we learn little that is new about the atmosphere or the model being used. The concept of a core diagnostic is introduced in the abstract presumably in the technical sense of Eyring et al. but without the reader being made aware of this. Such a concept should be better placed in the Introduction. Overall the paper would be made more rewarding to the reader if the facts of the model bias could actually be interpreted clearly in model terms and give rise to a strategy whereby the model could be improved. One all pervading issue is that paper does not make it clear whether the diagnostics are being used to validate chemistry or dynamics, which is perhaps related to the lack of a strategy for moving the results forward. Why not simply validate the planetary wave dynamics against reanalyses? The latter may be more reliable than total ozone for climatologies. I also have a general question regarding the use of harmonic analysis. Obviously, this was the tool at the disposal of the authors, but given what they are trying to achieve, wouldn't EOF analyses be more appropriate, as they are more physically based?

If the purpose of the paper is neither to validate chemistry nor dynamics, but some subtle combination of the two, then the diagnostics are no longer core diagnostics as I understand the concept. The difficulty with the paper is that without a problem to solve, there is no clearly superior technique. Another problem with inventing diagnostics is the logical continuation of the work. First use diagnostics to identify a problem (for example the CH₄ problem that I raise briefly below) then fix it. While the E39/C architects do not follow through with their own philosophy, they undermine this strategy and the model stagnates.

Specific Comments

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Comment

p.5672, l.7. Here and elsewhere, 'hemispherical' → 'hemispheric'.

p.5673, l.2. This clearly seen in the results of Eyring et al. '06 without developing a specific diagnostic.

p.5673, l.11-14. I don't see how it can be treated as a core diagnostic, for reasons given above.

p.5674, l.8. Here, and elsewhere, WMO (2003) is too vague a reference and is an insult to the reader. Please specify the source material or the Chapter if it is a new result.

p.5674, l.19-20. Replace by 'The latter indicates the significant influence of chemistry on ozone.'

p.5675, l.20-21. 'The strength of planetary ...' is a somewhat meaningless sentence. Depending on how 'strength' is defined can give a different answer.

p.5675, l.22. Why not use meteorological fields to determine planetary waves?

p.5675, l.23-26. Earlier (p.5675, l.1) it was noted that total ozone traced the transport processes. So, which is it, and on what time scales?

p.5676, l.11. TOMS footprint may be small, but there are still only about 14 orbits per day plus side scanning and therefore the resolution is no higher than Met. fields based on satellite data. So, 'considerable' is questionable and in any case subjective. A similar comment was also made in the abstract. p.5677, l.4. Here, in the polar night, met. fields are superior since satellite data do exist.

p.5678, l.7. It is not clear which was the previous model version which has been improved.

p.5678, l.10. The rates are out of date, and some comments of the recent revisions are in order.

p.5680, l.13-15. This may be true mathematically, but do the authors have any evidence that the harmonic analysis gives any more insight than a straightforward Fourier analysis?

p.5681, l.1-3. Their could be the start of some discussion here, but the text says little more than suggesting that a CCM should reproduce what occurs in the atmosphere.

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This is hardly new.

p.5683, l.20-23. While it is important that diagnostics show some robustness to atmospheric variability, the dilemma is that for a diagnostic to be useful, it needs to show sensitivity to some fundamental model or atmospheric process. This is an issue for further discussion and analysis of model results. Do the diagnostics show any trends, either for the results here or those submitted to WMO (2007) Chapters 5 and 6?

p.5684, l.24-25. This is not really a test of heterogeneous chemistry. It is more of a test of halogen amounts in the lower stratosphere, since T should be well below TNAT and any reasonable scheme will give significant ozone loss.

p.5684, l.27-28. High zenith angle J are not that important for the Antarctic (more for the Arctic). As in the comment above (p.5684, l.24-25), the overall behaviour of the ozone hole is mostly attributable to halogen amounts. Even the heterogeneous chemistry scheme is not that important. See Austin et al. (JGR, 94, 16,717-16,735, 1989) — which incidentally also used non zero J at 90+ degree solar zenith angles.

p.5685, l.6-10. Whatever you look at, if it is controlled by dynamics (climatology, waves etc.) 8 years is likely not enough — see eg. Scaife et al. (QJRMS, 126, 2584-2604, 2000).

p.5685, l.14. models → model's

p.5685, l.28-29. This defensive comparison with another model with a different upper boundary is not relevant to the current work and serves to expose the E39/C model as a tropospheric model. The poor transport characteristics of the model are clear in comparisons of CH₄ in Eyring et al. '06.

p.5687, l.4. Lagrange → Lagrangian.

p.5688, l.11-16. Again, this comment indicates that the issue of dynamical/chemical impacts on ozone are not satisfactorily resolved in terms of what is being tested and what a multi-model comparison would achieve. In the last sentence, 'a more generalised view' of what?

p.5689, l.14. 'month months' → 'months'

p.5691, l.21-24. This is not at all clear. The second sentence says nothing more than an unclear definition of the phrase 'representation chemical processes'.

p.5692, l.13-15. But TOMS is influenced by possible instrument drift, systematic errors from radiative transfer, and differences between satellites over multi-satellite periods. Of course these differences are minimised, as indeed they are in the met. fields.

p.5693, Several points in the text are unclear whether they refer to planetary waves 1 and 2 or ozone waves 1 and 2.

p.5693, l.28 et seq. The low resolution of the model compared with other tropospheric models may also be a limiting factor.

p.5694, l.14-16. Without analysing earlier or later years, how can this short period be considered representative?

p.5691-5695. The conclusion would benefit from a complete rewrite to clarify what is established from this study that has not already been learnt from earlier work.

p.5707. Fig. 6. The errors need further explanation. The 95% confidence interval presumably refers to the population mean. For example, the grey error bars overlap the red curve for the top panels, months 10-12. Try redrawing the error bars so that the position is clearer.

p.5709 Fig. 8. see comment to Fig. 6.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 5671, 2006.

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