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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Around the world in 17 days – hemispheric-scale transport of forest fire smoke from Russia in May 2003" *by* R. Damoah et al.

R. Damoah et al.

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Referee # 1

General Comments

We agree to the fact that this event was widely observed by the scientific community in real time using satellite observations. However, in this paper the case is described in the scientific literature for the first time. Furthermore, we concentrate on transport model simulations. To our knowledge, this is the first time a pollution plume could be tracked by observations for such a long period of time and where a transport model simulation could be validated over a period of more than 2 weeks. We will try to emphasize that fact in the revised version of the manuscript, as you have suggested.

Specific Comments

(1) We agree to your suggestion that the statement is a bit strong, so we have changed it from 'This is perhaps the first time that air pollution was observed to circle the entire globe.' to 'Not many events of this kind, if any, have been observed, documented and simulated with a transport model comprehensively.'

(2) You are right that the fires in Russia are not special, but most of the studies into Russian fires are more recent as compared to Canadian fires which have been under study for quite a long time already. And so we have rewritten the sentence as 'Despite the large areas burning in Russian forests almost every year, until recently relatively little attention has been paid to fires there compared to Canadian fires. However, recently Siberian forest fires have been the subject of several studies' (Yoshizumi et al., 2002; Conard et al., 2002; Kasischke and Bruhwiler, 2003).

(3) Recent estimates of the annual area burned in Russia vary considerably. Partly this is due to the large interannual variability and a strong increase in fire activity since the late 1990s. Yoshizumi et al. (2002) estimated that about 11 M ha were burned in 1998; Kasischke and Bruhwiler (2003) reported a value of 12 M ha for the same year; Lavoue et al. (2000) gave a long-term (1960-1997) annual average of about 4 M ha. The whole paragraph now reads 'A long-period (1970-1999) average estimate of burned areas for all Russian forests and tundra is 5.1 x 10^6 ha yr^{-1} (Shvidenko and Goldammer, 2001), Lavoue et al. (200) gave an annual average of 4 x 10^6 ha yr^{-1} (1960-1997), but some other estimates are as high as 10 - 12 x 10^6 ha yr^{-1} (Conard and Ivanova, 1998; Valendik, 1996). In fact, recent estimates of the annual area burned in Russia vary considerably. Partly this is due to the large interannual variability and a strong increase in fire activity since the late 1990s.'

(4) The fact that we use a passive tracer means that observed structures should always be present in the model results, too. However, the reverse is not necessarily true because where washout has removed the aerosols, the simulated tracer structures

ACPD

4, S837–S844, 2004

have no correspondence in the satellite images. As the emphasis in this paper is on the transport, we used a passive tracer representing CO so that observed structures will always be present in the model results for qualitative comparison. In the revised version of this paper we will omit the sentence "In this paper, we do not compare the CO tracer with measurements of CO because this was done previously in a similar study of a Canadian fire event (Forster et al., 2001)." and re phrase the next sentence as 'As the emphasis in this paper is on the transport, we used a passive tracer (CO) not undergoing removal processes so that observed structures will always be present in the model results for qualitative comparison.

(5) We think you are correct that some of the variability is artificial because clouds occasionally mask the fires from satellite detection. We have acknowledged that in the paper. However, some of the variability (perhaps the larger part) is real and any averaging procedure would also average out this real variability. Without having more accurate information available than the hot spot data, we think it is not possible to come up with a procedure that reliably eliminates artificial variability while preserving real variability.

Minor technical comments

(1) We think we did not state in the paper that GFS analysis data has 3 hrs temporal resolution. What we said was that 6 hrs analysis data are supplemented by 3 hrs forecast step data to provided 3 hrs temporal resolution. This is done for both ECMWF and GFS data.

(2) Ejection height is one of the problems of model simulation as we acknowledged in the last sentence of the section 'Tools and methodology'. But sensitivity studies performed on this event by varying the upper release level from 0.5 km to 4 km altitude did not change the results much (less than 4% of the global mean concentration). We have added; 'Sensitivity studies performed on this event by varying the upper release level from 0.5 km to 4 km altitude did not change the results much (less than 4% of the global

ACPD

4, S837–S844, 2004

mean concentration).' to the last sentence of the section 'Tools and methodology'.

