Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-455-RC2, 2018

Atmospheric Chemistry and Physics
Discussions
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Interactive comment on "Effects of mixing state on optical and radiative properties of black carbon in the European Arctic" *by* Marco Zanatta et al.

Anonymous Referee #2

Received and published: 18 July 2018

This paper uses in situ observations of black carbon from a surface site in the arctic to explore the mixing state and optical properties and then carry this data forward into a radiative transfer model to explore sensitivies. Given that the arctic is a very sensitive region in this regard and not very well studied, this is an important piece of work and well within ACP's remit. While the results aren't particularly dramatic, given the current uncertainties, it is vitally important that these treatments are given an observational basis. The paper is generally very well written in terms of language. I have only a few reservations, but these are mainly of a general nature and will not likely affect the paper's conclusions, therefore I recommend publication after minor revisions.

Comments

When comparing with other arctic SP2 datasets, DOI: 10.5194/acp-15-11537-2015 will

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Discussion paper

likely be of relevance, as this made airborne observations in a similar region.

Generally, there is a tendency in this paper to not introduce certain variables properly. One example would be the various definitions of RF, but there are others. The authors should take care to make sure that each variable or parameter is properly defined when it is first used, in particular the use of subscripts.

The authors use Mie to model the optical properties of the aerosol, but this is based on Mie-based data of the SP2 LEO inversion, which may suggest circular reasoning. However, I would consider this legitimate; Liu et al. (2017) present an experimental case that this applies for the 'thickly coated' particles observed by the SP2. I would suggest that the authors refer to this work to justify their method.

The authors refer to 'radiative forcing', but this is inconsistent with the IPCC definition of the term, which is the anthropogenic perturbation compared to a preindustrial base case. I would recommend that the authors refer to this differently, for instance 'radiative contribution'.

The authors used an Aethalometer to derive the absorption coefficient, but this is based on an assumed C value during the correction process. The authors should comment on the presumed accuracy of this. Furthermore, they use the Virkkula method to correct for the loading artefact, which is useful when there are no collocated measurements of BC or absorption, however given that there is collocated SP2 data, the (more accurate) Weingartner method (or the improved method in DOI: 10.5194/amt-3-457-2010) may be possible. The authors should justify the use of the Virkkula method better.

The authors refer to an SP2 method to determine 'attached' particles but do not adequately describe it. They should give more detail here. Also, anecdotal evidence suggests that this indicator is not consistent between instruments, so it would be useful to demonstrate that the unit used here is capable of detecting these.

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2018.