**Atmospheric
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Interactive
Comment

Interactive comment on "A linear CO chemistry parameterization in a chemistry-transport model: evaluation and application to data assimilation" *by* M. Claeyman et al.

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Received and published: 11 June 2010

The authors thank very much Dr. Jackson for his helpful and constructive review of our paper. Below, please find the reply to the general comment and the minor comments as well.

General Comments

In this paper a fast, linearised CO chemistry is introduced. The scheme uses a similar approach to Cariolle, and other similar, ozone chemistry schemes. Comparison of results with this scheme with those from a comprehensive scheme (RACMOBUS) and with independent observations indicates the good qualitative performance of the

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scheme and characterises its biases. It is also demonstrated that assimilation of MO-PITT CO data using the linearised scheme is feasible and produces understandable and reasonable results. This paper presents original research, since to my knowledge it is the first time that such a linearisation approach has been applied to CO. It is also clear that the linear scheme shall be highly useful in performing multi-year CO reanalyses in a computationally efficient manner. Therefore, there are a number of reasons that suggest this paper merits publication. However, there are a number of points that need to be addressed first before the paper is acceptable for publication.

1) Figures 2 and 3 show that LINCO has a low bias compared to RACMOBUS in the troposphere. Subsequent figures also show LINCO is biased low compared to independent observations. There is little indication in the paper of why this bias exists apart from perhaps on p 7007, l 7-9, which suggests too much destruction of CO in the LINCO scheme or too low emissions used in CTMs. The latter does not explain why the LINCO CO is lower than RACMOBUS, since they, I understand, use the same emissions inventory. Therefore there could well be an issue with the coefficients calculated for the LINCO scheme. Geer et al (2007), in the context of ozone chemistry, indicate the impact of changes to the reference state about which the chemistry is linearised. Using a similar argument, it may be possible that the zonal mean CO and temperature climatologies used in LINCO (ie the A3 and A5 coefficients) are inappropriate. The authors should plot comparisons of the A3 and A5 values used with corresponding CO and temperature climatologies from MOCAGE / RACMOBUS and from this deduce whether this is an explanation for the biases seen in LINCO, or whether to eliminate this as a possible explanation of the LINCO biases. If the former, it would be easy to re-run the experiments by substituting the original A3 and A5 values with the new climatologies.

Our reply:

A similar remark has been made by reviewer # 1 suggesting three possible causes. According to what we said to reviewer 1 (Figures R1, R2 and R3), we think that the

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term to modify is the production minus loss rate coefficient (A1) to reduce the negative bias of LINCO in the troposphere and not the A3 or A5 coefficients. We also corrected the new version of the paper in order to explain this (section 3.1)

2) The authors produce a plausable explanation for the low (high) bias seen in LINCO CO fields in the winter extratropical middle (lower) stratosphere, namely that this is due to too rapid downward transport of high CO from the mesosphere. To support this, further details on the ARPEGE analyses used to drive MOCAGE need to be given. Recent work (eg Schoeberl et al (2003), Monge-Sanz et al (2007)), indicates transport problems for US, Met Office and ECMWF analyses that use 3D-Var, and that many of the transport problems in the ECMWF analyses are reduced when 4D-Var was used.

Our reply:

Similar behaviours have been found by Teyssèdre et al (2007) using ARPEGE (but for different years) and MOCAGE by calculating the age of the air. References and comments were added in the paper (section 3.3)

What is also not particularly clear is why a high bias also exists in the lower stratosphere in the summer and tropics as well (see eg Figure 3). This is unlikely just to be a transport of the high biased CO from the winter lower stratosphere, since the photochemical relaxation time in the tropical and summer lower stratosphere is less than 30 days. A better explanation for the tropical and summer stratosphere bias is needed.

Our reply:

To better understand the bias in the stratosphere between RACMOBUS and LINCO, we follow the same methodology as Geer et al., (2007) (see §2.7 of this paper) to diagnose the relative strengths of the different LINCO coefficients. Figure R2 shows that this bias may be due to the positive bias between A3 and RACMOBUS and the positive (P-L) production minus loss rate coefficient in the stratosphere (A1) (Figure 1.b of the paper). Comments have been added in section 3.1 and 3.3

When examining the bias of LINCO CO in the upper troposphere stratosphere it makes sense to look at Figures 3 and 7 side-by-side. However, this is currently difficult, since they are not self consistent. Change these figures so that a) they both show the same months (currently Figure 3 shows January and July and Figure 7 shows October and March); b) they both use the same vertical co-ordinate (pressure or height), or approximate height is plotted next to pressure in Figure 3; c) the differences between LINCO and the verifying field have the same sign in both (eg positive means LINCO is higher in both Figures).

