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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A chemistry-transport model simulation of middle atmospheric ozone from 1980 to 2019 using coupled chemistry GCM winds and temperatures" *by* J. Damski et al.

J. Damski et al.

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Response to

Interactive comments on "A chemistry-transport model simulation of middle atmospheric ozone from 1980 to 2019 using coupled chemistry GCM winds and temperatures" by J. Damski et al.

We thank the referees for the constructive comments, our response is given below.

Anonymous Referee 1 Received and published: 1 February 2007

In general this is a fair piece of work. The methodology: using a low resolution CTM coupled to a GCM which itself has coupled chemistry seems a bit strange. There is not enough attention in the work to the differences between these simulations. In some regions, ozone columns at high latitudes are 50-100% different with the same meteorology! (Figure 3).

This work may could use some important revisions as detailed below.

1.1) Comment: The methodology here is a bit strange and not well justified. What is gained by 'resimulating' the chemistry in a coupled chemistry climate model with a chemical transport model? In the past this has typically been done if it is too difficult to run a coupled model. Yet many groups now do this, and at 5x11 degree x 3km vertical resolution with 40 some species, this should not be too hard to run coupled.

1.1) Response: We feel that there is a lot to be gained by 'resimulating' the chemistry in a CCM with a CTM even if the level of complexity of the chemistry/microphysics are similar. It allows you to investigate the sensitivity of future predictions to changes in the chemistry (e.g. reactions/reaction rates), while keeping the Meteorology fixed. It would be difficult to do this in a CCM as the dynamics are inherently chaotic and this could mask any sensitivity to variations in the chemical parameters.

It is true that the used GCM already contains a fairly elaborate chemistry scheme, however there are some differences. In FinROSE a significant amount of effort has been put on the description of the PSCs and subsequent chlorine activation and sedimentation processes (denitrification - including NAT-rocks, and dehydration).

In the FinROSE chemistry solver the main stratospheric chemistry has been included, and the number of reactions is around 150. The total number of transported species (including families) in FinROSE is 28. The heterogeneous processing in the FinROSE is also different from the scheme of UMETRAC. The PSC/aerosol scheme includes

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parameterizations for liquid binary aerosols, PSCs type-Ib consisting of ternary solution, PSCs type-Ia consisting of nitric acid trihydrate (i.e. NAT), and ice-form PSCs (type-II). The formation mechanisms in the PSC/aerosol scheme are based on explicit thermodynamics rather than specified temperatures derived from thermodynamics. The chemistry used in the UMETRAC simulation has been described by Austin and Butchart 2003.

Basically, the more detailed polar chemistry of the FinROSE model, should allow the model to simulate more realistically seasonal ozone variations over high latitudes. It is, however, not expected that the general global multi-year behaviour of dominating constituents like total reactive nitrogen, methane, water vapour, inorganic chlorine, or bromine in FinROSE would be significantly different than in UMETRAC. I.e. it is expected that while both FinROSE and UMETRAC should give similar results from general decadal perspective, the differences in chemistry scheme should lead to differences in average levels and variability of various atmospheric constituents (over high latitudes) as FinROSE reproduces the compositional inter-relations using its own chemistry scheme.

With respect to the tracer transport, relatively speaking, similar behaviour of both models is expected, since horizontal winds from UMETRAC are used in FinROSE. The differences between the model results are explained by the following: - At each timestep in FinROSE the horizontal winds are interpolated from UMETRAC daily values - The horizontal resolution is different, and interpolation is therefore also needed. - The transport codes are also different, and the vertical transport is calculated in FinROSE from the continuity equation to ensure mass conservation.

It should be noted that running such a model as FinROSE is computationally cheap, and our approach does not require supercomputing resources. Therefore we like to think that our approach would add value to results of true GCMs (like UMETRAC) when e.g. refined schemes for different processes, like PSC-scheme are used.

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Finally we would like to emphasize that the simulation results presented here was set up during the EU/EuroSPICE project (1999-2002).

