



## ***Interactive comment on “The SOCOL version 3.0 chemistry-climate model: description, evaluation, and implications from an advanced transport algorithm” by A. Stenke et al.***

**A. Stenke et al.**

andrea.stenke@env.ethz.ch

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### **Referee 1**

First of all, we would like to thank all referees for their valuable comments and suggestions, which helped us to improve the manuscript. In the following we provide our response to the specific comments.

#### **General Comments:**

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- *If my understanding is correct, vs2 simulation uses an underlying GCM different from that of the vs3 simulations. Can you make two simulations with the same GCM and the transport schemes presented in this article to distinguish between what is linked to the change of GCM and what is linked to the change of transport scheme? If not, as the differences in Figure 1 (b) and Figure 1 (d) are striking, additional references that would show the impact of a similar change in transport scheme would be beneficial.*

The suggested comparison is possible, in principle, and for sure interesting, but it is extremely difficult because the hybrid advection scheme by Zubov et al. (1999) used in SOCOLvs2 is not implemented in SOCOLvs3, and we do not see any reasons to do so for future applications. However, we know that the differences in CCly distribution (Fig. 1) reflect mostly the quality of the transport routine and do not depend dramatically on the choice of GCM. In the ideal case of a constant CCly source and a perfect transport routine, the stratospheric CCly distribution should be very homogeneous, independent of the applied GCM or circulation fields. In case of a temporally evolving CCly source, its distribution should resemble the distribution of the mean age of the air. Fig. 1 illustrates the gradual improvement of the transport scheme from vs1.3 to vs2.0 (using the same core GCM) and then to v3.0 with and without family transport (again using the same core GCM). Some comments have been added to the text.

#### **Specific comments:**

- *page 3422 line 5: CCly and CCly page 3430 line 7 have slightly different definitions. Please specify which one you used.*

Definition on page 3430 is used, clarified in the text.

- *page 3424 line 25: rather than the "The Chemistry scheme is called" shouldn't it be "The CTM is called"?*

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Text changed.

- *page 3425 line 3: can you provide an indication of the "further reduction of the wall clock time"?*

We added the following text: "For comparison, one model year with SOCOLvs2 required about two days on a single CPU (Schraner et al., 2008), while SOCOLvs3 requires about 4 hours for one model year (on 32 CPUs, T31 horizontal resolution, 39 vertical levels)."

- *page 3425 line 15: does the model with 39 levels go up to 0.01 hPa?*

Yes, clarified in the text.

- *page 3428 line 11: why did you analyze the 1975-2004 period, and not the 1960-2004 period as defined in CCMVal-2 REF-B1?*

Most of the CCMVal diagnostics are based on the years 1980-2004, the period with the best observational coverage for model evaluation. Therefore, we focused on the 1975-2004 period for the evaluation of SOCOLvs3.

- *page 3428 line 22: for the years 1950 ... why 1950?*

Text changed: "... for the years before 1996 ..."

- *page 3429 line 4: "wet deposition velocities": I think that the correct wording should be "wet removal rate", and Hauglustaine 1994 has a different list of species that undergo wet removal. What is your list of species that undergo wet removal?*

We agree that this section was misleading. We use only dry deposition velocities from Hauglustaine (1994). Wet removal is only considered for HNO<sub>3</sub>. New text: "MEZON considers dry deposition of O<sub>3</sub>, CO, NO, NO<sub>2</sub>, HNO<sub>3</sub> and H<sub>2</sub>O<sub>2</sub>. Dry deposition velocities over land and sea are based on Hauglustaine et al. (1994).

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Furthermore, the tropospheric wash-out of HNO<sub>3</sub> is described by a constant removal rate of  $4 \times 10^{-6} \text{ s}^{-1}$ , i.e. every two hours 2.8% of the tropospheric HNO<sub>3</sub> is removed." Furthermore, we moved this paragraph to Sect. 2.2.

- *page 3429 line 22: instead of "The assimilation" use "The nudging"*

Text changed.

