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**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 #2

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General Comments:

I agree with the first reviewer's assessment that this is a well-written description of the MOZART-4 chemical transport model, suitable for publication after some minor revisions. The authors document aspects of the model that will be useful beyond the MOZART-4 community. For example, the species mapping of MOZART-4 VOCs to other mechanisms is useful for regional modelers seeking boundary conditions from global models. In some places, additional explanation could enhance the utility of this study to a broader modeling community, as described below.

Specific Comments:

P1163 L7. What other options were considered for washout rates? What AOD observations were used, and how much did the agreement improve with this choice? Has there been any evaluation with observations over land?

Sections 2.1 and 2.2. If there are known areas where the mechanism and aerosol chemistry is most uncertain, this could be acknowledged. For example, isoprene oxidation chemistry is rapidly evolving with new field and lab experiments suggesting that current schemes are incorrect, with implications for both gas-phase chemistry and secondary organic aerosols.

Section 2.3. How different are the results for some key photolysis rates (e.g., J O1D) when FTUV is used rather than the lookup table? It might be clearer to state at the beginning that a major improvement here is the inclusion of aerosols in the photolysis calculations, in addition to the on-line (vs. lookup table) approach.

Section 2.5. A more explicit description of "strongly dependent on the soil moisture" (L14) and "vegetation-dependent" (L16) would be helpful, as would a rough idea of how variable the deposition velocities are when calculated online as compared to the climatology.

Section 2.7. It seems appropriate to briefly comment on the major differences in switching isoprene emission factor maps and the vegetation maps (range of emissions from Pfister et al 2008 seems to be a factor of 2 from their Table 2), and why the combination used here is preferred.

Section 2.8. For comparison with implementations in other models, consider giving the dependence on the soil dryness dependence more specifically. What is the contribution from fertilizer use to total global soil NO emissions? Please describe how the vertical distribution of lightning NO has been modified from Pickering et al [1998].

P1169 L24-26. It'd be helpful to make a stronger recommendation as to when to use the SYNOZ tracer, and how to judge whether it is needed (P1170 L13-15 should explain

how that conclusion was reached)

Section 3. As suggested by the first reviewer, this section could point out where there are changes adopted (or not) from these prior studies or updates reflecting newer information since those studies, such as in the chemical mechanism (e.g. rate differs from Pfister et al 2008a Table 1 for ISOPO2+NO and several differences from Horowitz et al 2007).

Section 5.1. The methane lifetime is heavily weighted towards the OH in the tropical lower troposphere, so it'd be useful to also provide the methyl chloroform lifetime and compare both to those derived from observations [Prinn et al., 2005, Geophys Res Lett, 32, L07809]. Further, as suggested by the first reviewer, the relative distribution of the OH in MOZART-4 vs. the climatology could be discussed, with suggestions for what contributes to the differences.

P1175 L1-2. Some elaboration on the variation of the bias with season would be appropriate. For example, over the U.S., the model is too low in winter, but seems too high in summer (the summer overestimate is consistent with the Hudman et al [2008, Geophys Res Lett, 35, L04801] findings from the ICARTT field campaign).

P1177 L3-5. This broad statement doesn't seem consistent with some of the problems highlighted in earlier sections.

Figure 3. How typical is this profile? Some context should be given in the text.

Figure 5. Any suggestions for what is causing the model underestimate in 2003 at Tenerife and Mauna Loa, where it otherwise seems to capture the yearly variability?

Technical corrections:

P1169 L18-20. Is some stratospheric chemistry being done online, or is there a parameterization to aerosol surface area density being used?

P1174 L19-20. Is the MOPITT averaging kernel sensitive near the surface? If not, then

would releasing the emissions at the surface lead to an apparent underestimate?

Section 4.1 Do the anthropogenic emissions include hourly, daily or seasonal variability? Which years are included in the REAS inventory? When only one year is given (e.g. Bond et al 2004 inventory; aviation and military traffic), are those emissions applied as annually invariant, or is some scaling assumed? It'd be useful to have a sentence stating which emissions vary on what time scales.

Table 2. Is there a reason why some species are given comments?

Tables 9 and 10. The 2006 totals disagree for ISOP and C10H16, possibly an issue with Tg C vs. Tg species?

Figure 7. Please provide the latitude and longitude ranges for the regions considered.

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