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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A revised linear ozone photochemistry parameterization for use in transport and general circulation models: multi-annual simulations" *by* D. Cariolle and H. Teyssèdre

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Referee # 1

Referee 1 has many specifics comments and suggestions to improve the clarity and writing of the paper. We will take them into account in the revised paper to be submitted to ACP.

In particular the following clarifications can be given:

1. “Large amount of computer time”. The MOCAGE model requires 20 times more computer time when used with the full chemical scheme compared to simulation with the linear scheme.

2. “NOx and ozone destruction”. Large amounts of NOx tend to decrease the effectiveness of the chlorine ozone destroying cycles. This is mainly due to the reaction $ClO + NO$ that competes in the gas phase with the dimmer Cl_2O_2 formation at polar latitudes. Thus, homogeneous ozone destruction is limited by NOx amount, and the return of NOx inside the polar vortices is controlled by HNO_3 destruction. There might be some circumstances where NOx injection can increase the $ClONO_2$ formation and subsequently favour the Clx release due to the reaction of $ClONO_2$ with HCl on PSCs. But this process does not operate after the PSC evaporation, so it is not relevant for the specific situation (without the presence of PSC) that we want to model with the cold tracer parameterization.

Referee # 2

Majors comments

1. Comparison with the older CD86 scheme. We will further comment the differences with the CD86 scheme, and in particular make a reference to the paper by Geer et al.,ACP,7,939-959,2007, that gives a detailed analysis of the different terms of the parameterization. The major differences are a better agreement of the ozone background distribution (A3 term) with FL climatology, a slightly shorter ozone lifetime in the lower stratosphere and a much shorter radiation term (A6). The latter being obviously too large in the CD86 version.

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2. Forcing by ECMWF analyses. The scope of the article is of course not to compare the various dynamical forcing fields that can be used for the ozone transport; however, this is an important issue since it impacts directly the comparison with the ozone observations. What we found is that the ECMWF operational analyses have a meridional circulation faster than the Arpege/Climat circulation. This leads to an overestimation of about 60-80 DU beyond 60 N and about 40 DU in the belt of ozone maximum in the SH when the v2a is used. This is made clear when figures 4 and 7 are compared to figure 6. When the parameterization is forced with the Arpege GCM circulation the ozone distribution is very close to the TOMS measurements, and most of the above biases are removed. This is discussed in detail in section 3.2 where a methodology is described to quantify the impact the circulation changes on the ozone field. As suggested we will further develop the inter-hemispheric differences, but we think that it will not had more if we repeat the calculations with another analysis dataset. The causes of the overestimation of the meridional circulation by the ECMWF analyses are certainly not easy to establish and are currently under investigation at ECMWF. On our side we have performed “age of air” calculations with the ECMWF analyses and the Arpege forcing, and we found that the mean age of air is shorter by about a factor 2 with ECMWF circulation compared to evaluation made with the Arpege forcing. Some of those results will be added to the revised article.

Minor comments

1. We will add suggested references to works that have used the CD86 scheme.
2. We have not introduced any humidity dependence in our scheme for activation of the heterogeneous term in the ozone continuity equation. This could probably improve further the scheme, and account for some of the differences seen in the

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activation of the chlorine chemistry between the NH and the SH. However this will be dependant upon the quality of the water vapour budget within the forcing model, so in practice it might not improve the simulations.

3. In our simulations the cold tracer production is suppressed beyond 40 latitude, so there is no ozone destruction at the equatorial upper troposphere/lower stratosphere despite that the temperature can be lower than 195 K. Tests have been made without this restriction and the impact on ozone at low latitudes was found to be limited because the cold tracer lifetime is shorter that at high latitudes, and because the chlorine content is lower so the A8 term given by eq.4 is much lower (at least by a factor 6).

Referee # 3

1. The references to ERA40 versus IFS will be clarified.
2. The MOBIDIC model is first integrated without any heterogeneous chemistry but with the A8 term using the total chlorine calculated by the model. Steady state is reached, and then the partial derivatives are calculated (without the A8 term to avoid double counting) for each month.
3. The order of discussion in section 3.1 is low latitudes, SH and NH. We will make it clear in the revised manuscript.
4. As discussed above, the ozone overestimation at the NH polar latitudes is of the order of 60-80 DU in Spring when v2a is used. With v2b it is reduced by about 20 DU. So it goes in the right direction but cannot solve the problem. This is expected because other diagnostics (e.g. age of air) show that the ECMWF circulation is too strong, so we do not expect the chemistry to solve the high latitude ozone overestimation problem.

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5. At CNRM/Meteo-France the parameterization is used in the context of IPCC climate scenarios. Given the overall sources of uncertainties in calculating the climate responses with GCMs, we feel that the use of the parameterization is valuable in taking into account more feedbacks in the stratosphere. Namely the ozone increase due to the stratospheric temperature decrease forced by the CO_2 increase, and the lower stratosphere cooling due to the SH ozone destruction. It is obviously better than models using fixed ozone climatologies, and still much easier to run than fully coupled chemistry-climate models.

Suggestions have been made by the referees to improve the clarity of some figures, this will be done as much as possible.

Comment by J. McCormack

1. As suggested we will make reference to the up-to-date scheme described by McCormack et al., ACP, 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1655, 2007.