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**Geoscientific
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Discussions**

Interactive comment on "Attribution of ozone changes to dynamical and chemical processes in CCMs and CTMs" *by* H. Garny et al.

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Response to Review comments on 'Attribution of ozone changes to dynamical and chemical processes in CCMs and CTMs' by Garny et al.

We thank the reviewers for their positive and constructive comments on the manuscript. Answers to specific comments are given in the following:

Anonymous Referee 1
Garny et al. outline a new method to separate the contributions of chemistry and transport to changes in ozone concentrations in regions of the atmosphere. This is

a well written paper and the method it introduces is likely to be very useful. I would recommend publication subject to the minor alterations noted below.

Minor comments:

p.17, lines 10-12: You state "What is less well known is the finding that transport does not act in one direction only but a considerable exchange of air masses takes place." There are various papers by Neu / Plumb / Strahan discussing this two-way mixing and you should reference one or two of them here.

We thank the reviewer for the suggested references and changed the sentence to the following, citing the review paper by Plumb, 2007:
'However, transport does not act in one direction only but a considerable exchange of air masses takes place through two-way mixing (Plumb, 2007). Here, the tracer flux in both directions across the defined boundaries can be explicitly quantified.'

p.20, lines 10-11: You talk about the RHS and LHS of an equation that has three 'sides' to it. Please be clearer that LHS refers to the middle term in the equation.

True, thanks and we clarified this.

p.21: In Figure 11 you refer to regions by numbers and you do not define which is which. I'd suggest that you use the abbreviations in Table 1 rather than numbers.

Good suggestion and we changed this.

p.21, lines 23-25: Please add a sentence or two explaining the following. Does mass

flux from southern mid-latitudes to the pole also decrease, indicating that the increased Brewer-Dobson circulation mainly causes more downwelling in mid-latitudes, or do ozone mass mixing ratios change such that import of ozone decreases whilst mass flux increases?

Good point, and we added some discussion on this issue. The way to answer this question is to compare the changes in ozone fluxes with changes in the mass fluxes presented in Sec. 3. However, as we do not intend to answer scientific questions in this paper but rather focus on the demonstration on the method, we choose to leave the further exploration of the results to future studies.

p.22, lines 17-20: A stratosphere resolving model is not just desirable for correctly simulating the upper part of the ozone layer, it is essential for correctly simulating stratospheric transport.

True, we exchanged the words 'would be desirable' with 'are necessary'.

p.23, lines 18-21: When applying your method to different models, do you not need to take into account the different tropopause heights and different polar vortex sizes in the models? You do talk about using dynamical boundaries but I think that is particularly relevant here.

Again it depends on the question asked as to which method (fixed or dynamical boundaries) is favourable (as discussed in the paragraph above and in Section 2.1). If the cross-barrier transport is to be examined, dynamical barriers are indeed necessary. However, the fixed boundaries allow the direct comparisons of ozone values and their sink and source terms (e.g. Model A has 10% more ozone in the same region as

model B), and a more straightforward interpretation of the differences (e.g. in Production: same conditions in a geographical region; otherwise the regions might be of different size and location and therefore the conditions for production are different). The differences in model dynamics (e.g. size of vortex) will then rather be an explanation of the differences found (e.g. a sink/source term is larger in one model because the vortex is different in size in this model compared to another).

We added a discussion on this issue.

Anonymous Referee 2

This study introduces a new diagnostic to analyze origin, and mass fluxes of ozone in chemistry climate models (CCMs). The tendencies of ozone change are separated into those caused by chemical production and loss as well as by transport from different regions. It is also shown how this method must be applied to analyze the differences between two different periods of simulation. The method is applied to the ECHAM model with the Lagrangian advection scheme ATTILA, however it is formulated such that in can be applied to other models. The paper is well written and contains sufficient new and interesting material. The described mathod is a valuable tool for the interpretation of simulations of ozone. I recommend it for publication in GMD, after the points listed below have been clarified.

1. Initialization of ozone fields and convergence:

It is not clear to me, how the different ozone fields are initialized. It is said, that the ozone diagnostic converges exponentially to the full ozone field, but the timescales must vary with altitude according to the difference in ozone lifetime. What is the convergence time (that is probably needed for a model "spinup")? Please clarify.

The initialization can, in principle, be done in any arbitrary way (see Appendix B), as the ozone diagnostic converges. But the reviewer is correct, the timescales of this convergence depend on the location in the atmosphere; namely they depend on the chemical lifetime and also on the timescale of transport. Therefore, the choice of the initialization is critical for avoiding long spin-up times. If, for example, a middle stratospheric tracer has non-zero concentrations in the UTLS in the initialization, the long lifetimes of ozone there will cause these ozone molecules to remain in the tracer field for a long time. Therefore we suggest initializing the tracer in a 'clever' way (set the ozone concentrations to the full ozone field within the region, but zero otherwise), which should then lead to spin-up times of less than about 2 years.

We clarified the issue in the paper, and added the suggested initialization to Appendix B.

2. Potential accumulation of errors:

Due to gradients of the individual ozone tracer fields and possible numerical diffusion and due to other numerical problems, some scaling and adjustments are introduced (p.6, line 9ff; p.12, line 19ff). If there was an mass flux error of around 10%, it is not clear, whether this would cause a larger error in the mass itself as the errors may accumulate over the period of simulation.

The reviewer mentions two problems here:

1) The 'online' adjustment of the ozone tracers to guarantee that the sum of the tracers equals the full ozone field (page 6). The reviewer mentions an important point in that the errors indeed accumulate over time. In the introduction of the ozone

origin diagnostic in Grewe, 2006, this problem was accounted for and the errors were calculated as accumulated errors. The values we refer to on page 6 of less then 2% in most regions are the accumulated errors, and we clarified this.

2) The numerical discrepancy that arises from the fact that the diagnosed Production and Loss rates, that are saved as model output, disagree with the actually applied tendencies in the chemistry scheme, as these are calculated with a numerical integration scheme (page 12). This is an error in the way the output data are saved, and the 'mass fixing' is applied to the output only and not 'online', so that no accumulation of errors occurs. We also clarified this more clearly in the paper.

3. Figure 3:

The unit (kg) for the top panels is not clear to me. Is it kg ozone per grid box or per m3? And is this the production over the 3-month periods? Please clarify.

Yes, this is the production in a gridbox and over the 3-month period. We clarified this in the Figure caption.

4. Figure 4:

It is confusing that the caption mentions 'northern mid-latitudes (black)' and 'colors follow fig. 2'. Thus one must think that the black line corresponds to the black NH troposphere. Please add 'stratosphere' to 'northern mid-latitude' in the caption and in the relevant locations in the text (e.g. p.9, line 2). Also misinterpretations could be avoided, if the (almost) black color in figure 2 for NH troposphere would be replaced. Are the numbers average mixing ratios over the 'yellow domain' or a different quantity? Or is it for a certain altitude? Please clarify.

True, the caption was not well written and it is clarified what is shown here.

5. Figure 7:

It is not clear to me, why in this schematic the ozone change from the last time step of a month to the first time step of the next month is not recognized. If one would add up the three values for dO3 it would not represent the total ozone loss over the 3 month period.

The reviewer is right in that the schematic does not take into account the changes from the last timestep of one month to the first of the next month, so that the change over the whole three month is not equal the sum of the 'dO3's. We agree with the reviewer that this is imprecise, and changed the Figure accordingly: the correct way is to take into account the last timestep of the preceeding month, which is shown now in the schematic as the value at the transition between the months.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1, 2011.