Our reply:

Pressure axis has been plotted next to the altitude axis in Figure 3 and the differences in Figure 7 have been corrected to be consistent with Figure 3.

However Figure 3 and 7 cannot be directly compared since LINCO is smoothed by MLS averaging kernels in Figure 7 and it is not in Figure 3. Furthermore, for Figure 3 we choose two months representative of the NH winter and summer (July 2004 and January 2005) but MLS data were only available in mid-August 2004. Thus we decided to select other months of the simulation for comparison with MLS, where CO concentrations are high in the SH (October 2004) and in the NH (March 2005).

Minor Comments

p6996, l 6, 11: "One the one hand.." and "On the other hand.." don't make sense in this context. Replace with "First" and "Second".

Our reply: corrected

p6997, l1: Change "For this" to "For these reanalyses". This is clearer and easier to understand.

Our reply: corrected

p6999, l12: Change "Long run" to "Long runs".

Our reply: corrected

p7001, l24 / Fig 1c: The text states that tau is up to one year, but the maximum shading interval shown in the Figure is 200 days. Can the shading be changed to show another shade at 400 days?

Our reply: The figure has been modified in the paper.

p7003, l21: Change "insure" to "ensure".

Our reply: corrected

p7006, l2: Delete "The runs are called...RACMOBUS (detailed)", since this just repeats the text that appears at the start of Section 3.

Our reply: corrected

p7008, l10-16: The explanation given for the high CO over the Tibetan plateau would be confirmed by showing the equivalent CO field for the RETROBUS run. This is because the (MOCAGE) dynamics should be the same as for the LINCO run. Therefore a similar model - MOPITT difference would confirm that the bias is indeed due to the model dynamics, and not the CO chemistry scheme.

Our reply:

In order to confirm this, we plotted the same figure for RACMOBUS smoothed by the MOPITT averaging kernel (Figure R4). The same over estimation of CO concentrations is observed over the Tibetan plateau which confirm that this bias is linked to the dynamics and transport in MOCAGE and not to the chemistry of LINCO. We added this comment in section 3.2. In order not to increase the number of figures, we do not however propose to include figure R4 in the final version of the paper.

p7009, l27-28: "This indicates that LINCO does not present a global systematic bias.." This is confusing, since Figure 10 seems to suggest the opposite. The results from Figures 9 and 10, and the apparent contradictions, should be explained better.

Our reply:

This sentence is relative to Figure 8 which is a comparison between MOZAIC and LINCO for levels between 300 hPa and 180 hPa and not for the complete troposphere which is the case in Figure 10. In Figure 10, the bias is also very low around the pressure level of 250 hPa. To avoid any confusion we modified the text in section 3.3

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C3870/2010/acpd-10-C3870-2010-supplement.zip

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6995, 2010.

A5   **ARPEGE Temp**   **A5-ARPEGE (K)**
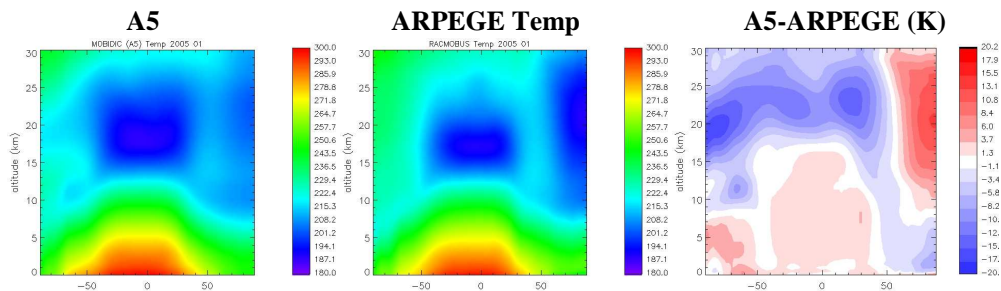
Figure R1: A5 term representing the January climatology zonal mean temperature in MOBIDIC
model in K (left); January 2005 zonal mean temperature from ARPEGE used by MOCAGE in K
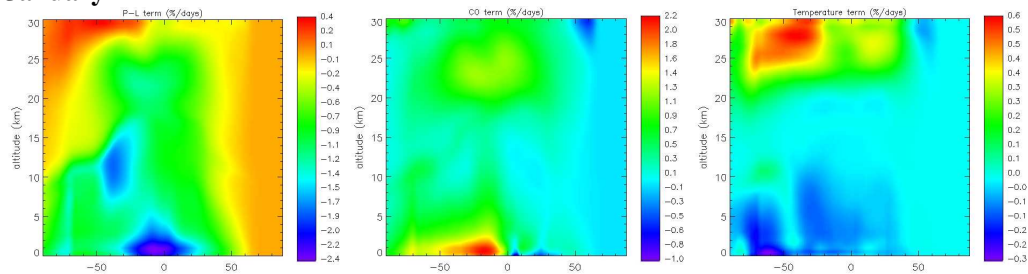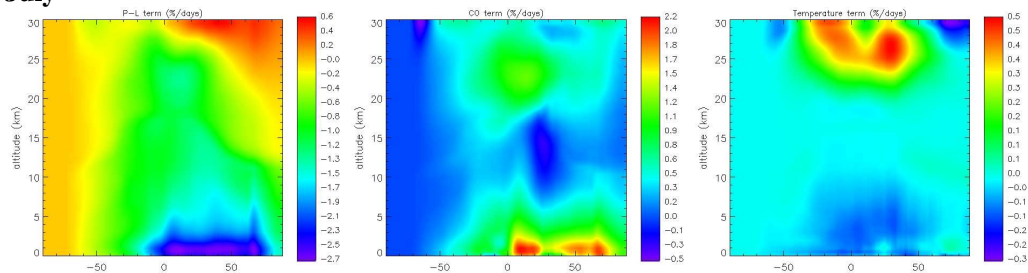(middle); Difference between A5 term and ARPEGE temperature in K (right).

