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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Results from a new linear O_3 scheme with embedded heterogeneous chemistry compared with the parent full-chemistry 3-D CTM" *by* B. M. Monge-Sanz et al.

Anonymous Referee #2

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Review of Monge-Sanz et al

General Comments

In this paper a new approach for developing a fast, linearised stratospheric ozone chemistry scheme (COPCAT) is introduced. As with previous such schemes, a comprehensive chemistry scheme is used to calculate the coefficients for the fast scheme. However, whereas previous schemes restrict such calculations to gas phase chemistry, and ozone loss due to heterogeneous chemistry is represented by alternative means, here the calculated coefficients implicitly include heterogeneous chemistry. Therefore, there is a greater degree of self-consistency involved in this approach than in previ-

ous ones. In addition, results from COPCAT are compared with results calculated with the same CTM and with the comprehensive chemistry scheme used to calculate the coefficients. Therefore, any issues with different chemistry schemes or model formulation are eliminated, and the differences in results should only identify issues with the formulation of the new, fast chemistry scheme.

Most results for the COPCAT scheme are shown for 2000, which is the same year as that used to calculate the COPCAT coefficients. This indicates that COPCAT is functioning pretty much as defined. However, a major issue is that the applicability of the COPCAT scheme to years other than 2000 has not been adequately demonstrated. This calls into question the authors' claim that COPCAT can be applied to multi years (ie a reanalysis). It also casts doubt on whether the scheme can be effectively used in NWP applications.

Since the heterogeneous ozone loss will be determined by the distribution of temperature and ODS, it is hard to see how much the COPCAT scheme, and its implicit heterogeneous ozone loss, will be applicable to years other than 2000. Some attempt at answering this question is made in Section 4.3, but there are several issues that remain unanswered:

* Figures 5 and 10 show, in the northern polar latitudes, the curious result that COPCAT produces larger errors, compared to a run with full SLIMCAT chemistry,in cold winters (1997, 2000) than in a warm winter (2001). This is despite the fact that the COPCAT coefficients are calculated using data from 2000. One could hypothesise that, if a cold winter implies more heterogeneous ozone loss, then these results show that the COPCAT scheme struggles to represent the heterogeneous loss well. In any event, the poorer performance in the colder winters needs to be further discussed.

* The comparison between the COPCAT and ECMWF Cariolle results is only shown for 2000, the year for which the COPCAT coefficients are calculated, and so in this comparison the odds are stacked in favour of COPCAT. Further comparisons of COPCAT

and ECMWF Cariolle schemes need to be made for years other than 2000. This shall make the authors' claim that COPCAT works better than the ECMWF Cariolle scheme much more robust.

* The performance of COPCAT in the southern polar latitudes was made for 2000 and also 2001, a year in which the meteorology is fairly similar to 2000. Therefore, one would expect the COPCAT scheme to produce similar results. A much more meaningful assessment of the performance of COPCAT would come from running the scheme for 2002, a year in which a major warming took place in the southern hemisphere.

I recommend, therefore, that the following changes to the paper be made:

1. Compare ECMWF Cariolle and COPCAT results for 1997, 2001 and 2002, not just 2000.

2. Examine the performance of COPCAT at southern polar latitudes in 2002, as well as 2000 and 2001.

3. Recalculate coefficients for a year other than 2000, and redo COPCAT runs for 1997, 2000, 2001, 2002, in order to understand the sensitivity of the COPCAT results to the year chosen for the calculation of the coefficients.

Incorporation of these changes will lead to a considerably enhanced assessment of whether the COPCAT scheme can be easily applied to reanalysis or NWP applications, or whether coefficients need to be continually calculated and updated, or indeed whether a more sophisticated representation of heterogeneous ozone loss than currently appears in COPCAT is needed. Without such changes, the paper is not suitable for publication.

Minor Comments

Introduction: The arrangement of the paragraphs in the Introduction leads to repetition and thus a loss of clarity. Currently, discussion of the current methods of including heterogeneous loss in parametrized chemistry schemes appears in paragraph 3 and

again in paragraphs 5 and 6, and the approach adopted by COPCAT appears in paragraphs 4 and 7. Paragraphs 3, 5 and 6 should be reordered so as to be contiguous, and likewise with paragraphs 4 and 7.

P12994, l 15-16: Break this into 2 sentences: change ", however" to ". However".

P13002, l 7: Change "range" to "ranges"

Section 3.1 and 3.2: Add a description of the errors in the HALOE and TOMS ozone products.

Section 4.1, para 1: Add a short summary of what SLIMCAT run 323 is – Monge-Sanz (2008) is not as widely available as some other publications, and therefore it is not straightforward to check out what run 323 is via this reference.

P13005, l 14-21: Geer et al included an investigation of the impacts of different ozone and temperature climatologies on the performance of parametrized ozone chemistry schemes. How do the results presented here agree or disagree with Geer et al's findings?

Figure 7 shows that SLIMCAT appears to underestimate tropical ozone, not just with parametrized chemistry but with full chemistry as well. These results require further discussion: are the differences due to biases in the correlative measurements, the SLIMCAT chemistry, or both?

P13008, l 7: Change "this kind" to "these kinds".

Figures 6, 7, 11 and 12: The solid blue lines here are so dark that they are hard to distinguish from solid bla ck ones. Use a much lighter shade of blue.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12993, 2010.