Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Factors Controlling Black Carbon Distribution in the Arctic" *by* Ling Qi et al.

Anonymous Referee #2

Received and published: 17 October 2016

In this study, the authors investigate the importance of three processes (Arctic emissions, dry deposition, and aerosol mass transfer from cloud water in ice clouds) to improve the comparison between black carbon concentrations simulated by the chemistry-transport model GEOS-Chem and Arctic observations. They find that emitting black carbon directly in the Arctic is more efficient than slowing down deposition during transport, although reducing the efficiency of ice-cloud scavenging also helps to improve the model.

The paper is generally well-written, although there are instances of plagiarism that must be removed, and the discussion and conclusion sections are not adequate and need to be rewritten. Figures are good, and Table 3 is a particularly interesting literature review that will be useful to other modellers. Possible implications for radiative forcing mentioned in section 4.2 are interesting, although they remain speculative. Scientifically, the study's weaker points are an unclear motivation for the choice of processes to be

studied, and a very underused discussion on precipitation biases.

I recommend publication after the comments below are addressed. Those comments are aimed at improving the discussion, but I do not expect that further analysis will be required.

1 Main comments

- There are too many instances of plagiarism in the paper, where the authors quote directly from sources without making that fact clear. Page 4, lines 7–8 have been taken directly from the description of the Barrow site at www.esrl.noaa.gov/gmd/obop/brw. Page 5, lines 19–20 are taken from Elvidge et al. (2009). The sentence that follows seems to have been taken from a World Bank document. The sentence after that (lines 21-23) is taken from Stohl et al. (2013), complete with hyphenation! It is important that authors realise that citing a paper does not mean taking whole statements from it: one must rephrase and extract the information that is relevant to the present study. I have not looked further for instances of potential plagiarism, but the authors need to remove them when revising their paper.

- The study often reads like a model tuning exercise: the authors have acted on three processes but the reader is left piecing together why those three processes have been targeted in priority. The information is already there to some extent in sections 3.2, 3.3, and 3.4, but would benefit from being gathered in a motivation paragraph in the introduction. Such a paragraph could also discuss why the authors believe that aspects like emission injection heights (page 9 from line 35), size distribution, or absorption properties have not been varied to improve simulated Arctic concentrations.

  Then, the authors seem to stop improving their model once its simulation is within

a factor 2 of the observations (page 9, line 3). It feels arbitrary, but I acknowledge that such practice is common in global modelling. Still, a short discussion would be useful in section 5. Schutgens et al. (2016) could be a relevant reference there.

- Section 4.4 is really interesting. Its key message is that difficulties in modelling BC concentrations in the Arctic may be really dominated by non-aerosol aspects of the model. It is a sobering conclusion: why put so much effort in having better aerosol emissions and physical processes when errors in precipitation can wipe out improvements on the aerosol side? Considering its interest, it is surprising that the findings of section 4.4 are not mentioned in the abstract. The section needs to be much better integrated in the paper.

- The discussion section is not adequate. First, the two paragraphs on page 15 lines 9–20 and 28–35 clearly do not belong there because they hardly mention the present study. Instead, we expect to see in that section the answers to questions like: What have we learned that is new? Are results consistent with previous studies? If not, why should we have more confidence in the authors' results than previous results?

  Similarly, section 6 is not an adequate conclusion. It gives too many quantitative details when it really should focus on the key messages and list the limitations of the study.

2   Other comments

- Page 1, lines 34–35: That sentence reads like the nuclear war described by Warren and Wiscombe (1985) actually took place. Fortunately, that was only a model study and the sentence should be rewritten to clarify that point.

- Page 2, lines 5 and 6: Are the authors comparing Arctic-averaged BC radiative forcing to globally-averaged ozone radiative forcing? If so, that comparison would be misleading and should be removed.

- Page 6, lines 10–11: If I understand well, the model already calculates dry deposition velocities using an analogy with resistances in series. The authors only updated the values used to represent the surface. Is that correct? If so, that statement should be clarified.

- Page 8, lines 3–6: That procedure is unclear. Can it be clarified?

- Page 13, line 27: How is precipitation halved or doubled in the model? By scaling precipitation rates? Does that method cause an imbalance in the simulated water budget?

- Page 26, Table 1: How is "data availability" calculated?

3   Technical comments

- Throughout the paper: Ny_Ålesund should be written Ny-Ålesund.

- Page 4, line 5: typo "It is received"

- Page 9, line 9: rephrase to "significant reduction".

- Page 11, line 8: rephrase to "thereby reducing"

- Page 14, line 3: less BC particles → fewer BC particles.

4 References

Schutgens, N. A. J., Gryspeerdt, E., Weigum, N., Tsyro, S., Goto, D., Schulz, M., and Stier, P.: Will a perfect model agree with perfect observations? The impact of spatial sampling, *Atmos. Chem. Phys.*, 16, 6335-6353, doi:10.5194/acp-16-6335-2016, 2016.

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