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Interactive Comment

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 #2

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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

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publication once the following points are dealt with in a satisfying manner.

Major points

1. 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. (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).

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-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.

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.

Minor/technical points

1. In figure 2 the comparison with the global ozone climatology could 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. In figure 3 lower panel (the high-latitude comparison in Northern Hemisphere), FinROSE 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?

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

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

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.

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

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

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

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.

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