Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2015-994-AC3, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Wildfires in Northern Eurasia affect the budget of black carbon in the Arctic. A 12-year retrospective synopsis (2002–2013)." *by* N. Evangeliou et al.

N. Evangeliou et al.

nikolaos.evangeliou@nilu.no

Received and published: 5 May 2016

Anonymous Referee #2

Received and published: 7 April 2016

The authors present a set of multi year simulations of northern hemisphere BC transport and deposition, based on two emission inventories. They focus on the Arctic, and estimate the contribution from Northern Eurasian Wildfires. The results in the paper are relevant to the ongoing discussion on the atmospheric, environmental and climate effects of black carbon. Their methods are sound and relatively standard, and the presentation acceptable - although I have some comments and suggestions. I

Printer-friendly version

Discussion paper

recommend that the paper be published in ACP after some revisions, mainly regarding the clarity of some of the arguments presented.

Major comments

- While the authors present results from one model (LMDZ-OR-INCA), they also compare their results to other studies. To fully make this comparison, I recommend adding a brief discussion on how their model has performed relative to others in recent multi-model comparisons, notably AeroCom Phase II (Myhre et al. 2013, ACP).

Response: Corrected!!! A brief discussion has been added to Methodology about how LMDZORINCA performed in this intercomparison exercise.

- Throughout, I also miss some simple sensitivity studies for the key or updated parameters of model, and some discussion of how robust the authors expect that their results are. E.g. on page 6 they state that " A comparison made with inert tracers indicated an enhanced vertical transport as the horizontal resolution of the model was increased from 144×142 grid-points to 280×192." Does this have any bearing on the results here? If so, what is the impact of this enhancement on the burdens and vertical profiles presented later? And on page 14 they state that " the annual mean lifetime of anthropogenic BC particles from BB was longer (6.8 d) than for BC from combustion (5.6 d)". However the uncertainties given above indicate that these values are consistent within errors. (What are the errors? One sigma? I cannot find this specified.) Figure 3 further indicates that there's little significant difference between the two estimates.

Response: Corrected!! We have added a paragraph discussing about the robustness of our model in end of Page – beginning of Page 6. The comparison with inert gases and the comparison of the vertical transport between the 2 model resolutions has a significant impact in out results, because it implies that it may be an underestimation of surface concentrations due to the enhanced vertical transport. As regards to the second part of the comment, we first need to clarify to the reviewer that 5.6 ±0.2

d and 6.8 ±1.0 d are annual global average lifetimes for anthropogenic and bb BC plus/minus the standard deviation of the dataset (N=365). It is now specified in the caption of Figure 3.

- On the lifetime calculation and Figure 3: Since the authors use a steady state definition (which I agree is reasonable), and the lifetime is on the order of a week, is it really meaningful to show daily lifetime values?

Response: The motivation to show daily lifetimes was just to examine the variation of lifetime with respect to the different origin of BC (anthropogenic, biomass burning). We have made a better Figure 3 now showing only timeseries of minimum, maximum and average lifetimes, which we think that it would be more consistent to how other researchers present such kind of results (see for instance Fig. 3 in Croft et al., 2014).

- I would recommend a thorough reworking of the figures. While they are well thought out and have the right content, they are often very hard to interpret. For the map plots, e.g. Figure 1, the resolution is low (perhaps just a feature of ACP processing), and the continent lines virtually invisible. A polar projection like Figure 2 is more readable, even if it skews the outer edge. In Figure 3 the whiskers come out OK, but the box mentioned in the caption is invisible. Same for Figure 7. Figures 5 and 10 have grey boxes overlaying the figure content (again possibly a processing issue, but please check).

Response: Corrected! As regards to the poor resolution, it is subject to the initial submission process required by the ACPD journal. Of course, the same figures exist in a higher resolution, as well, and they will be submitted when required. We agree with the reviewer about Figures 3 and 7 and we have recreated them. We do not show Box & Whisker plots anymore, but only average lines shaded from minimum to maximum, which is more or less the same.

