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

R. Damoah et al.

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Reply to reviewers

Referee 1

Specific Comments

Title: We do agree that the title of the manuscript is somewhat misleading. You are right the vertical transport of the model is induced by regular deep convection rather than pyro-convection, although we provide satellite observations of what seems to be pyro-convection. We have therefore changed the title to 'A case study of a pyro-convection event using a transport model and remote sensing data'

Page 6186, line 23: We have added the following references (Crutzen and Andreae,

1990) and (Iacobellis et al., 1999).

Page 6187, line 8: The references (Fromm et al., 2003) and (Fromm et al., 2004) have been changed to (Fromm and Servranckx, 2003) and (Fromm et al., 2005) respectively.

Page 6187, line 16: We have removed 'also' and changed tropical biomass burning plumes to tropical pyro-convection.

Page 6187, line 19: The following references have been added (Westphal and Toon, 1991; Herring and Hobbs, 1994; Trentmann et al., 2002)

Page 6187, line22ff: We agree to your suggestion to modify the citations with respect to cases of pyro-convection. We have therefore removed (Immler et al., 2005) and change the sentence to 'several recent papers have presented dedicated case studies of pyro-convection events (Fromm and Servranckx, 2003; Fromm et al., 2005), or cases that can likely be explained by pyro-convection although the authors did not explicitly suggest pyro-convection to be the transport mechanism (Livesey et al., 2004; Jost et al., 2004)?.'. '.

Page 6189, line 4: Stohl et al., 2005 has been cited as reference to FLEXPART. Furthermore more information especially about the treatment of convection has been included (The scheme requires the grid-scale temperature and humidity fields as input and calculates as a by-product a displacement matrix for each horizontal grid box. The matrix provides the necessary information for the particle redistribution which is performed on the same vertical levels as the input data. The convection scheme is called every FLEXPART time step which is 900 s. The temperature and specific humiditiy are read in from the meteorological data and linearly interpolated to the current time). We agree that 1000 m resolution is somewhat coarse, but it is used here only for the output grid, whereas the treatment of transport is based on the full resolution of the ECMWF analyses Thus, the resolution of the output grid does not determine the degree of numerical diffusion in FLEXPART. As long as the model output shows a 1000-m layer that is located entirely above the tropopause to be affected by the convection, the transport across the tropopause is fully resolved. This is the case here (see, e.g., Fig. 8a).

Page 6189, line 28: The temporal interpolation from ECMWF analyses to a given time linearly.

Page 6190, line 2: the Yukon Territory has been included after Alaska.

Page 6190, line 5: The website (i.e. (http//map.ngdc.noaa.gov/website/firedetects/viewer.htm) where MODIS hot spot data are available has been included.

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

Page 6190, line 19: We do agree that initial injections height of some fires is well above 3 km as estimated by Lavoue et al., 2000. However, emissions actually take place at the ground and the depth of the initial "injection" should ideally be determined by the model. The injection is a convective process that, in a coarse-resolution model, should be handled by the model's convection scheme. One purpose of this paper was exactly to see how well FLEXPART's convection scheme can deal with this situation. Thus, emissions were injected only into the lower troposphere and the convection scheme was used to redistribute the emissions. This seems to have worked reasonably well in this case and, therefore, there is no need to invoke a rather arbitrary "effective injection" height. We agree, however, that the convection scheme may not always perform so well and, thus, in these cases effective injection heights are still required.

Page 6190, line 19ff: We do agree to the fact that the effects of the fires are rather local and, therefore, may not, or not well, be represented in the ECFMWF data. However, the fires were relatively large and it is also possible that observations have been assimilated that indeed do point the ECMWF analyses into the right direction. It is impossible for us, though, to know to what extent indirect fire information was assimilated. Hence, we have restructure the statement as 'It must be noted that all the processes related to the fire itself are not treated in the ECMWF model. To some extent the effects of the fires (e.g., a destabilization of the atmosphere) might have been indirectly introduced into the analyses through the assimilation of observations (e.g., temperature data from satellites and radiosonde data from the vicinity of the fires). However, these effects are rather local and it is not clear whether observations that were affected by the fires indeed entered the ECMWF data assimilation. It is, thus, likely that convective transport over the fires is underestimated by FLEXPART.'