1.2) Comment: This work needs major revisions if it is to be publishable in ACP. In general more attention needs to be paid to the methodology. In particular, the major kernel of new information here is the relative impact of transport and chemistry on ozone, using the difference between the two simulations. This is not really discussed though. Why is the age of air different between UMETRAC and FinROSE?

I think this needs to be highlighted throughout. It could start with an ozone difference plot in addition to the panels shown in figure 2. Also figure 1 could show Age of Air from UMETRAC.

1.2) Response: In depth comparisons between the models are lacking because the focus of the paper is in the behaviour of the high latitude ozone and its variations over the simulated period. We are especially interested in changes and trends in polar ozone loss chemistry. We are in a way using the FinROSE model to interpret the UMETRAC results. The emphasis is therefore on the results obtained using FinROSE. The UMETRAC results have been published independently.

The Age-of-Air is very much dependent on the vertical velocities and since they are different also the Age-of-Air is different. The interpolation of the horizontal winds also affects the calculated vertical winds. The realized Age-of-Air should be taken as a feature of our simulation and shown as such.

In response to the comment we have added two panels to Fig 2. showing the differences between model ozone climatologies and the Fortuin and Kelder climatology. The text (p. 1149) and figure caption (Fig. 2., p.1171) was changed accordingly.

1.3) Comment: Also, several of the figures could be better presented. For those figures showing the complete timeseries from 1980-2019, (Fig 1,3,4,5 for example), it would be better (and more standard) to show deseasonalized anomalies from an average (say

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1980-1990). This would show ozone loss better, and is typical of model comparison papers (e.g. Eyring et al 2006)

1.3) Response: Figures 3 to 7 were changed. Figure 3 now shows relative anomalies in total ozone from the 1980-1989 mean. Figure 4 and 6 shows deseasonalized timeseries of total nitrogen and water vapour anomalies (compared 1980-1989 mean). Figures 5 and 7 now show climatologies of the relative effect of the ClOx and BrOx loss cycles during the past and future period.

Respective changes were made to the figure captions (Fig. 3 to 7, p.1172-1176) and the text (several small changes, p.1150-1156).

1.4) Comment: On page 6, 2nd column: a figure of deseasonalized anomalies (or annual mean) inorganic chlorine would be useful. This could be referred to on the bottom of page 7 for example to note when in the simulation it occurs.

1.4) Response: The evolution of inorganic chlorine (and bromine) is one of the driving parameters given by the UMETRAC simulation. The evolution is shown in Table 1. The inorganic chlorine concentration in the used projection peaks around the year 2000.

1.5) Comment: Figure 5 and 7 are hard to interpret. Perhaps showing a climatology over a year with variability would be better. It is hard to see the difference between the red and black curves, and whether the 1 pixel differences in width each year are significant.

1.5) Response: Figures 5 and 7 have been changed according to the suggestion and now show climatologies (see comment 1.3). The text was altered accordingly (p.1153-1156).

1.6) Comment: Figure 6 needs some work: it is not really necessary to show water vapor, and ozone concentrations should be deseasonalized.

1.6) Response: There is not much variation in the water vapour in Fig 6, but we want to present it analogously to Fig. 4 and show that no dehydration can be seen in the

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monthly mean over the Arctic.

Figs. 4 and 6 already show the difference between an inert ozone tracer and the model (chemical) ozone, which means that it is effectively already deseasonalized.

1.7) Comment: On page 10, you have not made an effective case that unambiguous recovery will not be seen. Why do you not show a time series? It certainly looks like some recovery may be seen?

1.7) Response: The trends in ozone for the future period are sometimes positive but, however mostly insignificant according to the statistical analysis. Very small (<1%) statistically significant trends can be seen in the northern mid-latitudes during the winter and in the northern high latitudes during the summer (Fig. 10).

