Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-601-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tracer study to estimate the transport of cruise altitude aviation emissions in Northern Hemisphere" *by* Lakshmi Pradeepa Vennam et al.

Anonymous Referee #1

Received and published: 12 September 2018

General comment

The study by Vennam et al aims at understanding the transport of species emitted by aviation with special focus on their impact on near surface values. This is an important topic and the idea to tackle the problem with a simplified numerical simulation is appealing. The paper is well written and in principal suitable for ACP. While having said this, I unfortunately have severe concerns with respect to the applied model, simulation set-up, presented results and the interpretation of the results. While the issue with the suitability of the model for this specific application might only be a lack of information (I tried to find respective results in other publication, but couldn't find them), the sim-

ulation set-up is, as far as I can judge, not supporting what the authors wish to show. More detailed information is given below. I recommend that the authors revise their manuscript with a more adequate simulation set-up and results presentation, which basically leads to a new submission.

Major Comments:

A) Model. Currently I am not able to judge whether the model is able to adequately model the dynamics of the tropopause layer. From Figure 3 I guess that the model vertical resolution is around 1 km and the model top is at 20 km. Please give a reasoning why CMAQ is capable to correctly treat the transport of species emitted at around 10 km. There is some indication that quite some counter-gradient transport might happen, since the model transports quite some amount of the tracer to altitudes of around 20 km, exceeding the surface values (Figure 3). How do you explain this transport pathway to such high altitudes in such short time?

B) While understanding the experimental set-up, I do not understand how to correctly interpret the simulation results. The quantity "Mass Fraction(surface layer)" is to zero at the beginning of the simulation. During the simulation the values increase constantly (as explained by the authors). The concentration increases everywhere in the model domain without limits and the ratio between the concentrations in the surface model layer to the column will converge to the model layer air mass to the column air mass and hence independent from the emission location. Latter because the difference between concentrations of model grid points concentration is getting small compared to the absolute steadily increasing concentration. Hence it looks arbitrary to me to take out any specific point in time. I think there is a principle problem in the interpretation of the results for this simulation set-up, which can only be resolved by a change in the simulation set-up. One possibility is to define a sink at the surface (deposition, ...) in a meaningful way. The simulation will converge to a quasi-steady state. This has also the advantage of having the possibility to check whether the results are in steady-state after three months. Referring to Grewe et al 2014 Figure 9, the water vapour

temporal evolution for cruise emissions show larger time scales, which might question the assumption of achieving a steady-state after 3 months (or actually 2 months, see below).

C) Quantities are biased by the model resolution. Results are not given independently from the model resolution (see below), which inhibits a proper interpretation, even if the comment B wouldn't apply.

Specific comments Abstract

p1 / l11 Please specify a bit more what kind of tracer you are referring to: inert gas-phase, inert particles with sedimentation, ..., what loss processes?

p1 / l12 Please explain in more detail what "tracer mass fraction means" in this context. Everything which is emitted in the atmosphere will eventually be deposited at the ground. From this perspective 100% would be expected. Near the surface is crucial. The smaller the volume the lower the percentage?

p1/l14 why "even"? It seems that the authors have expected something else. Please clarify this. p1/l16 Unclear. If something is emitted at 12 km it will always be deposited downwind. There is no direct instantaneous downward transport.

Introduction

p1/l20-22 There are indeed a couple of passive tracer studies, which were not included here, but a comparison might have been of interest. They are often not directly referring to the surface as a receptor region, but Figures are often including this information.

* Velthoven et al Atmospheic Environment 1997, is referred to below.

* Köhler, I., Sausen, R., Reinberger, R., Contributions of aircraft emissions to the atmospheric NOx content, Atmospheric Environment, Volume 31, Issue 12, 1997, Pages 1801-1818,

* Danilin, et al. Aviation fuel tracer simulation: model intercomparison and implications,

Geophys. Res. Lett., 25 (1998), pp. 3947-3950

* Schoeberl, M.R., Morris, G.A., 2000. A Lagrangian simulation of supersonic and subsonic aircraft exhaust emissions. Journal of Geophysical Research 105, 11,833–11,839

* Rogers, H. L., H. Teyssedere, G. Pitari, V. Grewe, P. van Velthoven, J. Sundet, Model intercomparison of the transport of aircraft-like emissions from sub- and supersonic aircraft, Meteorol. Z., 3, 151-159, 2002.