Referee # 2

Major Comments

(1) Introduction:

a. We totally agree that we have to give a time line, so we have added the sentence 'According to model simulations, the time scale of intercontinental transport of pollutant emissions is on the order of 3-30 days (Stohl et al., 2002). The upper range of this estimate may be a typical time scale for the mixing of pollutants in the northern hemisphere middle latitudes. In case studies, Wotawa and Trainer (2000) reported a duration of about 2 weeks for the transport of Canadian fire emissions to the southeastern United States, Forster et al., (2001) quoted a period of about 1 week for the transport of Canadian fire emissions to Europe.

b. The sentence has been rewritten for clarity as; 'Wotawa and Trainer (2000) found that the high CO and O_3 concentrations over southeastern United States in 1995 over a period of 2 weeks were caused by the transport of a pollution plume from Canadian fires and photochemical ozone formation in this plume.'

c. Yes, there are several papers on Russia fires but most of the studies are more recent as compared to Canadian fires, which has been under study for quite long. And so the sentence has been changed to; 'Despite the large areas burning in Russian forests almost every year, until recently relatively little attention has been paid to fires there compared to Canadian fires. However, recently Siberian forest fires have been the subject of several studies (Yoshizumi et al., 2002; Conard et al., 2002; Kasischke and Bruhwiler, 2003, Shvidenko and Goldammer, 2001, Shvidenko and Nilsson, 2000, Soja et al., 2004).'

d. We did discuss the burned areas for Russian forests, the long-period average quoted covers the time line 1970-1999. Within this time line, the extreme years recorded are

ACPD

4, S837–S844, 2004

1987 and 1998, but 2003 was even worse judging from the area burned. The whole paragraph now reads ; 'A long-period (1970 - 1999) average estimate of burned areas for all Russian forests and tundra is 5.1 x 10^6 ha yr^{-1} (Shvidenko and Goldammer, 2001), Lavoue et al. (200) gave an annual average of 4 x 10^6 ha yr^{-1} (1960 - 1997), but some other estimates are as high as 10 - 12 x 10^6 ha yr^{-1} (Conard and Ivanova, 1998; Valendik, 1996). In fact, recent estimates of the annual area burned in Russia vary considerably. Partly this is due to the large interannual variability and a strong increase in fire activity since the late 1990s. 1987, when 14.5 x 10^6 ha of forest and other lands were destroyed was an extreme year. Assuming typical emission factors (Andreae and Merlet, 2001), this contributed about 20% of CO_2, 36% of CO and 69% of total CH_4 produced by savanna burning during an average year (Cahoon et al., 1994). 1998 was another severe year when about 12 x 10^6 ha were destroyed according to recent estimates (Kasischke and Bruhwiler, 2003). It was even worse in the year 2003.'

e. Since 19 M ha (190,000 sqkm) is almost the size of Iraq, we have put; '(Slightly less than the size of Iraq)' at the end of the sentence; 'At the end of the 2003 fire season, more 19 x 10^6 ha of land had been destroyed in Russia 19 x 10^6 ha.'

(2) Tools and Methodology:

a. We agree totally that the description of the instrument should contain more information and references. Hence we have changed paragraph 3 to; 'Other platforms that observed the smoke were Total Ozone Mapping Spectrometer (TOMS) aboard the Earth Probe satellite which provides data on UV-absorbing tropospheric aerosols including smoke from biomass burning (Hsu et al., 1999). And the Sea-viewing Wide Field Sensor (Sea WiFS) (Hook et al., 1993) aboard the Sea Star spacecraft, which operates in 8 wavelength channels ranging from 403-887 nm but uses channels 765 and 865 nm for the estimation of aerosol radiance (Gordon and Wang, 1994).'

b. In the previous study, Forster et al. (2000) made a quantitative comparison of FLEXPART CO tracer concentrations from Canadian forest fires with measurements,

ACPD

4, S837–S844, 2004

whiles this paper aims at qualitative comparison. However for clarity, we have omitted the statement; 'In this paper, we do not compare the CO tracer with measurements of CO because this was done previously in a similar study of a Canadian fire event (Forster et al., 2000).' See also the discussion on point 4 by reviewer 1.