Minor comments

- The abstract is quite lengthy. I would recommend shortening it, as brief abstracts

Printer-friendly version

Discussion paper

greatly increase the readability of papers. The main results are anyway repeated in the Conclusions.

Response: Corrected. We agree with the reviewer and we have corrected according to his suggestion!! We have shortened the abstract to 1 page removing all the unnecessary information.

- Page 16: Two references to Figure 8 should be Figure 7.

Response: Corrected!!

- Figure 6: Is there any interannual variability in the shape of these vertical profiles? This is an interesting observable quantity for estimating transport. Your years cover the HIPPO flight times (Schwarz et al. 2013, GRL). Have you considered a comparison here? Even without this, many studies use HIPPO, and it would be interesting to know how stable the conditions seen by those flights were likely to have been.

Response: We have not compared our profiles with HIPPO, mainly because the HIPPO campaigns were located in a different location than the one of our interest (Eurasia). A fast comparison is shown in Fig. 1 (plot is from HIPPO, Schwarz et al., 2010) and Fig.2 (our model)

The MMR values of the x-axis were far away the HIPPO ones.

In the plot referring our model, it is obvious what was mentioned in a previous comment that the reviewer made about what the impact of the statement “A comparison made with inert tracers indicated an enhanced vertical transport as the horizontal resolution of the model was increased from 144×142 grid-points to 280×192” is in our results. You may see now that comparing to HIPPO, a larger mass has been transported vertically to higher altitudes.

- Figure 8: How was the vertical level averaging done? The caption states "averaging all the vertical layers". The standard definition of atmospheric burden is the sum of the abundances in each layer (i.e. concentration times the height of the layer). If this is not

what is shown here, another word than burden should be used.

Response: Corrected. We have recreated the Figure. We now summed the vertical levels in order to be consistent with the definition of "burden" as pointed by the reviewer. Note that the scale has now changed to include the much higher values resulting from the summing of the layers.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2015-994/acp-2015-994-AC3-supplement.pdf

---

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-994, 2016.