Page 6190, line 27ff: (Rorig and Ferguson, 1999) has been cited as reference for relationship between maximum daily temperatures, lightning strikes and number of started fires.

Page 6191, line 12ff: We have removed the statement 'The high temperatures also caused a large number of lightning strikes.' Since the relationship between high temperature and lightning is not obvious especially on 14 and 15 June. This was not meant to refer exactly to those days because many of the fires were already started earlier (when temperatures were indeed here). It was only on those days that severe convection developed over the fires.

Page 6193, line 18: The reference (Salby, 1996) has been cited for the statement temperature profile with strong gradient and no structure, but with a sharp tropopause is a characteristic for an environment sharped by deep convection.

Figure 7: We have rather plotted potential temperature which allows easy identification of convection.

Page 6195, line 8ff: We totally agree to your suggestion that other possibilities have to be considered (e.g. uncertainties in the injection height and underestimation by the model). We have changed the sentence from 'Thus we contend that the feature is located at lower (< 9 km) altitude.' to 'Thus this feature may be located at lower (< 9 km) altitude.'

Page 6196, line 4: There are two POAM profiles available, an enhanced one (orange) over Edmonton and an unperturbed one (black) over Quebec. We clarify the statement as 'The two POAM profiles (orange and black profiles in Fig. 11a) above 12 km show an approximately four-fold enhancement of the aerosol extinction of the perturbed (orange) relative to the unperturbed profile (black).'

Page 6196, line 8ff: We agree to the fact that pyro-convection is not included in the model, and that the vertical transport of the forest fire CO is due to other processes, i.e. regular deep convection. However, there is no generally accepted definition for pyro-convection at least for now, so one cannot clearly draw a line between regular deep convection and pyro-convection. Also, the model is using analysis data that might have seen assimilation of some data that were affected by the convection (be it pyro-, or not). But pyro-convection seems likely from the observation. We have clarify this in the text.

Page 6196, line 13: Fig. 10d now read Fig. 10f.

Page 6197, line 19ff: We do agree to the suggestion that the statement based on the photo that 'microphysical processes are actively influencing the release of latent heat in the atmosphere' be omitted.

Page 6198, line 10ff: We have changed the discussion of the data assimilation throughout the manuscript (see also response to other points related to this). The problem is that we do not know which data actually entered the ECMWF data assimilation and how it affected the analysis. A lot of surface and satellite data are also assimilated, so it is not unlikely that some of them did in fact show an unstable temperature profile. Furthermore, even the assimilation of a rather stable profile could destabilize the ECMWF analyses (perhaps at a somewhat later or earlier time) as long as the profile is still less stable than the background forecast from the ECMWF model. It all depends on how the various temperature informations entering the assimilation compare to the ECMWF forecast at the same location and time.

Page 6198, line 16ff: The paragraph about the findings by Cammas et al. at the summary and conclusion has been removed.

Referee 2

Specific comments

1. As of now, no climatology of pyro-Cb events exists. North American events have been studied primarily because of the much better data situation. It is likely, though, that similar events also happen over Siberia. In fact, extreme values of the TOMS aerosol index (AI) are often indicative of a pyro-CB event, and a preliminary climatology of high TOMS AI values by one of the co-authors (M. Fromm) also shows events over Siberia. However, it is possible that these events are less frequent over Siberia because more of the Siberian fires are surface fires, which have lower injection heights (Lavoue et al., 2000).

2. We do agree to your suggestion that Fig. 8 is not necessary, hence we have removed it.

Technical corrections

Page 6187, line 2: Novelli et al., 2005 now read Novelli et al., 2003.

Page 6187, line 14: thence has been changed to hence.

Legends and titles in figure 3, 7, 8, 9, 10, and 11 have been enlarged.

References not included in the paper

Lavoue, D. C., Liousse, C., Cachier, H., Stocks, B. J., and Goldammer, J. G.: Modelling of carbonaceous particles emitted by boreal and temperate wildfires at northern latitudes, J. Geophys. Res., 105, 26871 - 26890, 2000.

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