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Interactive Comment

Interactive comment on "Transport Modelling of a pyro-convection event in Alaska" *by* R. Damoah et al.

Anonymous Referee #1

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Review of Damoah et al., 2005

The manuscript by Damoah et al. presents results from model simulations and remote sensing observations to conclude that pyro-convection in Alaska and the Canadian Yukon Territory transported fire emissions into the UT/LS region in June 2004. The model simulations are conducted with the particle dispersion model FLEXPART that includes a treatment of convective transport. The latter was shown to be essential for the performance of the model. Remote sensing observations include TOMS, SAGE II, POAM III, and ground-based lidar measurements. All observations provide evidence for enhanced aerosol concentrations in the UT/LS. These elevated aerosol concentrations could be traced back to an area of particular intense pyro-convection in the Yukon

Territory on 23/24 June 2004.

This interesting paper adds valuable information to the growing database of pollution from biomass burning in the UT/LS region. The topic of the paper is well suited for ACP. Enclosed are my specific comments that should be considered before publication of the manuscript in ACP.

Specifc Comments:

Title: The title of the manuscript is misleading. The vertical transport in the FLEXPART model is not induced by the pyro-convection event itself, but is rather realized by 'regular' deep convection described by the convective paramerization.. This parameterization 'does not account for processes related directly to the fire' (p. 6198), so pyro-convection (defined as 'deep convection triggered or enhanced by forest fires', p. 6187) is not considered in the model. The authors do, however, provide convincing evidence from satellite observations, that pyro-convection occured in the Yukon Territory at the time of the investigation. While the event was pyro-convection, I do not think that this was actually simulated by the model as suggested by the title. Also slightly misleading is the fact, that the most intense fires did occur in the Yukon Territory, i.e., not in Alaska. This should also be represented in the title.

page 6186, line 23: maybe add some more recent reference for the role of fire emissions for atmospheric chemistry (e.g., Crutzen and Andreae, 1990, Science, 250, 1669-1678) and the atmospheric radiation (e.g., Iacobellis et al., JGR, 1999, 104(D10), 12031-12045)

page 6187, line 8: the reference Fromm et al., 2003, should be Fromm and Servranckx, 2003; the reference Fromm et al., 2004, should be Fromm et al., 2005

page 6187, line 16, remove 'also', maybe change 'tropical biomass burning plumes' into 'tropical pyro-convection'.

page 6187, line 19: Maybe add some reference to the statement that absorption by BC can lead to further lifting through heating, these may include: Westphal and Toon, 1991, JGR, 96(D12), 22379-22400 (model simulation, regional scale) , Herring and Hobbs, 1994, JGR, 99(D9), 18809-18826 (observations from the Kuwait oil fires), Trentmann et al., 2002, JGR, 107(D2), 4013, doi:10.1029/2001JD000410 (model simulations, small scale)

page 6187, line22ff: Several articles are cited with reference to other cases where pyro-convection transported fire emissions into the stratosphere. Only two of these articles link their stratospheric observations of fire pollution with pyro-convection (as opposed to regular deep convection), i.e., Fromm and Servranchx, 2003 (refered to as 'Fromm et al., 2003') and Fromm et al., 2005 (cited as 'Fromm et al., 2004'). The articles by Jost et al., 2004, and Livesey et al., 2004 only suggest that pyro-convection was responsible for the vertical transport, while Immler et al., 2005, conclude that their analysis does 'not support the idea of a direct injection of the forest fire smoke from thunderstorms created by the fires themselves.' (their page 353). I suggest to modify the citations of these articles accordingly.

Page 6189, line 4: the recent ACP article by Stohl et al. should be cited as a reference for FLEXPART: Stohl et al., 2005, Atmos. Chem. Phys., 5, 2461-2474.

I suggest to include more information about the model in the manuscript. Especially the treatment of convection, which is central for the investigations presented here, should be explained in more detail. The vertical resolution of 1000 m seems rather coarse for a study focussing on convective troposphere-stratosphere exchange. Was the robustness of the simulation results tested by using higher vertical resolutions? It should be noted that the calculations of the convective fluxes in the parameterization are performormed on the original ECMWF grid with 60 vertical layers. How are the particles redistributed onto the coarse grid after the convection parameterization? How is ensured that particles that remain in the troposphere after convection (on the ECMWF grid) are not placed into a box that, at least partially, resides in the stratosphere (on the FLEXPART grid)? I can imagine, that the redistribution from the high-resolution