Printer-friendly version

Discussion paper

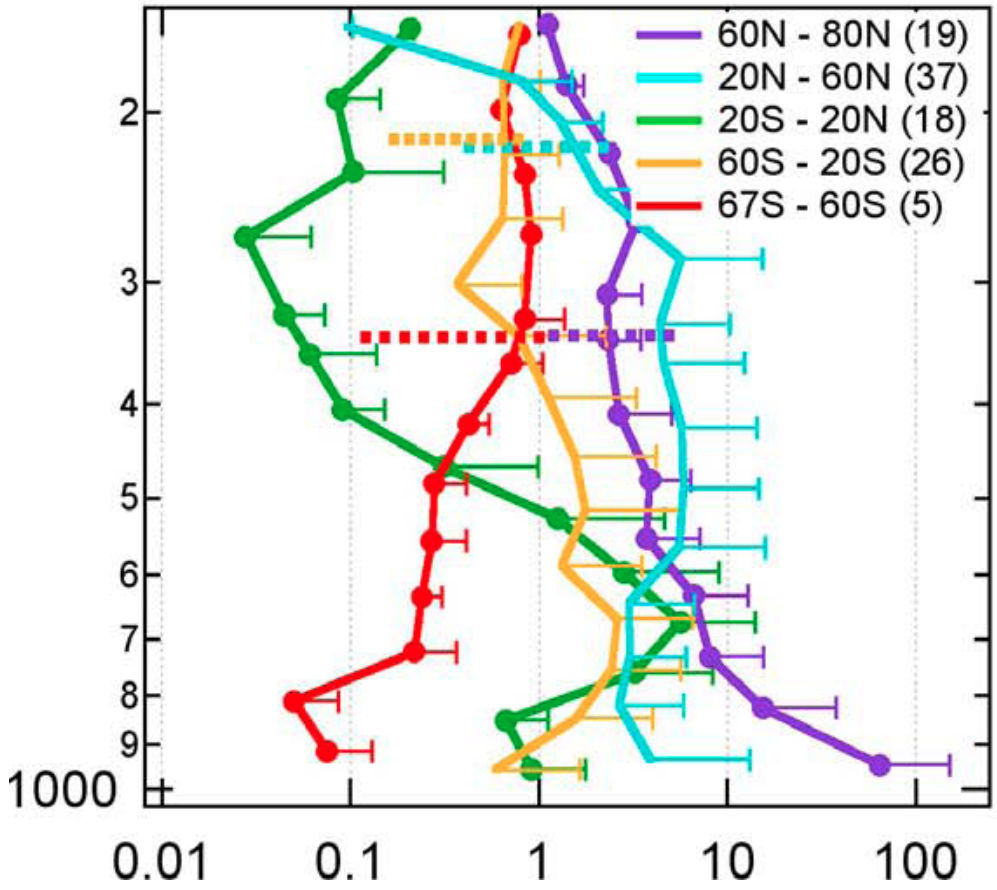

Fig. 1.

Printer-friendly version

Discussion paper

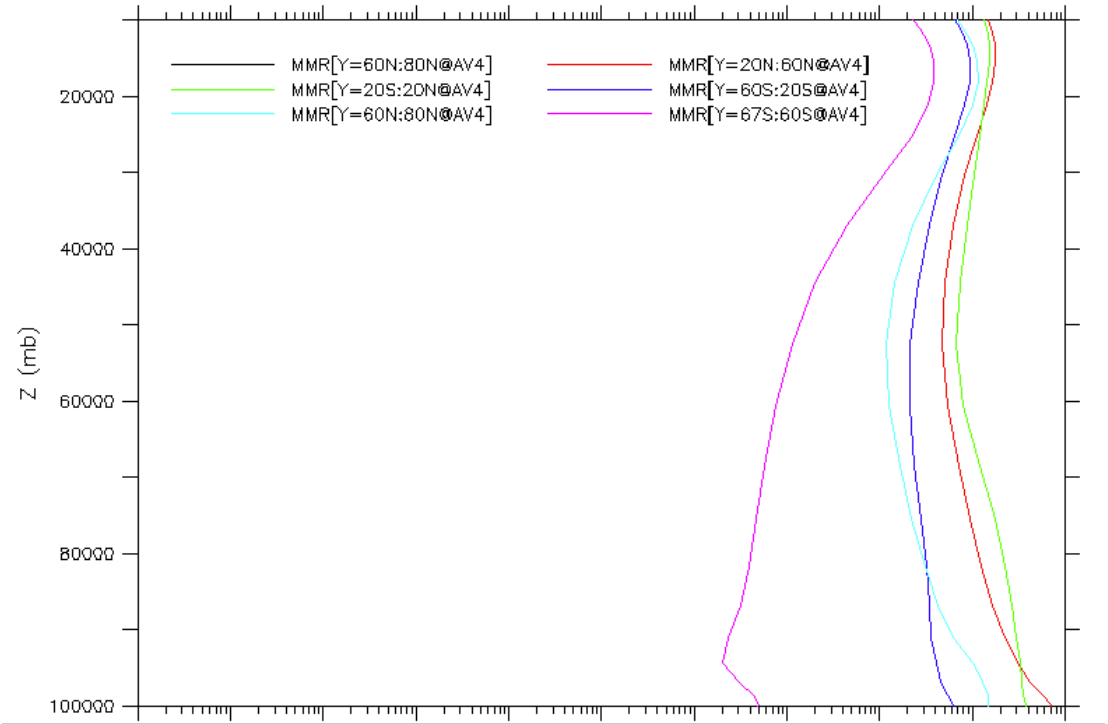
MMR[Y=60N:80N@AV4]
MMR[Y=20S:20N@AV4]
MMR[Y=60N:80N@AV4]
MMR[Y=20N:60N@AV4]
MMR[Y=60S:20S@AV4]
MMR[Y=67S:60S@AV4]
Z (mb)
20000
40000
60000
80000
100000

Fig. 2.