In the annual mean there is also a small, just barely significant, positive trend (Fig. 10). Because the ozone trends are only marginally significant it can still be said that the recovery is not unambiguously seen in the FinROSE results. Some additions were made to the text accordingly (p.1158, lines 23-28).

"The FinROSE results indicate that the negative ozone trends are levelling off during the near future. Since the positive trends in winter and spring high latitudes, calculated from the FinROSE results, are not significant, we can conclude (supported by e.g. Weatherhead et al., 2000; Austin and Butchart, 2003; Weatherhead and Andersen, 2006) that no unambiguous recovery of the ozone is likely to be seen before 2020."

A deseasonalized high latitude ozone anomaly timeseries is now shown in Fig. 3.

1.8) Comment: Also, is the "increased interannual variability" dynamical or chemical? Is it the same or different between UMETRAC and FinROSE?

1.8) Response: The changes in interannual variability are a sum of dynamical and chemical effects. Our analysis does not explicitly separate the relative contribution dynamics and chemistry. In general the interannual variability simulated by FinROSE follows the interannual variability simulated by UMETRAC (see error bars in Fig. 9).

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The statement in the text 'Ěincreased interannual variabilityĚ' is misleading and was removed (p.1158, line 25).

1.9) Comment: The discussion and conclusions could use some refinement as noted above. Why are there such large differences in ozone depletion with the same transport? Is it still transport (different Brewer Dobson Circulation)? Is it chemistry? Which one do you think is right? What is the effect of differences in resolution? Or are the resolutions (horizontal and vertical) the same?

1.9) Response: The goal of this paper is to analyze the high latitude ozone variability and trends, not to make a model comparison (see response to comment 1.2).

Looking at Fig. 3 the ozone, given as anomaly from the 1980-1989 mean, are comparable in both models and also comparable to observations (TOMS and OMI). In general, in comparison to the Fortuin and Kelder ozone climatology, the ozone in FinROSE is somewhat too low over the tropics and in UMETRAC it is too high. In combination with the Brewer-Dobson circulation, as discussed in chapter 4, this gives too high ozone values over high latitudes in UMETRAC and too low in FinROSE (Fig. 2).

The differences between the models, e.g. horizontal and vertical resolution, are discussed above in the response to comments 1.1) and 1.2).

1.10) Comment: Is FinROSE truely an 'independent represenation'? I don't think so. Why do the models disagree?

1.10) Response: The simulations can not be considered independent, as FinROSE is an off-line CTM using the UMETRAC forcing. However, the CTM simulation can be considered as a sophisticated 'post processing' of the GCM results (in our case using the FinROSE chemistry scheme to study polar ozone loss).

The discrepancy between the models is a sum of several factors (different chemistry, resolution etc.), see response to comment 1.1).

1.11) Comment: Pg 12, bottom of 1st column: Is 30% ozone loss in the arctic seen in

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some years not significant?

1.11) Response: The ozone losses in the Arctic are on several occasions up to 30%, which is clearly significant (Fig. 6) and in-line with observations. However, what we meant was that the Arctic ozone loss does not reach the magnitudes observed in the Antarctic vortex, despite a colder stratosphere during the future period.

The text was reformulated to make this point clearer (p.1162, lines 11-13).

"However, since the conditions in the Arctic polar vortex are less severe than in the Antarctic, no large scale Antarctic-like ozone depletion is seen in the simulation before the end of 2019."

1.12) Comment: Pg15: "representativeness of chemistry solvers" Have you shown in any way that the FinROSE solver is superior to the UMETRAC one? I would like to see more justification of why this is a good way to do things.

1.12) Response: Our goal is not to show nor prove superiority of our chemistry scheme, but to show the usefulness of our approach. The computationally cheap approach gives the possibility to easily test different descriptions of the chemistry, e.g. PSC related processes or chemical kinetics, and their effect on polar ozone variability and trends.

For differences in model chemistry see response to comment 1.1).

