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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mid-latitude ozone changes: studies with a 3-D CTM forced by ERA-40 analyses" *by* W. Feng et al.

Anonymous Referee #4

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The manuscript entitled "Mid-latitude ozone changes: studies with a 3-D CTM forced by ERA-40 analyses" by W. Feng and co-workers presents some sensitivity studies using an off-line three-dimensional chemical-transport model (CTM) in order to understand causes of the observed long-term changes in stratospheric ozone since 1977. The model is forced by ECMWF data (ERA-40 before 2002 and analysis after 2002) from the surface to about 60 km altitude. Different scenarios are proposed to check the sensitivity of ozone evolution to halogen and bromine evolutions from short-lived source gases, temperature and methane. Generally, modelled ozone change mimics observed ozone change although calculated ozone is greater than measured. Nevertheless, some differences remain. Although ozone amounts are becoming to increase since the beginning of the 2000s, there is apparently no indication of an ozone recovery induced

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either by halogens decrease, or by methane or temperature change since the end of the 1990s, or by additional 5-pptv stratospheric bromine from short-lived species.

The paper is well written and refers to recent publications in the field of the ozone recovery study. Figures and Tables are generally very informative. It is basically a continuation (improvement and update) of the work presented in Chipperfield (2003) referenced in the present manuscript. My main concerns are presented in the section General Comments. There are essentially linked to the weak improvement (sometimes some disagreements) with respect to the work of Chipperfield (2003) in terms of ozone amounts compared to the available measured data sets; weak or absence of explanations of some observed differences between measurement and model; the absence of other potential parameters that could influence the ozone evolution as e.g. solar illumination; the presentation of high latitude information although the paper refers to mid-latitude ozone change. For these main reasons, (and other specific comments listed below), I will not recommend the manuscript to be published in its present form. It will require some more explanations/studies in order to be just more than a continuation of the results presented in Chipperfield (2003).

General Comments

1. Feng et al. (2006) vs. Chipperfield (2003).

The work presented in this manuscript is an "update of Chipperfield (2003)" [p. 6698, l. 23]. Although some features in the O3 column evolution in the NH high latitudes (Fig. 2) are effectively calculated in Feng et al. during the period (1992-1995) while they are absent in Chipperfield, globally the O3 amounts in Feng et al., whatever the latitude band considered (Fig. 2 and 3), are much higher (by 10-20 DU) than both Chipperfield and measured data sets. This would require in-depth explanations of the worsening of the model outputs which are essentially attributed to a) the vertical domain considered and b) a too strong circulation. In that later case, what is the degree of confidence of these new results and of the chemical sensitivity exercise if the global circulation is not

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close to reality?

P. 6702, L. 6: I would add "by 10-20 DU" after "for all runs".

Figures 2 and 3a and b: I would overplot the output of Chipperfield (2003) run A. That will ease the comparisons and the discussions.

2. Measurements vs. Model

The calculated deseasonalized O3 time evolution as presented in the manuscript gives very intriguing results compared to measurements (Figures 3 and 4) at mid-latitudes. There is a strong negative linear trend from 1988 to 1993 in the model outputs whatever the run considered although total O3 measurements are quite stable during this period (Fig. 3). No explanations are given. In Fig. 4, from 1982 to 1988, anomalies of O3 changes are positive in the model and negative in the measured data set. This is again very intriguing since model output from Chipperfield (2003) much better reproduces the measured evolution. This will need clarifications and explanations.

The sensitivity of ozone change to parameters like Cly, ClO, CH4, and temperature is presented. My main concern is about the degree of confidence of the time evolution of these 4 parameters compared to measurements. For instance, the temporal evolution of Cly through HCl and ClO, that was based upon the measurements from UARS/HALOE, is now being revisited by using very recent AURA/MLS and ACE data (see Froidevaux et al., submitted to JGR, 2006, but accessible via the AURA/MLS Web Page). Consequently, the model temporal evolution of these particular parameters needs to be assessed.

3. Other parameters influencing ozone long-term evolution

Great emphases have been given to the influence of halogen and bromine compounds upon the long-term evolution of ozone over 30 years. It would have been wise to discuss potential parameters that could also act directly or indirectly in the ozone evolution as for instance the 11-year solar cycle and volcanic eruptions (volcanic loading)

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as Pinatubo whose signature is certainly present in the peak of ClO at 20 km in the beginning of the 90s [Fig. 8].

4. High vs. Mid-latitude Results

The manuscript title focuses on "Mid-latitude ozone change..." although one third of the paper is dedicated to high latitude ozone change (as in Chipperfield, 2003). I would encourage the authors to better explain why they have chosen to present high latitude results: validation of the model outputs, scientific significance, ... and I would change the title of the paper accordingly.

The calculated amount of BrO vertical profile is compared with measurements performed at Kiruna (67°N). The agreement is indeed very good. But the analysis focuses on mid-latitudes. Consequently, I would do my best to show a comparison with balloon-borne measurements not performed at high latitudes, for instance Aire-sur-Adour in France, or Bauru in Brazil.

Specific Comments

"Volcanic loading" (P. 6696, L. 21) is mentioned in the abstract although no real studies have been performed, shown or even discussed in the manuscript.

It is stated (P. 6701, L. 6) that "run A has a larger mixing ratio of Br below ˜25 km (compared to run D)". But this is impossible to check in Figure 1.

Technical Comments

P. 6700, L. 16: Typo "stratosphere" instead of "stratsphere".

P. 6704, L. 21: Add " and 20 km, respectively" after "40 km".

P. 6705, L. 19: Add "in both hemispheres" after "40 km".

P. 6705, L. 23: Add "(˜ -16 %/decade)" after "in the SH".

P. 6706, L. 5: Add "c and d" after "Fig. 10".

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