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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating the global atmospheric black carbon cycle: a revisit to the contribution of aircraft emissions" *by* J. Hendricks et al.

Anonymous Referee #1

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General Comments

The paper describes simulations of black carbon aerosol from aircraft emissions in the upper troposphere with the ECHAM model. Effects of aircraft emissions on tropospheric chemistry and climate are currently not well understood and could be potentially large. Results of this study give evidence for a relatively minor role of aircraft emissions on upper tropospheric carbon aerosol concentrations. The study represents an interesting contribution to ongoing scientific discussions and I would recommend publication of the paper after major revisions.

The authors undertook great efforts to develop a GCM that can be used to simulate carbon aerosols from aircraft emissions. However, my main concern is that the con-

clusions in this paper are based on considerably uncertain model results and a rather artificial approach (see specific comments for details). It appears that these uncertainties are not appropriately addressed in the paper. Also, the descriptions of the approach and results need to be reorganized in the manuscript. There is a general lack of focus in many parts of the paper which makes the paper quite difficult to read.

Specific comments

(1) Basic model approach. Uncertainties in the model results can be generally attributed to inaccuracies in the representation of transport processes and treatment of aircraft emissions and aerosol processes in the GCM.

The vertical resolution in ECHAM (19 levels) is likely not sufficient to accurately simulate tracer transport in the upper troposphere. Transport processes in the upper troposphere may be considerably affected by mass and energy fluxes across the tropopause. These fluxes cannot be realistically represented in a model that does not resolve the mean stratospheric circulation and wave activity in the UTLS region. However, even GCMs which are designed to simulate stratospheric processes produce fluxes at the tropopause level which may vary considerable among different models (e.g. Austin et al., 2003). Furthermore, simulated tracer results for the upper troposphere are substantially affected by inaccurate advection algorithms in GCMs. The semi-lagrangian transport method (which is used in this study) is highly diffusive in typical GCM applications. A less diffusive transport method might result in considerably (i.e. probably at least a factor of 2) reduced aerosol concentrations from lower tropospheric sources in the UT for the simulations presented in this paper.

Another important cause of uncertainty is the treatment of aerosols sources and chemical/microphysical processes in the GCM. If I understand correctly, the approach in this study does not explicitly account for any subgrid-scale effects and the initial time evolution of aircraft emissions. Emissions from aircraft represent a strongly localized source of aerosol mass in the upper troposphere. Typical contrail growth rates are on the or-

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der of a few metres to tens of metres per minute (Jager et al., 1998). This means that chemical reactions and mixing of aerosols are spatially confined to very small fractions of the GCM grid cells, at least during the first hours after emission. Additionally, the emissions are highly episodic in nature so that it appears to be problematic to use seasonal mean emissions in the model (which should, for example, lead to unrealistically small coagulation efficiencies). Efficiencies of mixing and chemical processes have been shown to be very different in contrails compared to background aerosols (e.g. Wu and Menon, 2001). The authors should demonstrate whether their approach is able to accurately simulate aircraft emitted aerosols based on comparisons with observations and/or other model results (e.g. models with sufficiently high resolution and/or appropriate conceptual models). Sensitivity tests with the GCM may also be useful to investigate the potential ranges of uncertainty.

(2) P. 3488, discussion of results of other similar studies. More specific results should be added, i.e. how do the aerosol burdens simulated by Koehler et al. (2001) compare to the aerosol burdens simulated in this study?

(3) Section 2 and Appendix, model description. Frequent references between these parts and other sections should be avoided to make this part easier to read. The organization of the model description section is not clear at all. Why are obviously important parts of the model description in the Appendix? On the other hand, main model assumptions are not clear due to a large amount of additional and very detailed information (just one example: discussion near the end of section 2.2).

(4) Section 2.4. It seems that the treatment of aerosol from aircraft emissions and from other tropospheric sources is inconsistent. Different assumptions are made for hygroscopicity, aerosol number, size distribution, and aging processes for aerosols from lower tropospheric sources and aircraft emissions. It is not clear whether there are any processes in the model that would allow these types of aerosols to mix with each other, e.g. via coagulation. If mixing of these two types of aerosol is indeed possible in the model, are the mixtures parameterized as tropospheric aerosols or as aircraft emitted

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aerosols? Differences in physical/chemical characteristics and treatment of processes for these types of aerosols should be well described and explained. Furthermore, it should be explained whether (or not) the treatment in the model might lead to an inconsistent representation of mass and number size distributions for the carbon aerosol. I don't think that a consistent representation of aerosol mass and number is generally possible in a bulk model like the one described here. Bin or modal models would likely be more appropriate.

(5) P. 3494, line 11: "As shown in the Appendix...". I couldn't find this in the Appendix.

(6) P. 3494, Eq. 2. Is the use of EI(N) appropriate in this equation? It seems that this equation is for aged aerosol and EI(N) is for freshly produced aerosol. The main assumption behind this approach and motivation are not clear.

