Atmos. Chem. Phys. Discuss., 5, S5510–S5518, 2005
www.atmos-chem-phys.org/acpd/5/S5510/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The origin of ozone" *by* V. Grewe

V. Grewe

volker.grewe@dlr.de

Received and published: 14 February 2006

1. General remark:

The referees and Heini Wernli gave a lot of constructive comments, which I all tried to include into the manuscript. The main critisism deals with the upper boundary, which is located at 10 hPa in the model E39/C. It has been asked for including results from a middle atmosphere model to make sure that the 10 hPa upper boundary is not dominating the results. This has now been included in the paper in a way that the the focus is still on the E39/C model, since this model provides a possibility to assess the ozone origin from the surface to the stratosphere. The middle atmosphere model results are used to correctly interpret the stratospheric results.

The replies are summarized in one response, since general remarks are overlapping.

2. Overview on main changes to the manuscript:

1. A new section has been introduced between '2 Model description' and '3 Methodology' to give an overview on the abilities and disabilities of the model system with respect to ozone and a special focus on the upper boundary.
2. The middle atmosphere model version MAECHAM4 has been run to inverstigate the importance of the 10 hPa upper boundary condition. The intercomparison concentrates on the part 'Stratospheric ozone origin'. There, two figures were included to show the differences to the 10 hPa version. The text has been adapted throughout the manuscript, e.g. in the model description part.
3. In order to better assess numerical diffusion of the approach an error estimate has been included.

3. Detailed Response to Referee # 1 - 7 November 2005

Major comments:

line 1-15: The referee is totally right that the 2 top layers (10 and 20 hPa) are used as a so-called sponge layers, where waves are damped. Furthermore, the Brewer Dobson Circulation is not totally correctly represented in that model. However, the processes and transport pathways are still included in the model and the question I tried to answer is: What effect has this mis-representation on the results? First I tried to argue (and I further extended it in my first reply and now in the section

ACPD

5, S5510–S5518, 2005

Interactive Comment

'Model validation') that there is a lot of evidence that mean state, variability of ozone and interactions between dynamics and chemistry are represented well enough to perform this kind of transport pathway study. On top of that I tried to speculate what answer a middle atmosphere model would give.

Answer: In order to better understand this issue, I now included a paragraph on the upper boundary condition (3rd par in model description), included a whole section on model validation to show that not only the mean state but also the variability is well represented and performed simulations with the middle atmosphere model MAECHAM4/CHEM. This middle atmosphere model basically confirms the conclusions as formulated in the original document. The pattern of the ozone traces are basically the same in both models (below 10 hPa, of course). The main result that tropical ozone is of less importance at mid and higher latitudes is confirmed. The assumption that E39/C gives an upper limit for that contribution (=50%) is confirmed since MAECHAM/CHEM shows values as low as 30% only. The reasons for this are explained in greater detail in Section 5 2nd par. For a better illustration the discussed pathways are included in Fig.1.

line 15-16: The referee states that he does not agree with mainly the whole discussion section. The argumentation has been split into 2 parts: a) the uncertainty with the methodology itself and b) the performance of the model E39/C.

Answer:

To a)

The methodology is now extended by an error analysis associated with the methodology. This is included in Section 4 and results in two additional tracers for a positive and negative error (eq. 5 and 6). Fig. 3 shows the error, which is in most areas less than +- 2%.

To b)

The additional results from the middle atmosphere model make most of the discussion obsolete, since the results are now included in section 5 (see above).

line 16-21: The referee argues that the results of Fioletov and Shepard (2003) are basically mis-interpreted and used to justify the conclusion that E39/C calculates and upper limit of the contribution of tropical ozone to the extra-tropics.

