

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

Anonymous Referee #2

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This paper presents a study of typical mixing times between air parcels in the troposphere, by means of numerical modeling of the chemistry and transport of CO and comparison of the model results with 2001 MINOS CO observations from different measurement flights. Two models are used in this study: a 3D CTM (TOMCAT), and the gridded results from this are used to initialize the second model, i.e., a parcel trajectory model (CittyCat). The qualitative resemblance of smaller scale mixing features is then used to establish an upper estimate of typical mixing time scales. The results are presented in a clear way, and the use of English is generally good although a few typos and incorrect grammar are found here and there. The paper raises several questions on the validity of the method which are not (fully) addressed by the authors.

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General remarks:

1- The authors already state that the mixing process in the real atmosphere is fundamentally different than the mixing as studied in the approach (page 2, 2nd column, top). This is probably not a large problem in the stratosphere and UTLS region where mixing is generally adiabatic (as in the Haynes and Shuckburg studies), but it may not be so simple when applied to the troposphere as in this study. Trajectories of up to 10 days are used here, while in the troposphere considerable uncertainty is associated with trajectories of 5 days or longer. e.g., due to convection. Since this directly affects the results it should be discussed.

2- The authors indicate that the mixing time scale applied in trajectory models depends on the parcel concentration. STOCHEM and ATTILA are mentioned as examples (page 9). However for this study, it is equally imaginable that the modeled small scale structure of CO along the flight tracks is co-determined by the resolution of the data used to initialize the trajectories (i.e., the T42 TOMCAT grid results). This would directly affect the estimation of the mixing scales. This aspect should be explored. Considering that different resolutions represent different efficiencies of grid-scale mixing, I suggest that the experiment is repeated for one of the flights but with a trajectory initialization based on TOMCAT data in a different resolution, e.g., T21 (which can be derived from the T42 data) and/or from higher resolution versions of the model (this needs a rerun of TOMCAT).

3- The determination of the mixing scale times occurs through a qualitative comparison of the observed and modeled small scale structure of the CO concentrations along the flight track. This, however, is quite subjective. Is it possible to somehow quantify the small scale structure, e.g., through auto-correlation, and use that as a base for the comparison between model and observations?

Specific remarks

Abstract: The part of the abstract 'Specificallysmall scale tracer structure.' would

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be more appropriate in the introduction. The abstract lacks information on the method used in the study.

p2, col 1, line 13: Constraining means quantification, a more quantitative method would be appropriate here as already mentioned earlier.

p2, col 1, 'The approach suggested here...' The approach has not been explained properly before this discussion starts, which makes this discussion less clear.

p2. col 2, 1st alinea: see general remark. This certainly needs more discussion for the troposphere.

p3, col 2, top section 4: '...suggests the possibility of relatively recent mixing'. Note that other studies (Scheren et al, 2003; Roelofs et al., 2003; both on APCD) suggest that this air originates from Southeast Asia and has been travelling for about 10 days. According to these studies the air is very homogeneous and apparently has no recent mixing in of other air volumes.

p4, col 2, below. How can August 7, 8 and 10 be used to initialize the August 8 CO?

p5, col 1. Based on the relative homogeneity of CO in the August 8 flight the authors suggest the possibility of recent mixing or convection. However, for the August 3 flight, a 'large amplitude' and 'small .. spatial scale' is mentioned as indication of recent mixing (page 6). This appears contradictory, can you explain this? Is it generally so that with increasing smoothness of the measurements, the mixing time is decreasing?

p6, col 2, l5-6: How do you know, why is it stratospheric, why is it the edge?

p6, col 2, l 21: What is a 'sferics location system'?

p8, Fig 10: The trajectory model produces a peak at 13.6 hr. However, this is not seen in the TOMCAT results in Fig. 1. Where does this peak come from?

p8. The KS test presented here may provide a quantitative measure as indicated above. Why restrict this only to the 11 day time scale?

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p9, col 2, bottom half: 'Thus choosing a trajectory length ... in the real atmosphere'.
This part is not very clear.

Technical remarks:

p2, col 2, line 10 from below: meteological -> meteorological

p10 col 1, middle: underestimates -> underestimated

p10, col2: initilisation -> initialization

Check capitals in the reference list, e.g., India (not india), STOCHEM, O3, North Atlantic, etc.

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