Geosci. Model Dev. Discuss., 2, C297–C299, 2009
www.geosci-model-dev-discuss.net/2/C297/2009/

Geoscientific
Model Development
Discussions

Interactive comment on "Description and evaluation of the Model for Ozone and Related chemical Tracers, version 4 (MOZART-4)" *by* L. K. Emmons et al.

Anonymous Referee #1

Received and published: 28 September 2009

This is an excellent paper documenting the most recent developments of an well-established global atmospheric chemical transport model. The paper is clearly laid out, providing an overview of key model algorithms before focusing on new treatments in this model version, and then providing a brief evaluation of performance by comparison with observations over the 2000-2007 period. Overall, the description is comprehensive, well written and thorough, and it provides a good foundation for researchers in the atmospheric modeling community who may wish to download (or upgrade to) this new version of the model. The paper is appropriate for publication in GMD with only very minor revisions that address the comments outlined below.

Page 1160: potential users may want to know what approach to parallelism has been adopted (MPI, OpenMP, both?)

Page 1166/1167: the equation numbers here are confusing and should be removed; anyone wishing to cross-reference with the Guenther paper should have little difficulty identifying the relevant equations.

Page 1167, line 20: loose parenthesis in this equation. How does the scheme used here differ from that in the Guenther paper? An additional sentence of explanation would be valuable.

page 1170, section 3: given the number of studies involved, I find this section on recent applications of the model very brief. I would welcome an additional paragraph here noting the strengths and weaknesses of the model that these studies have uncovered, or comparison with earlier model versions or with observations.

Page 1173, section 5.1: although OH is slightly less that the climatology, the profile also differs in shape (less of a bulge in the mid-troposphere.) What additional information about the simulation could this provide? Can the weakness be attributed to the meteorological fields used (humidity, cloud cover?) or is it likely to lie in MOZART algorithms? If OH has been evaluated with other met fields this might be more evident.

Page 1176: the final sentence of section 5.4 does not inspire confidence in the model. While it is good to speculate about potential weaknesses, it would helpful to indicate how these might be evaluated, tested or addressed.

Page 1196, end of table 3: products have been omitted in the DMS reactions. Is this intentional, i.e., are other products (e.g., HCHO) neglected? If this is so, it would be helpful to state earlier in the paper that DMS is only included as a precursor for sulfate aerosol.

Page 1202, table 9: are the biogenic emissions of CO treated through MEGAN, or are these emissions independent? This should be noted in section 2.7.

GMDD

2, C297–C299, 2009

Fig 4 and Fig 6: replace the internal run labels in the titles of independent figures with "MOZART-4"

Fig 6: columns expressed in mixing ratio: over what depth is this an average?

Interactive comment on Geosci. Model Dev. Discuss., 2, 1157, 2009.