Answer: The phrasing in the manuscript was certainly mis-leading and is now changed in Section 5 (former Section 4). Neither the life-time of ozone nor the turn-over time was meant to be the topic of the comparison to the findings of Fioletov and Shepard, but the decay rate of an ozone perturbation. However, since the dynamical variability in summer is low, a photochmical ozone relaxation time can be derived from the ozone decline from late spring to autumn, according to Fioletov and Shepard. In Stenke and Grewe (2004) we discussed the lifetime of spring ozone anomalies, which is, of course not totally identical to Fioletov and Shepard's photochemical relaxation time, but to my understanding it is similar enough to be directly compared, since in both in cases a relaxation time is given, which depends on dynamical and chemical processes, less on dynamical, however. The dynamical turn-over time is certainly much smaller than the photochemical turn-over time in both the models and reality. In E39/C a perturbation at around 18 km at mid latitudes has a turn-over time of a little bit more than 1.5 years (paper in preparation), whereas the photochamical relaxation time is in the order of months. In Section 5 the wording lifetime has been specified. This part has been taken out of the Discussion Section, since it is not needed anymore due to the overall changes.

line 21-31: The additional simulations clarify this issue. I decided to additionally apply the middle atmosphere version of ECHAM4 (MAECHAM4/CHEM) rather than ECHAM5.MESSY/MECCA as suggested by the referee, since latter model is not yet runing operationally on our systems and since I believe that for this issue a middle atmosphere model is more appropriate to compare to than just a new

ACPD

5, S5510–S5518, 2005

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

version of the standard climate model ECHAM. Moreover, the chemical module is identical to E39/C, which better resolves the impact of the different dynamics and transport.

Minor comments:

I agree: The lifetime between 10 and 30 hPa is between 10 (10 hPa) and 100 days (30 hPa) this can be found in literature and is simulated also by E39/C. The dynamical lifetime depends on the defined region. The difference in age of air between 10 and 30 hPa is around 1 yr, the tape recorder effect indicates also a 1 year shift between 10 and 30 hPa (e.g. Fig. 7-7 and 7-8 in WMO, 2002). This is considerably longer than the chemical lifetime, indicating that chemistry controls ozone rather than dynamics.

Lifetime between 100 to 60 hPa: Agreed. Text changed accordingly.

Page 9651 13-14 Agreed, text adapted

Abstract: thanks

Figure 7: sorry dashed line as shown in Figure itself.

In the electronic version the figures can easily be enlarged. The line width is increased. The colors are coded acording to Fig. 1.

4. Detailed Response to Referee # 1 - 9 December 2005

line 1-2: Upper boundary condition description is now added to the text.

line 3-7: This passage has been cited from Austin et al., 2003. of course upper boundary conditions and transport characteristics are linked.

ACPD
5, S5510–S5518, 2005

Interactive Comment

EGU

line 8-12: I cannot follow the argumentation. Manney et al. (2005) do not recommend the use of the REAN data set, but I can't find such a general statement on NCEP. Manney et al., also state that the most sensitive diagnostics agree well between the individual observational data sets. ThiiiRrs is of course different for two aspects: the pre-satellite era and the upper stratosphere. However, both aspects are not discussed in my manuscript. Steinbrecht et al. (2005) explicitely pointed out that their findings (page 9212, line 25ff), e.g. concerning stratospheric temperature correlations, do not depend on whether they include the pre-satellite era or not.

line 14: The model used in Dameris et al. (2005) is basically identical to that in Hein et al. (2001), except for some some updates, which are described in detail in the former paper. Most of the updates are published prior and changes discussed and compared to observations. The basic characteristics of the model are not affected. I think the list in section 'Model Validation' is a good description of an evaluation of the models abilities. Perhaps I am misunderstanding the word 'validation'? In the paper Dameris et al. (2005) 9 out of 16 Figures are devoted to evaluation aspects. This includes wind, temperature, ozone, with respect to mean values and variability. Observational data are taken from satellites, combined groundbased and satellite data, in a variety of ways. The observational data are not always included in the plots, but detailed references are given in the paper (e.g. Figure numbers of other papers) and the colour coding and isolines are chosen in the same way so that results can easily be compared. I totally disagree with the statement that the model isn't validated at all, I rather think that no other chemistry-climate model has been compared to observations in such an extensive manner.

line 23: Here, I totally agree with the referee. The comparison is not really appropriate and can't serve as a validation. Gauss et al. are even critical by themselves, since time periods are very different between the model data and the observational

ACPD

5, S5510–S5518, 2005

Interactive Comment

data. This part has been withdrawn now.

line 24-33: In principle I agree with the referee. One can be suspicious whether the chemical ozone production and loss is well enough simulated in E39/C. However, I think the comparison to the middle atmosphere model provides frech support.

line 35-end: I followed the recommendations and included results from the middle atmosphere model. However, one has to be aware that the MAECHAM4/CHEM model does not resolve the tropopause region and the troposphere well enough to be included in the STE and troposphere section. Text added acordingly at the end of Section 8 (Discussion).

