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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.

**Anonymous Referee #1**

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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.

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.

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.

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

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.

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.

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

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?



ACPD

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

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?

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

Pg 12, bottom of 1st column: Is 30% ozone loss in the arctic seen in some years not significant?

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.

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