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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative forcing from particle emissions by future supersonic aircraft" *by* G. Pitari et al.

Anonymous Referee #2

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I found this paper to be extremely disappointing and cannot recommend publication. I had expected that with the quality of the authors, that this paper would be well written and a significant contribution to the literature through the detailed comparison of several excellent atmospheric chemistry models. However, it did not live up to those expectations and si rather poorly written as well. First of all, the Introduction is very poorly written as well as having a number of omissions and errors. For example, the cruise altitude of 19 km depends on the Mach number of the aircraft but this isn't even discussed; there is nothing about supersonic aircraft that says they would fly at about 19 km except for the choice of Mach number. Stating that HSCT emissions will result in an ozone column decrease also depends on the altitude of the emissions (also note that no references are provided for their statement of an ozone decrease). Along with

a number of English errors throughout the paper, many references are also missing throughout the Introduction and for that matter, the rest of the paper. On page 2 of the introduction, the authors discuss solar effects but neglect to mention any IR effects affecting aerosol radiative forcing. The SLIMCAT description mentions T15 without explaining what this resolution means; 8 x9 degrees is an awfully coarse resolution model for doing chemistry and/or transport studies in the atmosphere. At the bottom of the section, the authors mention SCENIC without even explaining what that is. The section on scenarios on page 3 basically refers to a gray literature report rather than explaining exactly what the scenarios are. They need to describe the scenarios fully in this paper. Also, why do they use an abbreviation of Tab. for Table? Page 4 starts with a discussion of Mach number for the various scenarios but then doesn't say what cruise altitude these Mach number s correspond to. Section 4 is where the paper gets really disappointing; a discussion is made of derived particle profiles relative to observations, but for only one modle, the ULAQ model. Why aren't all of the models compared with observations and why aren't they analyzed with much more extensive detail than provided here? On page 4, the authors also mention extinction profiles but then don't provide any graphics to explain the comparisons with SAGE II and HALOE; but then go on to say they compare well. Unbelievable! Section 5 on radiative forcing is also only for the ULAQ model. The other models should have been included in these analyses (more on this later; I know Figure 7 has some results from all models but that figure has other problems). The authors then describe what appears to be an antiquated radiative transfer model used by ULAQ where the newest reference about the basis seems to be 16 years old and the oldest 40 years old; I think there have been a few improvements in representing solar and IR absorption and scattering since those papers. The tables with this section mean nothing because there is nothing compared or data alluded to. The problem with Figure 7 is that it uses the DLR radiative transfer model but there is no comparison with the ULAQ model to set any kind of standard. Also, there is no way to tell what model got what result other than an overall range. Section 5.2 finally brings in some comparison of the models for chemistry results, but even here the discussion

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is cursory and incomplete, e.g., saying that reasons for model differences is due to transport but not proving this is the case. I really think that the models should have been compared in detail throughout the paper, including the radiative transfer studies. The conclusions are misleading in implying that was done.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5091, 2008.