**Atmospheric
Chemistry
and Physics
Discussions**

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

D. R. Jackson (Referee)

david.jackson@metoffice.gov.uk

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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 scheme and characterises its biases. It is also demonstrated that assimilation of MOPITT 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.

2) The authors produce a plausible 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

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.

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).

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".

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

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

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?

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

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

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.

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.