Atmos. Chem. Phys. Discuss., 8, S5304–S5305, 2008
www.atmos-chem-phys-discuss.net/8/S5304/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: Chemistry-climate model SOCOL: version 2.0 with improved transport and chemistry/microphysics schemes" *by* M. Schraner et al.

Anonymous Referee #2

Received and published: 27 July 2008

Title: Technical Note: Chemistry-climate model SOCOL: version 2.0 with improved transport and chemistry / microphysics schemes.

Schraner et al., 2008.

This paper summarizes improvements to the SOCOL chemistry-climate model. This work specifically addresses published peer reviewed shortcoming in the distribution of chemical constituents. The authors show that these shortcomings are directly related to the non-conservative properties of the SOCOL semi-Lagrangian transport scheme. This work describes the implementation of a mass fixer approach that minimizes the transport scheme's non-conservation nature. Model results are shown and compared

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to observations for water vapor, ozone, HCl, and total inorganic chlorine.

This is clearly a model description paper and no substantial science is presented. However, these types of paper are important and model capabilities need to be documented in the literature.

I recommend this paper be published as a technical note in ACP.

Minor Comments:

Figure 4b. I'm a bit surprised that even after applying the mass fixer, the SOCOL model still underestimates CClY in the SH, Polar stratosphere. The values are around 2.7 ppbv instead of 3.1 ppbv. Do the authors understand why?

Figure 5. In simulations R4 and R5 (version 1.3 with mass fixer), the ozone abundance in the October SH 50hPa period is approximately 2.4 ppmv. Ozone during this period at 50hPa should be approaching 0.5 ppmv. Isn't version 1.3 with the mass fixer essentially version 1.4? If so, why does the ozone at 50hPa seem inconsistent with Figure 3 total column plot (panel d)?

Concern: I completely understand the need to apply a mass fixer to total inorganic bromine and chlorine species in the polar stratosphere. However, applying a mass fixer to ozone, even if it is in a restricted latitude region worries me. This issue is precisely why many CCM groups have moved to the Lin and Rood advection approach. Is there a reason/advantage why the authors have choosen this semi-Lagrangian scheme?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11103, 2008.