ECMWF grid to the coarse FLEXPART grid could introduce artificial vertical diffusion into the stratosphere.

page 6189, line 28: How is the temporal interpolation from the ECMWF output onto the FLEXPART timestep done?

page 6190, line 2: Include ’and the Yukon Territory’ after ’Alaska’.

page 6190, line 5: Please give a reference for the MODIS hot spot data, are they available via the internet?

page 6190, line 17: The references should read (Damoah et al., 2004; Spichtinger et al., 2004)

page 6190, line 19: The initial injection height of large crown fires in Canada is well above 3 km; Lavoue et al., 2000, JGR; 105(D22), 26871-26890 estimate a mean injection height of 7.6 km. How sensitive are the results towards the initial height of the fire emissions?

page 6190, line 19ff: I do not agree with the statement regarding the temperature effects of fires being introduced into ECMWF data. Fires destabilize the atmosphere through their release of heat. The spatial extent of this temperature effect, however, is rather limited, and does not extend much further than some km from the location of the fire. I do have my doubts that these local temperature anomalies are introduced into the ECMWF model through assimilation of temperature observations. Please specify, how the destabilizating effects of fires are introduced into ECMWF analysis data.

page 6190, line 27ff: please add a reference to the statements that maximum daily temperatures are related to the number of lightning strike, and that the number of lightning strikes is related to the number of started fires.

page 6191, line 12ff: on 14 and 15 June, temperatures were not particular high compared to the days before and after. The causal relationship between high temperature and lightning is not obvious. Please comment.

page 6193, line 18: please include a reference for the statement that a temperature profile with a strong gradient and no structure, but with a sharp tropopause is characteristic for an environment sharped by deep convection.

Figures 7: I suggest to plot the temperature profiles in a skewT-logp-diagramm, which allows a more easy identification of the convective potential of the radiosonde profile. It might also be helpful to include the value of CAPE for both profiles.

page 6195, line 8ff: Based on FLEXPART results the authors conclude that the aerosol feature seen in the TOMS aerosol index close to the Beaufort Sea (red rectangle in Figs. 10a and 10b) is located at altitudes below 9 km. I suggest to consider also other possibilities in this discussion (e.g., underestimation of the vertical transport by the model, uncertainities in the initial injection height). Especially since this feature exhibits the highest value of the TOMS aerosol index, which is very sensitive to the elevation of the absorbing aerosol, suggesting that the smoke resides at high elevation, in contrast to the FLEXPART results.

page 6196, line 4: The two profiles showing enhanced aerosol loading are based on POAM III and SAGE II data. I am confused by the sentence 'The two POAM profiles ... four-fold enhancement', I guess the authors are referring to the polluted POAM and SAGE profiles. Please clarify.

page 6196, line 8ff: I do not understand the argumentation that the presence of forest fire CO in the FLEXPART model above the tropopause close to the location of the enhanced aerosol extinction as measured from POAM and SAGE underlines the suggestion that a pyro-convective blow-up was responsible for the vertical transport. In my opinion, as stated above, vertical transport by pyro-convection is not included in the model, suggesting that, in the model, other processes are responsible for the vertical transport, i.e., regular deep convection. From the observation, however, it does seem very likely that indeed pyro-convection was responsible for the vertical transport of the smoke.

page 6196, line 13: 'Fig. 10d' should read 'Fig. 10f'.

page 6197, line 19ff: The photo of the pyro-convection is impressive, but I suggest not to draw too many conclusions from it. I agree with the statement, that it shows how 'smoky the pyro-convection was'. Based on this photo, the suggestion that microphysical processes are actively influencing the release of latent heat, however, seems to be rather far-fetched, and should be omitted here.

page 6198, line 10ff: I do not understand how the FLEXPART simulations benefited from the assimilation of the close-by (how close to the fire?) radiosonde data into the ECMWF model. If the radiosonde showed signatures of active deep convection, it should be a rather stable profile and therefore not favor convection in the model. To favor deep convection in the ECMWF data, I would assume, that assimilation of an instable radiosonde profile would be best suited. For the case of pyro-convection, this could be achived in the case that the temperature anomaly from the fire is present in the profile, but I do not think that this is the case for the profiles shown here.

page 6198, line 16ff: This paragraph about the findings by Cammas et al. does not belong to the summary and conclusions, but rather to the introduction. Since it is already mentioned on page 6188, line 17ff, so it should be removed here. Also it should be clarified if this work presents evidence for pyro-convective or convective TST. Boths terms (pyro-convection and deep convection) are mentioned.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 6185, 2005.