Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-864-RC2, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of Black Carbon Mitigation on Arctic Climate" *by* Thomas Kühn et al.

Anonymous Referee #2

Received and published: 6 December 2019

General comment

The here presented manuscript describes the potential impact of BC-emission reduction on the forcing exerted by aerosol in the Arctic. The topic of the paper is of great interest and represents a nice bridging example between the science and policy communities. Overall the manuscript is well written and clear, however, some editing is still needed. I thus recommend the authors to consider the comments below for a second review round.

Major comments

The manuscript tends to be over-inclusive. Some sections are definitely out of context here, especially Section 3.6 is completely out of topic, as also indicated by the absence

of figures in the main text. I suggest removing the above-mentioned section because it does not comply with climatic effects. Something similar might be said for Sections 3.1 and 3.4.1. Global emissions and resulting forcing are definitely of interest, but it is hard to understand what is the goal of such two chapters.

My major concern is, however, the superficial description of the aerosol-physics module (Section 2.2). As a consequence, it becomes almost impossible to judge the reliability of simulations shown in Figure 3, 7 and 9. This is unfortunately amplified by a consistent lack of references along the entire manuscript. Each statement on aerosol-cloud properties or model performances must be explained with citations or results (see in specific comments below). Interestingly, the conclusions are full of references, which is quite unusual.

Specific comments

P2L6-7: please provide a reference.

P2L11-14: these two statements are redundant.

P2L15: sea ice extent.

P2L16-29: in this part of the introduction many protocols, documents and meetings are cited. However, any reference cannot be found. As a note, the whole section suffers from a general lack of peer-reviewed references.

P4L14: replace "sections" with "bins" or "modes". Apply to all manuscript.

P4L13-P5L9: I noticed here a tendency to oversimplification of processes. As examples: in which size mode sits BC or OC? Is OC considered to be completely soluble? The text can be kept simple, but more references should be provided, I assume Kokkola was not the only one using the model.

P5L2: what exactly it is meant with "all aerosol particles are assumed to have the same chemical composition"?

P5L16: would this affect the DRE estimations for the 2030 and 2050 estimations? Could the author provide an estimate for this? I am particularly thinking of this publication doi:10.5194/acp-14-537-2014.

P6L20-23: no black line for SU in figure 2; I thought that CO2 emission was assumed to be the same for all simulations, why CO2 should then decrease? In what way a reduction of SU is unfavorable?

P6L27: I would call this increase as "negligible". Following a previous statement at L21"..OC and BC emissions are mostly unchanged. . ."

P7L1-2: Why CLE-SU is compared with OC-SLCF? What do you want to demonstrate?

P7L4-7: provide references and try to articulate a fluent story here.

P7L7-14: As also stated in P11L23, meteorology is very important and large-scale circulation and precipitation control the transport of pollutants to the Arctic. How do the vertical structure of the atmosphere vary in your simulations? Is there any constant feature supporting your upper-lower atmosphere distinction (potential temperature, RH, temperature, etc. . .)?

P8F3: 1) How do your profiles in 2010 (F3) compare with observations? Are these simulations reasonable? Please elaborate in the text. If such profiles are off, all the following results in the manuscript will be biased. 2) Make use of literature to justify your results, there are plenty of studies on transport to the Arctic. 3) Please change the units from Kg/m3 to some more traditional units ug/m3.

P10F4: How are the burden calculated in F4? From F3 I would guess that BC burden would be higher in LA than in UP for at least 2010. But it appears to be the contrary. There is no consistency between units used in the text and in F4.

P12L21: "N100 are a common proxy for CCN", never heard of it. Please provide a robust explanation and references.

P12L23-26: along with the paper, there is a tendency to rapidly come to conclusions. I do understand that SU correlates well with N100, but here you do not bring any evidence of NPF dominating the SU concentrations. . .correlation does not always mean causation. Please explain this, potentially in 3.2.3.

P13F5: N100 and CDNC both in 1/cm-3. Are the 2010 profiles similar to reality (see comment on F3)

P14L7 "A decrease in CDNC means that the cloud droplets are on average larger". This is true if the liquid water content is constant, demonstrate the diameter change. Lower CDNC might mean

P18L26: Could you explain the difference between dry deposition and sedimentation?

P20L19-24: Similar to aerosol vertical profiles. . .what are your aerosol concentrations in snow for the 2010 year, in the range of Doherty?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-864, 2019.