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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

T. Erbertseder et al.

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First of all we would like to thank John Austin for his valuable and detailed comments. Thanks also for the useful suggestions for minor changes. We will consider all annotations when creating the revised manuscript. We believe that the paper will strongly benefit from his comments.

In the following we will address all remarks, suggestions and specific comments.

The suitability of total ozone as a tracer for stratospheric dynamics was quantified by Wirth (1993). On a monthly mean basis one can explain 95% of the total ozone variability by wave number one and two. We understand our approach as a logical continuation of these findings. Following the demand for a diagnostic within CCMVal that

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contributes to evaluate stratospheric dynamics we introduced the hemispheric ozone variability indices. We propose this diagnostic as a supplement to other diagnostics defined in Eyring et al. (2005) that allows to get a first-guess on how the dynamics is represented in a model simulation before applying costly and more specific diagnostics. They constitute nothing but a simple hemispheric measure for dynamic activity in the stratosphere. We did not intent to evaluate both, dynamics and chemistry.

At the SPARC CCMVal Meeting in Boulder we proposed the diagnostics "hemispheric ozone variability indices" for the evaluation of stratospheric dynamics. Following some discussion and your comments we see some shortcomings that are not negligible. However, we would like to motivate the usefulness and applicability of our concept:

- Speaking in terms of process-oriented validation we see that a single process like planetary wave propagation can not be isolated by our approach. However, recalling the findings of Wirth (1993) total ozone is suitable as dynamic tracer for planetary wave activity. Thus, the diagnostics we are introducing gains a vertically integrated measure of stratospheric dynamics.

- The representation of planetary wave activity can easily be quantified in model results by comparing it to observed data. However, we are not aiming at delivering a full diagnostic if a model is performing perfectly or not.

- Instead of replacing an existing diagnostic it is understood to be a supplement in the process of evaluating model results.

- The presented diagnostics based on a simple quantity derived from measured data (e.g. TOMS) that is freely and easily available. It is not based on modelled or analysed data like ERA40. It is not contaminated by possible biases.

- Within the process of model result evaluation, we propose a possible application like this:

o First of all one examines climatologies (e.g. monthly means of several parameters) o Then the hemispheric ozone variability indices could be derived before applying costly and specific diagnostics. By doing so, one gets a first-guess and a good insight if the dynamics are represented at first place.

o Then one can continue by applying detailed process-oriented diagnostics We have added a stronger justification for out approach in the introduction.

Another point we would like to address here is that in a first step we aimed at introducing the hemispheric ozone variability indices derived from satellite observations in order to investigate stratospheric dynamics. In a second step we exemplify the use of these indices for evaluating results of coupled-chemistry climate models in general. The application of the indices as diagnostics is then exemplified by results of E39/C (time slice experiment E2000). This is intended just to be a point of start and to establish a basis. Once the indices are introduced with this paper we plan to apply them to results of different CCMs. We hope to contribute to future multi-model evaluations in the sense of Austin et al. 2003 or Eyring et al. 2006. As suggested, a first intercomparison study could also be made with data submitted to the WMO ozone assessment. A further goal will be to analyse possible trends in E39/C or other model data. But this is beyond the scope this paper.

The Harmonic Analysis has been developed over the years by two of the authors. It has been proven to be a valuable, robust and transparent tool for any kind of spectral statistical analysis that is superior to a simple Fourier transformation (see comment p.5680,l.13-15 below). The application of EOFs could be a future task for a more physical based evaluation of the model results.

Of course, it is the main objective of the modeller group to improve E39/C, but it is not trivial and cannot be reduced to single processes as will discussed later. However, the main purpose of this paper is not to improve E39/C, but to introduce a new diagnostics.

Some more details are given below under specific comments

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We have modified the manuscript in order to clarify all the above mentioned points and to strengthen the motivation. This will improve the overall outcome of the paper.

Specific Comments

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

-> Has been changed throughout the text.

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

->Although this has been seen earlier, a possible explanation is given here: planetary wave number one and two deviate significantly in the amplitude. We could further identify a lag in time.

