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

Anonymous Referee #3

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This is an interesting and well-written manuscript presenting a model-based study of the potential impact of reductions in black carbon emissions on the Arctic climate, and also on pollution-related mortality. Of particular note is the focus on the impact of limited reductions by only those countries with a direct interest in the Arctic region, and which therefore have a strong likelihood of actually implementing such changes. I'm pleased to recommend the manuscript for publication in ACP, provided the minor issues noted below can be addressed:

1 General comments

1. The introduction section seems heavily slanted towards the policy background to the study – while this is important, this section should include a little more of the scientific background on the climate and health impacts of aerosols and BC in particular, and how this study fits into the previous literature regarding global and regional-scale emission reductions.

2. It's unclear how anthropogenic emissions that aren't directly tied to a country are treated in the scenarios – e.g. shipping emissions that occur over the ocean. Are reductions in these included in the mitigation scenarios , and if so how are they included/excluded in the regional reduction scenarios like AC_ACT?

3. Throughout, the terms "lower atmosphere" (LA) and "upper atmosphere" (UA) are used to refer to the regions below and above 450 hPa. This is confusing, as the term "upper atmosphere" is commonly used to refer to much higher regions above the stratosphere and mesosphere (which are conventionally termed the "middle atmosphere"). I would recommend changing these to "lower troposphere" (LT) and either "upper troposphere" (UT, if contributions from the stratosphere and above are negligible) or "rest of atmosphere" (RA/RoA, otherwise).

2 Specific comments

p.5, lines 24–25 The MACC reanalysis only covers a period of 10 years, but these simulations are run for 30 years. Which year(s) of the reanalysis dataset are used for which years of simulation, or is a derived climatology used rather than the reanalysis directly? For future work, the authors should be aware that this dataset is now superseded by the CAMS reanalysis (Inness et al., 2019; 10.5194/acp-19-3515-2019).

p.6, lines 8–11 If the meteorology is fixed, then any changes in the source–receptor relationships arising from the rapid adjustments in the atmosphere will not be represented. The authors should consider briefly how significant the impact of this is likely to be.

p.12, line 24 Why must this be mostly NPF rather than the condensation growth of smaller particles to cross the size threshold?

p.14, lines 1–2 As well as being "fairly small" this change is also not statistically significant given the stated uncertainty (especially in 2050; it's marginal in 2030).

p.14, line 16 Is this shift in vertical profile statistically significant or not?

p.15, line 5 Since the error bars suggest the change in ERF is not statistically significant (unlike that in RF), it's stretching the data to say the changes "lead to warming" here.

p.23, line 18 "ECHAM" does not include prognostic aerosol; "ECHAM–HAM" or "ECHAM–HAMMOZ" should be used to refer to the aerosol–climate model.

p.24, lines 13–17 The model data used in the paper should be available to the reader (either from an archive, or at least by contacting the authors) without having to re-run the entire set of simulations.

Figures 3, 5 Can some indication of the uncertainty on these vertical profile differences be included, as has been done for some of the other types of plot? Otherwise it's unclear whether changes are statistically significant or not.

Figures 4, 6, 7, 8, 9 The manner in which the error bars represent uncertainty should be briefly stated in the caption.

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