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Anonymous Referee 2 Received and published: 13 March 2007

The past and near future stratospheric ozone evolution is investigated here with a FinROSE CTM 40-year simulation driven by the UMETRAC meteorology. The combination of the more detailed CTM chemistry with the GCM generated winds and temperatures is an exercise not often attempted and here is carried out generally well and with valid scope. By comparing model ozone trends between the recent past (1980-1999) and near future periods (2000-2019) it is shown that polar ozone depletion has reached its maximum and will not recover before 2020. Overall this is an interesting study and the whole effort is rather well presented. A main concern is that the results for the past ozone depletion (and trends) need to be validated and discussed more thoroughly in order to conclude with confidence about the future ozone behaviour. The paper deserves publication once the following points are dealt with in a satisfying manner.

Major points

2.1) Comment: The FinROSE chemistry related to ozone depletion gives sensible results in figures 4-7 but some additional validation or discussion on the model's ability to simulate stratospheric chemical evolution, especially in the high latitudes, is warranted. This can be done by showing comparisons from a FinROSE run (if any) forced by assimilated meteorology, where the model's chemistry can be tested against observations in the Arctic and Antarctic (or perhaps information (if relevant) from the Damski et al. 2006 paper could be used). The bottom line is that FinROSE is a CTM and some proof (or reference if this is done before) is needed of its ability to realistically simulate polar stratospheric ozone depletion (and for the right reasons). Only then the results of the CTM/GCM mode can be read with more confidence.

2.1) Response: The ability of FinROSE to realistically simulate polar ozone depletion has been shown in the Damski et al. 2006 paper, which is cited in this paper.

The conclusions in that paper were as follows: "As a general conclusion it is fair to say that the FinROSE model reproduces the observed global patterns, seasonal variations

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and year-to-year evolutions of stratospheric ozone well. Also the main stratospheric processes are captured well in the FinROSE simulations: ozone depletions with PSC formation, chlorine activation and prolongation of the ozone depletion due to denitrification."

2.2) Comment: 2. (related to point 1): In figure 4 it is not so easy to follow the seasonal evolution of the ozone depletion related species in a 40-year plot with such small spacing. Expressions pertaining to short-term temporal scales are used in the text like "... the large-scale ozone depletion is typically over by the end of November" (p.1152, l. 20) which cannot be possibly verified from the plotted monthly mean time-series. The authors mention that they don't look at the fine scale processes in this study. But a clear demonstration of the seasonal evolution of polar ozone depletion and related species in the past (and ideally a comparison with observations) would add more confidence to the near-future results and discussion. Maybe plots of two 20-year or four 10-year climatologies of the daily (or any datapoints shorter than monthly) evolution of ozone depletion and total nitrogen and chlorine activation would help (optional).

2.2) Response: The revised Fig. 3 now shows polar ozone anomalies from the 1980-1989 mean. The anomalies in both models are comparable to anomalies calculated from observations (TOMS and OMI).

The revised Figs. 4 and 6, now shows water vapour and total nitrogen anomalies (instead of concentrations) in order to make the figure more readable.

The referenced sentence (p.1152, line 20) was meant to be more general and applies basically to all figures 3 through 7. The text was altered accordingly.

2.3) Comment: 3. The degree of ability of the FinROSE ozone to reproduce accuratelly past ozone trends and especially the latitudinal evolution must be pointed out. For example, a distinct feature of the N.H. mid-latitude negative ozone trend, which peaks between 45-50 degees north and then it reduces toward 60 degrees north (figures 9 and 10, in year-round and winter) cannot be simulated by FinROSE (or any other GCM-

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driven models actually) which just gives a linear trend vs latitude from lower to high latitudes. Ozone from CTM experiments forced by "real" winds and temperatures from assimilated meteorological fields can capture exactly this latitudinal variation of the trend, pointing to dynamical causes (e.g. Hadjinicolaou, Pyle and Harris, 2005). This lack of (all GCM-driven ozone simulations) to account for realistic dynamical variability in the past creates also an uncertainty about the simulations (and conclusions) for the future and it should be mentioned in the discussion.

