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Geoscientific
Model Development
Discussions

Interactive comment on "Carbon monoxide as a tracer for tropical troposphere to stratosphere transport in the Chemical Lagrangian Model of the Stratosphere (CLaMS)" *by* R. Pommrich et al.

Anonymous Referee #2

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Review of the paper entitled " Carbon monoxide. . . " by Pommrich et al.

The paper assesses the quality of the simplified chemistry version of the CLAMS model to reproduce the variability of carbon monoxide (CO) in the tropical Upper Troposphere/Lower Stratosphere (UTSL), or Tropical Tropopause Layer (TTL), by comparing model outputs over several years (2002-2008) with space- and airborne measurements (MOPITT, MLS and COLD) in the troposphere and in the stratosphere. The seasonal, geographical and vertical variabilities of modelled CO tend to globally match the measured variabilities, but some systematic differences are present. Some are attributed to the introduction of a new scheme in the ECMWF integrated forecast system after

2006, other are attributed to the model itself that cannot track the convective activity of the air parcels.

The paper is well written, the Figures are clear and concise. These are the main positive points. Unfortunately, the scientific contain is rather weak. The manuscript, in its present stage, in unable to explain the systematic differences observed between the model and the measurements, whatever the altitudes considered (in the troposphere and in the stratosphere) in the northern hemisphere (NH) winter/spring seasons, whatever the years considered (even prior to 2006). There exists a great underestimation of CO in the model that needs to be seriously discussed: boundary conditions, loss/production paths, simplified scheme, global and sub-grid scale transport, etc. The importance of O3, N2O, CH4, CCl3F, and CO2 in the model runs is presented throughout the paper but no results are given for these molecules and for some species (e.g., N2O and CCl3F) it is even impossible to understand how they can contribute to the CO variabilities. For these reasons and some other points listed below, I cannot propose the manuscript to be accepted in its present form. It will require major revisions.

Major points.

1. Underestimation of the modelled CO. Whatever the instruments (MOPITT, MLS or COLD) and whatever the altitudes (350 K-250 K for MOPITT on Figs. 2 and 3; above 100 hPa for MLS on Fig. 4; and 250 K-475 K for COLD on Figs. 5 and 6) considered, CLAMS CO is systematically less that measured CO, particularly during the Northern Hemisphere (NH) winter and spring seasons. This absolutely needs to be discussed, independently of the discussion on the introduction of a new scheme in the ECMWF integrated forecast system after 2006: boundary conditions, loss/production paths, simplified scheme, global and sub-grid scale transport, etc. Note that the comparisons COLD-CLAMS on both 5 and 15 February 2005 (Figs. 5 and 6, respectively) are really bad around 350 K with a factor of 2 difference that cannot be solely explained by the convective activity on one date and not on the other one.

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2. Long-lived species. It is not obvious why all the molecules listed in the simplified scheme of CLAMS like O3, N2O, CH4, CCl3F, and CO2 are so important in the model runs since, for some species (e.g., N2O and CCl3F), it is even impossible to understand how they can contribute on the CO variabilities from Reactions (R1-R10). If these molecules are so important, some CLAMS results need to be presented and assessed against measurements since that could explain the systematic underestimation of model CO.

3. Chemical reactions. Again, regarding these reactions, we first need to understand why they are so important to track the CO variability in the TTL, and second, they need to be correct. For instance, (R4) should produce HCl. (R8) cannot produce 2xO3. A simplified reaction as 3 O2 -> 2 O3 could be more understandable. Finally, (R9) and (R10) are somewhat difficult to understand before having read the manuscript. I would rather put some brackets as:

[O3 + OH -> HO2 + O2] x 2

[O3 + HO2 -> OH + 2O2] x 0

Minor points.

P1188, L8: In the introduction, the structure of the paper (the different sections) needs to be presented.

P1188, L7: Remove the sentence “We find. . .” since it is a result.

P1188, L25: “then” should be “than” and a “s” should be added to “give”.

P1188, L25: The discussion on DFS relates to the tropospheric, the stratospheric, the tropospheric-stratospheric or the total contents of the MOPITT measurement information?

P1189, L10: I would add “millimeter and” prior to “sub-millimeter”.

P1189, L18: I would write “6-9 %”.

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P1193, L3: Typo in “O(1D)”.

P1195, L24: Note the systematic underestimation of model CO over the tropical Africa and Atlantic Ocean in winter, spring and autumn.

P1197, L4: Can 100 hPa still be considered as TTL? The definition or the vertical extent of the TTL should be presented. My understanding is that the MLS measurements and the CLAMS outputs in this section are representative of the Lower Stratosphere and not the TTL.

P1197, L20: “altitudes” should be “isentropes”.

P1197, L21: The sentence “we attribute. . .” needs some more discussions since only vertical transport is considered there, and not horizontal transport (or other causes).

FIGURES:

Fig.1: I am not sure I have really understood the importance of this Figure in the article.

Fig. 2: Please expand the vertical axes on the 4 boxes. The CLAMS-MOPITT comparisons will be better highlighted and explained.

Fig. 3: What is the period taken into consideration in the seasonal averages?

Fig. 4: The black line is missing on the top panel.

Figs 5-6: The date should be in YYMMDD for a better understanding.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1185, 2011.