- *page 3430 line 7: "Due to the absence of atmospheric sinks and sources": please provide additional explanations for this "absence".*

In the stratosphere and mesosphere there is only chemical conversion within the CCl<sub>4</sub> family, but there are no direct emissions, chemical production etc. Exchange with the troposphere is the only source and sink. We changed the text to clarify this point.

- *page 3433 line 12: does the SOCOL vs2 simulation cover the CCMVal-2 REF-B1 period, i.e. 1960-2005, or does it cover the 1975-2004 period?*

The SOCOLvs2 simulations cover the period 1960-2005, the SOCOLvs3 simulations started in 1975, after 10 years of spin-up. New text: "The SOCOLvs2 simulations are identical to the CCMVal-2 REF-B1 scenario, i.e. they cover the time period 1960-2005. The boundary conditions are mostly identical to the SOCOLvs3 simulations."

- *page 3433 line 22: I would not include the sentence: "The general characteristics of the model temperature biases are similar in both revisions.", as this is not the case for all parts of the stratosphere.*

Sentence removed.

- *page 3435 line 6: "shows very similar temperature biases ...". Can you indicate that this is true for all time periods and all latitudes analyzed? This seems a very*

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*strong argument. Can you include references that would confirm (or not) a similar result?*

We included the results for MA-ECHAM5 (T31L39) without interactive ozone in Fig. 2 (green line) and added some further discussion to the text: "However, it should be mentioned that the pure GCM MA-ECHAM5, without coupled chemistry (green line in Fig. 2), shows similar temperature biases in the polar winter stratosphere to SOCOLvs3. During other seasons (spring and summer, not shown) MA-ECHAM5 shows up to 5 K higher temperatures in the upper stratosphere. These differences are most probably related to different stratospheric water vapor concentrations: Since MA-ECHAM5 does not include chemical water vapor production, upper stratospheric water vapor concentrations in MA-ECHAM5 are 2-3 ppmv lower than in SOCOLvs3, resulting in less long-wave cooling in MA-ECHAM5. Comparing stratospheric ozone distributions in MA-ECHAM5 and SOCOLvs3 reveals largest differences in polar fall and winter. During this time shortwave heating by ozone is negligible in polar regions, indicating that the specification of the ozone distribution (fixed ozone versus interactively coupled ozone) has only a minor impact on simulated temperatures in the polar stratosphere."

- *page 3439 line 25: I think that "substantial" is a little too strong*  
"substantial" removed.
- *page 3342 line 11: the best agreement of T42 versus observations is not that clearly visible. Can you add four figures, for 1980-1989 and for 1990-1999 of the differences between NIWA and the corresponding T31 and T42 simulations?*  
We added a figure showing the differences between NIWA and the different SOCOL simulations for the 1980s and 1990s.
- *page 3459 Figure 4: what is this NCEP/NCAR data compared to the NCEP data listed elsewhere in the text?*

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NCEP/NCAR and NCEP means the same, NCAR removed.

- *page 3461 Figure 6 (and following): please indicate over which years the climatologies (obs and model simulations) have been calculated.*  
Model climatologies have been re-calculated over the period 1992-2001 to be consistent with HALOE data. Figs. 6, 7 and 10 have been replaced, but there are no substantial changes in the results.
- *page 3463 Figure 8: it would be preferable to have averages over the time period of the HALOE observations (1992-2001).*  
We re-calculated the H<sub>2</sub>O climatologies over the time period 1992-2001 and replaced Fig. 8 by a new version. However, the model tape recorder did not change substantially by changing the averaging period.
- *page 3466 Figure 11: the NIWA 1990-1999 contours are curiously unsmoothed. Do you have an explanation for that?*  
There was a problem with the applied smoothing routine. We re-plotted this figure with a better smoothing.

#### **Typing errors:**

- *p 3428 line 11: horizontal resolutions*  
done
- *page 3433 line 19 the ERA-40*  
done
- *page 3436 line 21 to be a more reliable*  
done

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- *page 3342 line 23: previous sections*  
done
- *page 3457 Fig2: relative to the ERA-40 reanalysis*  
done

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Interactive comment on Geosci. Model Dev. Discuss., 5, 3419, 2012.

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