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

-> As stated by way of introduction we see some shortcomings that are not negligible if treated as core diagnostics and neither in the sense of the concept of Eyring et al. (2005). Therefore, we no longer claim to be core diagnostics. Nevertheless we would like to motivate the usefulness and advantages of our supplementary diagnostics.

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.

-> Has been changed. We refer to the regarding chapters now.

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

-> Has been replaced accordingly

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.

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-> Has been changed to: “The activity of planetary waves can be quantified by their amplitudes”

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

-> As outlined above, the suitability of total ozone as a tracer for stratospheric dynamics, i.e. planetary waves, was quantified by Wirth (1993). On a monthly mean basis one can explain 95% of the total ozone variability by planetary wave number one and two. Therefore, we understand our approach as a logical continuation of these findings. The simple measure for planetary wave activity in the stratosphere we present here is based on observed data, with proven accuracy. TOMS total ozone data is a consistent data record that is not influenced by model biases. An advanced retrieval scheme makes it robust to sensor degradation effects. Thus, it is even used for trend analysis which is not a subject here. The data is freely available, small in size and thus easily to handle. Since it is an vertically integrated parameter it easily allows a first check-up of planetary wave activity in model results.

-> As a matter of fact, we already have used meteorological fields to determine planetary waves. There was a paper submitted to ACP (Mager and Dameris, ACPD, 5, 2559-2598, 2005). The paper aimed at comparing planetary waves from reanalysis data (ERA40) with E39/C results. However, the paper was rejected. It was criticized that we compare model results with model results. The meteorological fields represent an analysed quantity only and not necessarily the real atmosphere. They might be too biased.

-> This is the reason why we base our new diagnostic for stratospheric dynamics on total ozone from TOMS. It represents an observed parameter and currently hardly any alternatives to trace global dynamics are available (considering the long data record).

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?

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-> Please see previous point (reply to p.5675, l.22.)

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.

-> In our opinion, satellite observations have an improved spatial coverage compared to ground-based observations. As we consider monthly mean values the daily limitation to 14 orbits is compensated to a certain extent. In fact, the resolution of satellite observations is comparable to that of meteorological fields. Therefore, have replaced the subjective "considerable" by "well suited" (in the sense of "for our purpose")

p.5677, l.4. Here, in the polar night, met. fields are superior since satellite data do exist.

-> We agree that the polar night regions are not covered by satellite observations in the UV/VIS and thus the global coverage is limited. Assimilated ozone observations allow a better coverage, but are not used here due to possible biases induced by the meteorological analyses (e.g. temperature) or shortcomings within the chemical-transport model (e.g. NOx chemistry). As stated above there is hardly any alternative to TOMS when it comes to consistent longer-term records.

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

-> The reference to the previous model version has been added.

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

-> In order to clarify the used model set up including the reaction rates this paragraph has slightly been modified. Instead of exemplifying the diagnostics with the latest transient run of E39/C our results are based on a time slice experiment, which uses an improved model version compared to Schnadt et al. (2002). Information on these improvements has been added. However, due to the application of the time slice experiment data here chemical kinetics are based on Sander et al (2000). The model results are used here more as a demonstrator for the diagnostics than to extract new scientific conclusions.

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?

->Bittner et al. (1994) have shown that the Harmonic Analysis (HA), using an "all step mode", allows one to find a much better parameter vector for the least squares scheme than a Fourier Analysis which is based on an "one step mode". Both methods deconvolute the power spectrum successively. Firstly, the dominating spectral feature is determined and a sinusoid to that spectral feature is fitted. Then the residuals are computed and a second trigonometric function is fitted to the residuals and so on. Contrary to the Fourier Analysis within the HA all previous determined spectral components are fitted again with each step, iteratively. This is a superior procedure because the resulting linear combination of sinusoids turns out not to be unimodal when fitting the data.

-> The HA does not allow leakage or anti-aliasing effects. Therefore, the data may be irregularly spaced in time or space, i.e. no equidistant input data is needed.

