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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.

Anonymous Referee #2

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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 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).

- 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.

- 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?

- 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).

Minor comments

- The abstract is quite lengthy. I would recommend shortening it, as brief abstracts greatly increase the readability of papers. The main results are anyway repeated in the Conclusions.

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

- 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.

- 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.

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