(3) Results:

a. In figure 1, GOME NO_2 data were used, because nitrogen oxides are critical for the formation of ozone, and it was important to confirm previous work by Spichtinger et al. (2001) that emissions of nitrogen oxides can indeed be detected by GOME. Although we do not show it in this paper, we have also used studied MOPITT images produced for the period, and MOPITT indeed observed the plume over Alaska. We have added the sentence'. Images from the MOPITT (Measurement Of Pollution In The Troposphere) instrument (not shown) studied within this period did show forest fire emissions (Edwards et al., 2003) over Alaska.' after the first sentence under the section 3.1 Smoke over Alaska. Images from the MOPITT (Measurement Of Pollution In The Troposphere) instrument (not shown) studied within this period did show forest fire emissions (Edwards et al., 2003) over Alaska. Attempts were made to get fire product from any instrument in operation, such as AVHRR but all attempts failed until finally we obtain MODIS fire data.

b. We thank the reviewer for his positive comments.

c. We appreciate your suggestion that we look at the meteorological set-up first, then the FLEXPART simulation, and finally the observations. However, the drive of this study was the numerous satellite observations that were circulated within the scientific community about this event and the opportunity of using these data for model comparison. Therefore, model results and satellite images are presented together. However, we have tried to improve the flow of the text by moving the detailed discussion about the meteorological conditions to the beginning of the section and somewhat restructuring the rest.

ACPD

4, S837–S844, 2004

d. The response about the steps at 3c above is also applicable to this section. Other instruments might have captured this event but we used the MODIS image because it was available to us and also captured the structure relevant for this study over this region. Whiles in the other regions we had images from Sea WiFS instrument. We did not show TOMS aerosol index over Alaska on 21 May because there was no data available between 15 and 23 May 2003 due to the shut down of the instrument. On 27 May we used the Sea WiFS image because it agrees quite well with the model simulation hence the decision to use it. In general we used the instrument that show an image over a region at the right time and which is available to us.

e. The response about the steps at 3c above is also applicable to this section. We have separated the lidar results and sub-title it as 'smoke over Germany (3.4).' In view of that, the sub-title '3.3 smoke over Europe' has been changed to 'smoke over Scandinavia'. To include some previous lidar research measurements, we have put the sentence 'Lidar measurements from the German aerosol lidar network has been used to observe Canadian forest fire emissions over Germany (Forster et al., 2001)' before the statement; 'Fig. 6a shows a strong lidar backscatter ratio of aerosol at 1064 nm without any separation between the boundary layer and the free tropospheric aerosol layers.' Although simulation using ECMWF and GFS are very similar, we do agree there are differences however, as we stated under conclusion it is not clear from this study what the reason might have been. We have also included FLEXPART backward simulation that shows the origin of the smoke plumes seen over Leipzig.

General Comments

As this paper is a qualitative study, we included the GFS analysis to show which data sets could best simulate this event, but in general the two data sets showed the fine-scale structure seen in the satellite image although there are some differences but from this study is not clear which one best simulates the event. As we have no detailed information on the observed smoke such as the height over the other regions, it is not possible to run FLEXPART backward simulation.

ACPD

4, S837–S844, 2004

Minor comments and technical corrections

1. References have been now listed according to dates.

2. The statement implies, apart from the tropics, boreal forest fires are another strong source of emissions.

3. Introduction 4th paragraph; The 2nd sentence now begin with the word 'In'

4. You are right, the sentence should read 'At the end of 2003 fire season, more than 19 x 10^6...', so we have add the word 'than'.

5. Yes the last word should be plural; hence we have change from 'cloud' to 'clouds.'

6. The sentence now reads; 'western Germany on 27 May (Fig. 5c).

7. We agree that including the timeline to title at each region will make it flow better, therefore the timeline at each region has been added.

8. The print out from the print version we have looks okay.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1449, 2004.