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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.

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This paper presents LINCO, which is probably the first linear CO chemistry parametrization, and is based on an approach that was originally developed for ozone. It will be useful for long chemistry model runs and for data assimilation. The scheme is validated in three ways: against a more detailed chemistry model, against independent data, and within a data assimilation system. This validation covers the troposphere and stratosphere, making for a good, comprehensive paper.

However, there is one major area that requires more work. The validation shows that in the troposphere, LINCO produces CO values that are around 20% too low com-

pared to both the detailed chemistry model and to independent MOZAIC observations, which agree with the detailed chemistry model. This negative bias crops up throughout sections 3 and 4. The explanation given for this bias is simply that there is "a larger destruction of CO in LINCO" on p 7007. This is not sufficient. The causes of the bias need to be properly explained. I think there is a good chance that the cause can be discovered and moreover, that the bias could easily be removed from the LINCO scheme.

There are some very likely causes of this bias:

1) The bias reflects a difference in the CO climatology of MOBIDIC (used to derive the LINCO coefficients) compared to RACMOBUS. If so, this bias will be reflected in the A3 term in LINCO, which represents the CO climatology towards which LINCO will relax. This hypothesis should be easy to check, and must be considered in the paper.

2) The bias comes from a systematic difference between MOCAGE model temperature T and the RACMOBUS zonal mean temperature coefficient A5. This again should be considered in the paper.

3) The bias comes from systematic transport differences between the MOCAGE model and RACMOBUS, meaning that the production - loss rate (the A1 coefficient) is not in balance with the MOCAGE model transport. The authors already explain polar winter stratospheric biases in this way.

The first two biases are most likely where the photochemical relaxation time is relatively fast, and the third is more likely where it is slow. These kinds of problems are common in linearised ozone schemes and are addressed in Geer et al. (2007, ACP, 7, 939-959, particularly sections 2.1 and 2.2) and in Coy et al. (2007, ACP, 7, 2917-2935). Other studies which have investigated these effects are Eskes et al. (2003, QJ, 129, 1663-1681) and McCormack et al. (2006, ACP, 6, 4943-4972). If either of the first two hypotheses explains the bias, it would be easy to remove it by adjusting the climatology parameters A3 or A5 appropriately. This should produce a version of LINCO that is

more in agreement with detailed chemistry and with observations.

A final possible explanation for the bias would be that the MOBIDIC, MOCAGE or RACMOBUS emission and deposition climatologies may be inconsistent. Again this should be considered.

Minor and technical corrections

p.6996 l.6, l.11: "on the one hand .. on the other hand..." suggests the balancing of opposing concepts, which is not the case here.

p.6996 l.10: Differences in ppbv - could the % figures also be supplied?

The abstract could be compressed. 250 words is often recommended, and helps readers quickly assess a paper.

p.6997 l.20 "along with photochemistry production". Do you mean "photochemical production"?

p.6999 l.2 "consistent 4-D fields" - please say what these fields contain (e.g. chemical species, dynamical quantities?).

p.6999 l.12 "makes possible long run" -> "makes possible long model runs?"

p.7001 l.25 "in nh" -> "in the nh"

p.7001 l.26 "complemented with surface emissions and deposition" - please add some details or references

p.7002 l.5 "hybrid (sigma, p)" - "sigma" and "p" need to be defined.

p.7002 l.20-22 - this long list of references is not particularly useful to the reader. It does not seem necessary to give more than one or two representative examples.

p.7002 l.28 - what were the conclusions of Ricaud et al. (2009)? How do they impact the current work?

p.7004 l.10-12 "altitude" -> "altitudes"; "paper is" -> "paper are"; "profile" -> "profiles"

p.7004 l.15 "noise generated by the galaxy" - could you give just a few more words on what is meant by this?

p.7004 l.16 "MLS CO" -> "The MLS CO"

p.7005 l.10-11 - this sentence doesn't make sense to me.

p.7006 l.24 - If Fig. 2 shows differences in the troposphere up to -40%, how can Fig. 3 show differences of no more than -10%? There should be a consistent reference used in each case, probably RACMOBUS as this appears the more accurate.

p.7006 l.25 - "region with the intense convective activity" -> ITCZ?

p.7007 l.13 - "April maximum in NH" -> "The April maximum in the NH"

p.7007 l.15-16 - "in SH" -> "in the SH"

p.7007 l.17 "However, LINCO scheme" -> " However, the LINCO scheme"

p.7009 l.4 "this behaviour has already been reported by van Noije concerning ERA-40". Are there any relevant results concerning the ARPEGE fields used in MOCAGE? There are many more papers on this phenomenon available, in particular Monge-Sanz et al. 2007, GRL 34, L04801, which evaluates several different versions of the ECMWF system. Perhaps one of these provides a closer parallel to the ARPEGE system you use?

p.7009 l.27-28 "This indicates that LINCO does not present a global systematic bias" (versus MOZAIC) - this seems to contradict Fig. 10, which shows a clear bias between LINCO and MOPITT and MOZAIC.

p.7012 l.16-18 - I don't understand this - perhaps rewrite?

p.7015 l.15 "because of the too rapid downward transport" - this remains a hypothesis rather than a proven fact for MOCAGE, as indicated on p.7014 l. 26: "we suggest that

this deficiency is related to a too rapid ownward transport..."

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