(7) P. 3498, line 25: The results shown in Fig. 4 differ by up to one order of magnitude for different experiments yet the authors seem to claim that the differences are minor. A more quantitative and qualitative discussion of the differences appears to be necessary.

(8) P. 3501. The authors appear to claim that there is good agreement between model results and observations, yet they write on page 3503 that "Due to the lack of extensive observations...it is currently not possible to evaluate the quality of the simulations...". It should be indicated why and to what extend the comparisons with observations are useful in this study. In particular, a removal of the BEA03 data from the comparison would yield a systematic overestimate of concentrations for all model versions. BEA03 seems to be a small data set and it may be that it is not representative in a statistical sense. The purpose of the comparison in this section is generally not clear at all and I would recommend to completely rewrite or remove this discussion from the paper due to the large uncertainties that are associated with the observed data.

(9) P. 3503, line 27-: "The absolute aviation impact is large during summer when the vertical inflow from layers carrying higher amounts of aircraft BC is largest". There shouldn't be a large number of low-flying aircraft over the North Atlantic and so it should

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not be expected that concentrations in the UT are considerably affected by upward transport of emissions from low-flying aircraft in this region in summer.

(10) P. 3505 (near the end) - P. 3506. The reader may not be familiar with Danilin et al.'s (1998) results and more details should be added. Also, I don't understand the discussion in this section at all. What is that the authors want to say in this part?

(11) P. 3508-3509. What is the purpose of the discussion in this section and the apparently pointless references to other sections? Is this still part of the model description? Again, a much more coherent description of the basic assumptions and the approach is necessary. It might be useful to add a schematic to summarize the chemical species and processes.

(12) P. 3513. Eq. A1. Is the coagulation efficiency valid for a GCM or for the conditions in (fresh or old) contrails? This formula does not produce any changes in Nf for Nl=0. How does the model treat coagulation of aerosol from aircraft in the absence of background aerosol? A discussion of subgrid-scale effects and time-evolution of aerosols from aircraft emissions is missing.

(13) P. 3513, general. Is coagulation according to Eq. A1 the only aging process for the contrail aerosol number concentration and is the same aging process also applied to the aerosol mass budgets? It seems likely that hydrophylic aerosol from aircraft emissions would also age by condensation and evaporation of sulphur species and in-cloud chemical processes. Has this been accounted for and how?

(14) P. 3513, last paragraph, p. 3514, top. It appears that different assumptions are made for the size distributions for aircraft emitted aerosol in this section and for background aerosol on p. 3491? Generally, differences in the assumed aerosol size distributions should be explained, i.e. it is not clear why sizes of aircraft emitted aerosols should be any different from sizes of background aerosols for aged aircraft emitted aerosols. Physical reasons should be given for differences in assumed aerosol sizes for background, fresh and aged aircraft emitted aerosol. Furthermore, it is not clear

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from the description whether the calculated aerosol number and mass in the model are consistent with the assumed size distributions in this section.

(15) P. 3541, line 5: "Since scavenging is more efficient for particles with different sizes than for particles showing comparable sizes..." This seems to be very misleading and should be rewritten.

(16) P. 3514, last 10 lines. Have contributions from other aerosol species, e.g. sulphate, been considered in this comparison and if not, why? Is this comparison based on results for Nf, Na, Nl or for the sum of these? Also, there are no results shown that would support these findings. Supporting results should be added. Alternatively, this section should be removed.

(17) P. 3515, first 11 lines. I don't understand the motivation for the sensitivity test with increased value of Nl. What is the evidence that this value is too low in the simulation and what is causing this? Why an increase by a factor of 5?

(18) P. 3515, last three lines "...not only the BC processing by background aerosols but also the cloud processing if BC from aircraft is chosen to be very efficient in the model...". Is there any observational or theoretical evidence for this? What are the basic assumptions in the model for this process? From this and other descriptions would the authors agree that their approach is rather artificial in nature? It seems that a large number of processes are treated in the model without any sufficient physical basis. This raises the fundamental concern that the results presented in this paper are not well enough constrained and that different parameter tuning may lead to very different results. The authors should put considerably more efforts in a sound description of the physical basis of their approach and identify the tunable parameters. If appropriate, evidence should be added to establish relations between uncertainties in model results and uncertainties associated with certain processes. Sensitivitiy tests should be performed for at least some of the processes to study the effects on model results (e.g. simulations without any aging and perfectly efficient aging processes).

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Interactive Comment

(19) P. 3516, line 21: "Since the details of these interactions are not well understood yet, the number concentration of the aged BC particles is not simulated here". You have lost me here again. It seems number concentrations are compared on p. 3514. It is not clear how the comparisons are done and for what type of aerosol the number concentrations are calculated.

(20) English language. The use of certain formulations (e.g. repeated use of "delimit") is confusing at times. It is not possible to list all of those here and the manuscript should be checked and revised accordingly.

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