Fig. 1.

January

July

Figure R2 : (P-L) term A1 (left) ; CO term A2(r_CO-A3) (middle) ; Temperature term A4(T-A5) (right) in %/day. The top three figures are relative to January and the bottom three figures are relative to July.
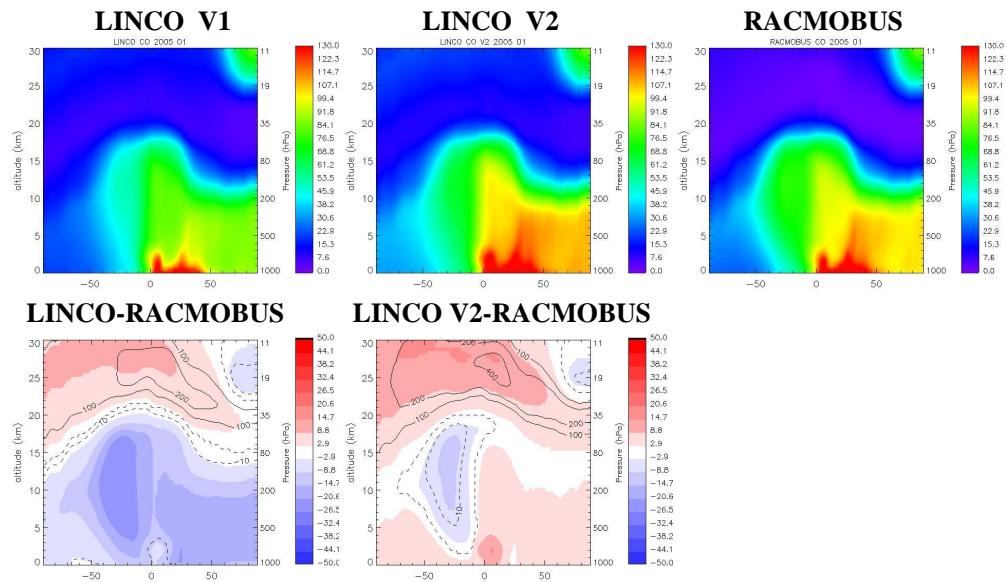
Fig. 2.

Figure R3 : Top : Zonal mean CO concentration from LINCO V1 (left), LINCO V2 with A1'=A1-0.2*A2*A3 (middle) and RACMOBUS (right). Bottom : difference between LINCO V1 and RACMOBUS (left) and LINCO V2 and RACMOBUS (right).

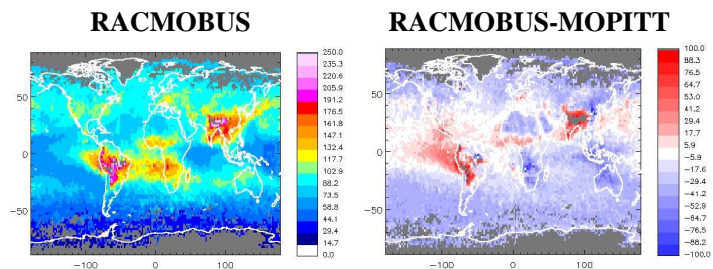**Fig. 3.**

RACMOBUS   **RACMOBUS-MOPITT**

Figure R4 : CO fields in parts per billion by volume (ppbv) at 700 hPa calculated by RACMOBUS smoothed by MOPITT averaging kernels (left) and corresponding relative difference (%) (Model-Obs)/Obs*100 (right).

Fig. 4.