-> The HA is defined for short times series while the Fourier Analysis, mathematically speaking, is defined for infinite time series only.

p.5681, l.1-3. There 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. This is hardly new.

-> By means of our modifications following the suggestions, we hope that we could clar-

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ify our concept and better motivate our intension. Despite the fact that this statement is hardly new, referee 1 highlighted this as one of the key statements of the paper.

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?

-> The diagnostic is sensitive planetary wave activity (via the amplitude as traced by total ozone). If a population of this quantity is examined it is not biased by a single anomalous year (e.g. like the ozone hole split 2002). I.e. the September 2002 does not produce an outlier. Robustness on the one hand side and sensitivity on the other hand side can be shown by comparing two or more periods. In the paper we present 1996 to 2004 and 1978 to 2004. We also compared 1978 to 1993 with 1996 to 2004. In principal, the periods show similar statistical moments. However, there are small deviations in the statistical moments. The statistical significance of these deviations in term of trends, however, has not been analysed yet. In general, trend analysis of the presented indices is beyond the scope of the paper and will be a future task. As suggested, a first intercomparison study could be made with data submitted for the 2006 WMO ozone assessment.

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.

-> Since chemistry is not addressed in the paper this paragraph has been deleted.

p.5684, l.27-28. High zenith angles 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.

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-> Paragraph has been deleted. Please see previous point (p.5684, l.24-25.)

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).

-> We have clearly stated that the period is not long enough. But there is hardly any alternative of available total ozone data regarding climate conditions for the year 2000. This problem will be solved when analysing results from transient runs instead of those from time slice experiments.

p.5685, l.14. models -> model's

-> has been modified accordingly

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.

-> In our opinion, we do not refer to another model here since MA/ECHAM belongs to the ECHAM family as SOCOL does. We mention this point since the positive bias in total ozone cannot fully be explained by transport problems of E39/C or a too low upper boundary layer. The positive total ozone bias constitutes a general problem of the ECHAM family. Despite MA/ECHAM having the upper layer at 0,1 hPa, it shows an even increased total ozone bias. Hence, this proves that this bias is not a primary effect of the upper boundary. It was noted that, following Eyring et al (2006), the transport is too diffusive (as seen in CH4) and this is a key problem to solve. In fact, for SOCOL a less diffusive transport scheme was implemented that shows indeed more realistic CH4 distributions. However, the positive total ozone bias is still evident. We conclude there is still no solution to this problem and therefore we would like to mention the ozone bias in this paper very briefly.

p.5687, l.4. Lagrange -> Lagrangian. -> Has been changed accordingly

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?

-> As discussed above, the focus of the paper is on a dynamical aspect i.e. activity of planetary waves. Generalised view means the reduction in dimension in this context. We reduce several latitudinal (zonal) values to one hemispherical. Before applying costly and specific diagnostics one can analyse this parameter which gives a good insight if the dynamics is represented at first place.

p.5689, l.14. 'month months' -> 'months'

-> has been changed

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'.

-> This topic has been clarified. Please refer also to the adapted motivation and the reply above (p.5675, l.22. and p.5688, l.11-16)

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.

-> As already pointed out above, TOMS is an excellent data source. TOMS total ozone data is derived by a state-of-the-art retrieval and is used for detailed trends analysis, too. This, however, is not performed here and thus we think that TOMS is a valuable source for our subject.

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

-> We have revised the conclusion accordingly to clarify this point.

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

-> This point has now been considered and added to our conclusion

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

-> We have compared the statistical moments of different periods (1978-2004, 1978-1993, 1996-2004). As expected small deviations in the statistical moments for the different periods can be found. Their statistical significance in terms of trends has not been investigated yet and is a crucial future task. Examining figure 1 there is no possibility to say something about an obvious trend. So we assume there is no trend and have added this assumption in the paper. We assume stationarity and consider the period as 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.

-> Conclusion has been rewritten.

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.

-> Figure 6 has been redrawn and it is now noted that the difference of the population means is tested for significance (at the 95% confidence interval) .

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

-> Figure 8 has been redrawn. Please see reply above (p.5707. Fig. 6).

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