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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraining tropospheric mixing timescales using airborne observations and numerical models" *by* P. Good et al.

Anonymous Referee #1

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The paper describes a method for the estimation of atmospheric mixing time-scales, by comparing in-situ data with results of a Lagrangian model, which is initialized with tracer distributions of an Eulerian CTM. Meteorology analyzed by the ECMWF is used for the Lagrangian trajectory calculations and for the meteorological forcing of the CTM. The method is based on a qualitative comparison of the observed variability of carbon monoxide with the results of the Lagrangian model using varying trajectory lengths. In general, the authors present their work in a clear way, and they discuss with care the limitations of the method. Nevertheless, some questions remain, which should be discussed.

1. General comments

1. It is repeated at least four times (p.1214, l.18-19; p.1223, l.24-26; p.1224, l.22-24; p.1228, l.19-21) that the results are 'very insensitive to CO photochemical change calculated along the trajectories'. How has this been tested? For CO on this time-scale, could it be sufficient to regard CO as a passive tracer along the trajectories?

2. What is the reason for only presenting a qualitative method, based on 'visual inspection' of the CO variability, whereas later, for the sensitivity study of the initial gradients a statistical method (KS test) is applied? The presentation of the method will be much more powerful, if the analyses throughout the paper are quantified. Uncertainties as described, for instance on page 1224, lines 17-21 could then be avoided.

3. Clearly, the derived mixing time-scales depend on the spatial scale (i.e., the grid size) of the initial tracer distributions provided by the CTM. How does the mixing time-scale scale with the grid size? What happens, e.g., if the initial grid size is reduced?

4. The estimated mixing times are compared to those used in global Lagrangian models, and the method is meant to constrain this free parameter (p.1215, l.23-24 and p.1216, l.2-3). On the other hand, the comparison reveals conceptual differences between the time scale used in hybrid Lagrangian-Eulerian models and the mixing times derived in the present analysis. Furthermore, the analyzed mixing times are dependent on the initialization grid (see above). Then, what is really the relevance of the analyzed mixing times for these models? And how representative are the estimated mixing times for the entire globe (and are they time dependent)? Even for the different flights in the same region analyzed in this study, estimated mixing times differ by almost one order of magnitude (explained

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by the influence of convection). Partly, these questions are already addressed, however spread throughout the text. These important issues should be worked out more clearly and discussed in more detail.

Furthermore, the paper could be significantly improved by considering the following remarks.

2. Specific comments

1. p.1214, l.22-28: The numbers presented here in the abstract are only half the information, since the corresponding spatial scale (grid size of the initial tracer distributions) is missing. Either this information should be included in the abstract (and the link between both), or these numbers should be left out. Otherwise it might be misleading.

3. Technical comments

1. p.1214, l.20 and later in the text: I think the correct form is 'i.e.', instead of 'ie.'.
2. p.1215, l.7: ('... idea of for how long ...'): It might be better to write '... idea about how long ...'
3. p.1219, l.7: '... it will almost always have the correct sign, however, underestimate the magnitude ...' is likely more clear.
4. p.1219, l.11: Better without the word 'note'.

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5. p.1219, l.24-28: It is not quite clear what is meant here. These sentences should be reformulated.

6. p.1223, l.4-5 ('..., suggesting that ...'): This sentence is not clear and should be reformulated.

7. p.1225, l.14: What is a 'slightly less strong, general result'? This should be reformulated.

8. p.1225, l.16: ('... have to to have ...'): Only one 'to'.

9. p.1225, l.17: Must it not read '... to be correct?'?

10. p.1226, l.6: '... have the same statistical distribution' is better than 'come from the same statistical distribution'.

11. p.1226, l.18: '... or so ...' is a bad formulation.

12. p.1226, l.24: '... for features to start to loose ...'. Is this really correct English?

13. p.1227, l.4: '... and therefore needs ...'

14. p.1235, Figure caption of Fig. 2: Although it is clear, the colors '(black)' and '(green)' for measurements and model results, respectively, should be mentioned.

Please note that the list of typos is most probably not complete.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1213, 2003.