5. Detailed Response to Referee # 2:

Please see also 'General remarks' and 'Overview on the main changes'. Major comments:

First bullet: I agree with the referee, except for the location of the upper boundary, which is not at 30 but 10 hPa. This comment has been considered by including a middle atmosphere model and comparing the results to the E39/C model results. I have included a paragraph on the upper boundary condition (3rd par in model description) and a whole section on model validation to show that not only the mean state but also the variability is well represented. The middle atmosphere model MAECHM4/CHEM largely supportsx the E39/C results. The pattern of the ozone traces are basically the same in both models (below 10 hPa, of course). The main result that tropical ozone is of less importance at mid and higher latitudes is confirmed. The speculation that E39/C gives an upper limit for that contribution (=50%) is confirmed since MAECHAM/CHEM shows values as low as 30% only. The reasons for this are explained in greater detail in Section 5 2nd par. For a better illustration the discussed pathways are included in Fig.1.

Second bullet: A description of the convection scheme is added in the model description. I agree that the impact of the convection (or the scheme) on the results is interesting, as well as the impact of the lightning parameterization. However, a more detailed consideration of these aspects is beyond the scope of this paper.

Third bullet: The term tropopause always refers to the tropopause according to the WMO criteria, i.e. lapse rate criteria. This information is added to the text.

The definition of the areas plays only a role for the ozone production of the different ozone tracers. At tropopause levels the ozone production is almost negigible, as is indicated by tracers NHLS and SHLS. The tropics might be of more importance, but there the variability is much lower and the separation into the regions is following the tropopause height anyway. Therefore, a significant impact can be excluded. A discussion on that is included in the methodology section.

technical corrections: done - thanks

6. Detailed Response to Referee Heini Wernli

- Yes Fig. 6a (in the meanwhile Fig. 9) are net fluxes and the coordinate is pressure, as suggested by Grewe and Dameris (1996). This information was lacking and is now added to the text.
- Yes, Heini Wernli is right that the section on STE just gives information on the methodology applied. Since STE it is not the main and only topic of this paper, I tried to be as brief as possible, but obviously more discussion is needed, which I am happy to include.

ACPD

5, S5510–S5518, 2005

- Between 40°N and 60°N around 50 $\times$ 10^6 kg/s/km are derived from the model simulation in DJF, whereas Sprenger and Wernli give values between 1 and 3 or 4 $\times$ 10^6 kg/s/km, for a residence threshold time of 96 h. The values are a factor of 2-3 higher when assuming a threshold of 24 h. This study is based on 12 h values, which partly may explain differences in the results. So indeed the values are higher in the model simulation and in Grewe and Dameris (1996). However the shape is similar, with a maximum at 40°N going down to slightly negative values around 70°N. However, it is not clear, whether this difference arises from the different methodology (Wei versus Lagrangian) or the model.

  With this respect, I probably was too euphoric and present the results more critical. However, not the upward mass flux was of interest but the downward net flux as a source of ozone in the troposphere.

- Good question. Reithmeier (2001, his Fig. 5.3a) showed that the DJF mass flux calculated with a Lagrangian approach is similar to the results obtained with the Wei-Formula under certain assumptions. These are that for the calculation of mass exchange only those trajectories are used, which remain in the troposphere, respectively stratosphere, for at least 12h after crossing the tropopause. I take this as an indication that the results obtained with the Wei-formula, as implemented in this case, lead to acceptable results and represent the simulated mass flux in the model, since the Lagrangian approach should give reliable results of the simulated mass flux (Reithmeier, 2001).

- Even a better question. Taken the above argument, one should expect that the Lagrangian approach leads to a balanced mass flux. This is not the case when using the thermal tropopause as a separator, however it is when using the 380 K surface (Reithmeier, 2001), so I exclude that this is a problem for the model simulations.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9641, 2005.