2.3) Response: Regarding the ability of FinROSE in general to reproduce past ozone values, see response to comment 2.1.

The following sentence was added to the text (p.1158, after line 18): It is interesting to note that a typical problem of model simulations over the northern mid-latitudes is also evident here. The northern hemisphere mid-latitude negative ozone trend, which peaks between 45-50N and then reduces toward 60N (e.g. Hadjinicolau et al. 2005) is not well simulated.

2.4) Comment: 4. The paper's main focus is the high-latitude ozone depletion and future recovery, but the polar ozone trend is not validated well I think, because in figures 9 and 10 the TOMS trends (due to lack of data in the polar night) are not there. Since the individual high latitudes data are not enough, you should add a trend comparison for both periods and hemispheres using the averaged 75-90 degrees data from figure 3.

2.4) Response: The TOMS and OMI data is missing during polar night also in Figure 3 (note that the red and blue line is not continuous). Therefore, the trend analysis for 75-90 degrees is not possible during polar nights in Figures 9 and 10 using the TOMS data.

Minor/technical points

2.5) Comment: 1. In figure 2 the comparison with the global ozone climatology could

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be helped if the percentage differences between model and observations could be plotted (or at least some mentioning of the magnitudes) and not just the asbolute value deviations as it is now.

2.5) Response: A panel showing the difference between FinROSE and the Fortuin and Kelder climatology was added to Fig. 2. The text was adjusted accordingly (p.1149-1150).

2.6) Comment: 2. In figure 3 lower panel (the high-latitude comparison in Northern Hemisphere), Fin- ROSE underestimates by at least 70 DU the winter-time values. How is that consistent with the unrealistically fast Brewer-Dobson model circulation (which we would expect to lead to higher winter/spring extra-tropical total ozone?

2.6) Response: This is mostly due to the too low ozone values in the tropics (see Fig. 2) which compensate for the strong transport.

2.7) Comment: 3. Any comment for the negative trend in DJF between 0-30 degrees north in 2000-2019?

2.7) Response: We decided not to speculate on this feature because the trend is not statistically significant, and it is also not within the scope of this paper, as we focus on high latitudes.

2.8) Comment: 4. p. 1144, l.2: replace "A Global" with "A global"

2.8) Response: The text was altered according to the suggestion.

2.9) Comment: 5. p. 1145, l.26-29: how will you "validate and compare these results" by just showing essentially the results you say you want to validate? Please rephrase.

2.9) Response: The text was rephrased (p.1145, line 26->): "We will show how the average high-latitude concentrations of stratospheric ozone, and ozone destruction have evolved during the past period (1980-1999), and how ozone is expected to evolve in the near future (2000-2019). The stratospheric processes affecting high latitude strato-

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spheric ozone will also be analysed, and the trend analysis of the past and future periods will be shown and compared against measurements."

2.10) Comment: 6. p. 1147, l.8 : bottom model level is the ground?

2.10) Response: The model domain starts from the surface (the bottom model half-level is the ground pressure). However, the tracer concentrations at tropospheric levels are given as boundary conditions. The text was modified as follows (p.1147, line 7): "The vertical resolution of the simulation was around 2700 m at 24 pressure levels from surface up to 0.15 hPa (or 62 km)."

2.11) Comment: 7. p. 1161, l.21: put a comma after "(e.g. WMO, 2003)"

2.11) Response: The text was altered according to the suggestion.

2.12) Comment: 8. p. 1164, l.11: replace "recieved" with "received"

2.12) Response: The text was altered according to the suggestion.

2.13) Comment: 9. The ozone trend figures 9 and 10 are really hard to read (axes labels) even with 200% magnification of the .pdf file. Please enarge somehow to ensure proper viewing.

2.13) Response: The axis labels and titles in figures 9 and 10 were modified to make them more readable (bigger fonts and less text in the titles, the information is given in the figure captions).

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1143, 2007.