* Grewe, V., Reithmeier, C. and D.T. Shindell, Dynamic-chemical coupling of the upper troposphere and lower stratosphere region, Chemosphere: Global Change Science, 47, 851-861, 2002.

Some of this is referred to later in the text, but should be clarified already here, since the impression is given that those studies do not exist.

p2 / l5-19 The text might suggest that aircraft emission tagging approaches were not used previously. However, there are studies 20 years back (Brasseur et al 1998) or recently Grewe et al 2017, who use such approaches. Please clarify the text.

* Brasseur et al, European scientific assessment of the atmospheric effects of aircraft emissions, Atmospheric Environment, 32, 1998, 2329-2418 (Figure 35)

* Grewe, V., Tsati, E., Mertens, M., Frömming, C., and Jöckel, P., Contribution of emissions to concentrations: The TAGGING 1.0 submodel based on the Modular Earth Submodel System (MESSy 2.52), Geosci. Model Dev. 10, 2615-2633, doi:10.5194/gmd-2016-298, 2017.

p3/l5 I suggest to use "atmospheric transport (resolved, parameterised and unresolved)" instead of "dynamic", since the role of turbulence, diabatic heating, etc. is not investigated.

p3 l7-9 I do not understand the importance and significance of this approach. There

are several effects mixed. Continuous emission lead to continuous increase of the concentrations. At any time, the surface concentration is a snap shot and mixes emission at time T0, where a lot of the emitted species may have reached the surface with emissions at time T0+90days, where no contribution to the surface concentration is expected. Further, since the tracer is not deposited it remains in the atmosphere and artificially increases the concentration, which disagrees with the worst case assumption.

Methodology

Section 2.1: Nothing is said about the vertical extend of the domain, number of layers and especially the resolution at tropopause levels. This is an extremely important point. Large-scale transport as well as diffusivity of the transport scheme might give largely different results for too low vertical resolution or a too low model top. The large concentrations at 20 km might indicate such problems.

Section 2.2 p4 l 14

why "only"? Is there a disadvantage in this approach?

Equation (1):

I strongly advise the authors to convert this mass fraction from an extensive to an intensive quantity. In the current version “Mass Fraction (MF-layer)” is dependent on the model resolution, which is not given (see above). For example a thick layer at 500 hPa will give a large number, not because there is a lot of tracer mass in terms of concentration, but solely because the model layer is thick. One possibility is to further divide by the layer thickness to obtain %/km as unit. The consequence is that the results are resolution independent, better to be interpreted and can be compared to other model results. In Figure 3, e.g., the total amount can be obtained by summing up the values. When MF is divided by the layer thickness, the total amount can be obtained by integration.

Results

p5l11 I think, by taking the last 30 days out of a 90 days simulation period, you implicitly assume a spin-up of 60 days and 30 days simulation, right? Hence, actually you assume a much smaller mixing time than suggested in Section 2. I suggest being a little bit more specific about the lifetimes used. For example if you assume that 95% of the air is mixed after 60 days, this will result in an e-folding mixing lifetime of 20 days, which is certainly too low. A 60 day e-folding mixing lifetime will result in a 95% mixing after 180 days, which would require a much longer simulation time.

Figure 1: Units are actually moles/s/gridbox. Please provide Figures, which are not dependent of the chosen resolution, e.g. moles/s/m2 or moles/s/m3.

p5 l23: How well do you simulate tropopause folds and the associated strat-trop exchange?

p5 l23ff: The discussion is very speculative. I think you should be able to support your arguments with your simulation results. E.g. showing the tropopause fold etc.

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