

## ***Interactive comment on “Tagged tracer simulations of black carbon in the Arctic: Transport, source contributions, and budget” by Kohei Ikeda et al.***

### **Anonymous Referee #1**

Received and published: 20 April 2017

This paper titled: “Tagged tracer simulations of black carbon in the Arctic: Transport, source contributions, and budget” studies the long range transport of BC from various source regions to the Arctic. The main concept of the paper, although not new, is important. However there are many issues with current version of the paper. In general several sections need be re-written. For example the authors are “barely” describing the sensitivity simulations and also there are descriptions of simulations for which no results has been shown (e.g. preliminary simulations). In addition, the literature review needs improvement with inclusion of key studies. The interpretation of comparisons between observations and model simulations are also problematic. Therefore this paper can only be recommended for publication after all of the major comments below

[Printer-friendly version](#)

[Discussion paper](#)



are addressed thoroughly and the write up is improved extensively.

### Major Comments:

In abstract authors seem to claim that the new scheme has improved comparison with observations, however, this does not seem to be true when looking at figure 2 for Barrow and Zeppelin where the so called standard scheme suggests a better comparison with observation. Also why there is no blue line in Fig. 3 similar to Fig. 2?, how does blue line compare here? Why Fig. 4 does not show comparison with standard and new scheme?

Page 2, lines 30-33: I agree that Eckhardt et al. (2015) found that BC is still underestimated in several models. The potential reasons for this were investigated by Mahmood et al. (2016) who used data from same model used by Eckhardt et al., 2015 and found that one major reason is convective wet deposition process outside the Arctic which influences transport of BC into Arctic. This is a major study for Arctic BC processes and should be included in the introduction. Mahmood, R., K. von Salzen, M. Flanner, M. Sand, J. Langner, H. Wang, and L. Huang (2016), Seasonality of global and Arctic black carbon processes in the Arctic Monitoring and Assessment Programme models, J. Geophys. Res. Atmos., 121, doi:10.1002/2016JD024849

Page 4: Emission inventory: I wondering why the authors are using an older version of GFED fire emission data when a new version (GFED4 and GFED4s) are available?

Page 4, lines 10-25: It is not quite clear which anthropogenic emission inventory the authors are using. At beginning they say that “In this study, we adopted the BC emissions of HTAPv2.2”, however later on they mention that they used an inventory by Huang et al., 2015. In addition, the authors also claim, without any proof, that the inventory of Huang et al., 2015 improved comparison with observation. I do not see any such results of their so called “preliminary simulations”.

Page 4, lines 15-20: The doubling of BC emissions in Asia and Russia, How realistic

that would be? The authors argue that it is necessary to match modeled BC with observations in Arctic, but could this not be due to other modelling errors/discrepancy? How certain the authors are about this? A recent study showed that the differences in modeled aerosol processes in different models can contribute to overall concentrations in the Arctic (Mahmood, R., K. von Salzen, M. Flanner, M. Sand, J. Langner, H. Wang, and L. Huang (2016), Seasonality of global and Arctic black carbon processes in the Arctic Monitoring and Assessment Programme models, *J. Geophys. Res. Atmos.*, 121, doi:10.1002/2016JD024849.)

Page 4 lines 20-22: “which was about 20%”, Is 20% correct? It seems to be ~22.2% from the numbers given in that line? Even after rounding it would be 22%? Please also check the subsequent numbers.

Page 5 paragraph 25: the authors say “We separated the major source regions of anthropogenic BC such as Europe, Russia, Asia and North America into different tracers”, which different tracers?

Page 5, lines 25-30: “Asia was separated into three regions (i.e., East Asia, South-east Asia and India)”, According to Fig. 1, the region named “India” contains several other countries, e.g. Sri Lanka, Pakistan, Nepal, Bangladesh, Myanmar, so this region should be named “South Asia : SA”.

Page 6, lines 25-30: The authors claim that the correlations between observed and model BC has improved with new scheme. However, at least from Figure 2, it does not seem to appear that the new curve changed in its shape compared to standard scheme, only the magnitude seems to have changed then how the correlation is improved?

Page 6, lines 30-35: the authors say: “ This is mainly because the new scheme yielded an increase in BC concentrations except in summer with maximum effects in winter at the all four Arctic sites.”. Figure 2 clearly shows that BC is also increased in summer, though relatively small. Thus I think this sentence is not correct. Similarly from Figure

[Printer-friendly version](#)[Discussion paper](#)

2, I do not think that the new scheme improved BC values at Barrow as the authors seem to claim. There is clearly way more over-estimations for 9 months in new scheme than the standard scheme. How can the authors claim it an improvement when it is overestimating more than the standard scheme for most months of the year including, November, December, January, February?

Page 7 lines 10-15: Is there any evidence for overestimation of BC emission from Russia?

Page 7, lines 15-30: Why there is no discussion of standard scheme in Fig. 3? If the authors want to claim that the new scheme is better than the standard scheme then all model and observations comparisons should include results from both schemes. Same comments for next paragraphs about Figure 4.

Page 9, lines 10-15: The authors write “The stable condition by cold temperatures near the surface suppresses the upward transport of BC over Russia especially in winter”, I agree that stable conditions would suppress vertical transport of BC, but it would also depend on source. For example, if source is gas flaring or forest fires then emissions could reach middle troposphere?

Page 11, lines 17-18: “The relative importance to the BC concentrations on an annual basis will be discussed later (Table 2)” This sentence does not make much sense and therefore need be rewritten.

Page 12, lines 25-27, How is the BC lifetime defined here? More importantly how this discussion is related to the current study which is primarily about regional BC processes. It would more relevant if the lifetime of BC in the Arctic is given here since this study is focused on Arctic (for a multi-model comparison of lifetimes see Mahmood et al., 2016)

Page 13 lines 32-33: the authors write “We also quantitatively estimated the relative contributions to the total deposition of BC to the Arctic region (Table 2)”. relative con-

[Printer-friendly version](#)[Discussion paper](#)

tributions of what?

Page 14: Conclusions: This section has conclusions which I would find hard to agree with. For example, the authors seem to claim that they have identified important pathways for BC transport to the Arctic. Stohl (2006) had discussed the transport pathways to the Arctic. I am not satisfied that the authors provide adequate discussions of transport pathways and how they would differ with those discussed by Stohl 2006. Again authors seem to claim that new scheme improved bc simulation in Arctic which is not obvious for at least two of the four sites for which observation data they used.

Also there is no discussion of uncertainty about BC simulation results. Using just one model simulation can have problematic results. It is advisable that the author either use nudging technology or ensemble members or both to minimize the influence natural variability or at least provide some uncertainty range.

Minor comments:

Several figures can be improved. For example for Figure 4, 5 and 6 only color bar may be used instead of repeating same color bar for individual plots.

The font size of numbers of color bars is too small.

---

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-237, 2017.

Printer-friendly version

Discussion paper

