Atmos. Chem. Phys. Discuss., 7, S1128–S1131, 2007
www.atmos-chem-phys-discuss.net/7/S1128/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aircraft pollution: a futuristic view" *by* O. A. Søvde et al.

Anonymous Referee #2

Received and published: 12 April 2007

General Comments:

It was very frustrating to review this paper. The figures clearly don't go with the figure captions. I eventually figured this out, but it was a hassle. Investigating the effects of aircraft on the atmosphere composition has been the topic of many research studies from CIAP to present day. Many of the statements and conclusions of this paper have been discussed prior. I believe it is important for the author to state up front what is new besides just listing the results of the aircraft perturbation.

I also found it a bit surprising that there was no reference to the decade long (1990s) study of High Speed Civil Transport studies conducted by NASA. There was a tremendous amount of research that published in the peer reviewed literature and summarized in these documents below:

Stolarski et al., 1995 Scientific Assessment of the Atmospheric Effects of Stratospheric

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Aircraft, NASA Ref. Pub. 1381, Nov 1995.

Kawa et al., 1998 Assessment of the Effects of High-Speed Aircraft in the Stratosphere:1998, NASA/TP-1999-209237, June 1999.

One of the main conclusions from this extensive effort was that before you use a model for assessment activities, you first need to evaluate the model to see if the physics, chemistry, and dynamics (+transport) processes are appropriate for the given perturbation. In this work, I see no evidence that the model has been evaluated in a manner that makes it useful for aircraft assessment calculations. In my opinion, before this paper is published, the authors need to make an effort to convince the reader that this modeling framework (CTM and meteorological fields) is useful for aircraft perturbations in the UTLS region. I will discuss this in more detail below.

Specific Comments:

Page 2533, "Several studies focusing on aircraft NOx emissions have been carried our (Hidalgo and Crutzen, 1977, ...)." This is a very select list of references. The most important and first reference on this topic is not even listed (Johnston et al., 1971).

Page 2534, Why did the authors choose the horizontal resolution of 5.6 x 5.6 degrees and the vertical extent (surface to 10 hPa (center grid))? How does resolution affect the results. The choice of horizontal resolution and vertical extent is not limited by the ECMWF meteorological fields. I believe year 2000 ECMWF operational meteorological field extend up to 0.1 hPa. The horizontal resolution can easily be T42 or T61.

It should also be pointed out that year 2000 was the beginning of a unique period for temperature, H2O vapor, and ozone (Randel et al., J. Atmos. Sci, 61, 2133-2148, 2004). In the later part of this year there was a cold bias observed near the tropopause, along with low H2O, and low O3 abundances. Assuming this is a typical 2050 meteorological year for UTLS aircraft studies may be an issue, however probably not a major one.

Page 2535. More discussion how the Oslo CTM2 handles H2O vapor is needed. I understand that you keep track of the total hydrogen, but how do you partition the H2O vapor between gas and liquid (or solid for water-ice PSCs)? Are you using the H2O from the ECMWF analysis? Do you transport total hydrogen, if so how is this handled, especially if you are using baseline H2O from the ECMWF analysis. Is the aircraft H2O advected separately? A few more sentences on how H2O is treated in the model is needed.

The authors make the point that adding NOx will produce ozone in some regions and destroy it in others. They also state that adding H2O will form more HOx and "contribute to ozone loss". This is true, however, adding HOx does not necessarily decrease ozone. There are coupling reactions between the HOx and NOx families that can minimize this. Here, it is very important that assessment papers give the reader an understanding on how realistic the model reference atmosphere is. For example, if the dynamical inputs are not accurate, one can get distributions of CFCs, HCFCs, Halons, H2O, CH4, N2O, etc.. that are not representative of observed atmospheric distributions. This will subsequently give background abundances of odd-oxygen loss radicals (e.g., OH, HO2, ClO, NO, NO2, Br, BrO) that are incorrect. The sensitivity (for ozone production or loss) of adding additional NOx or HOx radicals would then also be in error. See Kawa et al., 1998 and reference within for discussion of these issues. Some of these issue are also discussed in: Douglass, A. R., M. J. Prather, T. M. Hall, S. E. Strahan, P. J. Rasch, L. C. Sparling, L. Coy, J. M. Rodriguez, Choosing meteorological input for the global modeling initiative assessment of high-speed aircraft, J. Geophys. Res., 104(D22), 27545-27564, 10.1029/1999JD900827, 1999.

Along these lines, the ECMWF meteorological inputs, if from operational analysis, will give too young of age of air in the stratosphere. This will greatly affect background distributions of long-lived tracers and also affect conclusions on cross-hemispheric transport of aircraft emissions. Please define in more detail what meteorological fields were used. An age-of-air figure would be very useful. Comparison of odd-oxygen loss rates

ACPD

7, S1128–S1131, 2007

Interactive Comment

similar to Wennberg et al 1994 would also be informative. Simple comparisons of N2O and NOy distributions in the LS (compared to observations) would also be informative, see: Strahan, S. E., Climatologies of lower stratospheric NOy and O3 and correlations with N2O based on in situ observations, J. Geophys. Res., 104(D23), 30463-30480, 10.1029/1999JD900775, 1999.

Description of aircraft emissions. Table 1 is not adequate. What are the assumptions that go into these scenarios. It would be nice to have a little more discussion regarding the scenarios used in this study. I suspect this is written up in Rogers, 2005, correct?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 2531, 2007.