

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2022-288

William Collins (Referee)

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Referee comment on "Mapping the dependence of black carbon radiative forcing on emission region and season" by Petri Räisänen et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-288-RC2>, 2022

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This is a useful study of the effect of emission location on BC forcing efficiency. As the authors themselves note, this is a single model study with lots of uncertainty. However they explain the physical processes behind their results which are likely to be more generally applicable even if the absolute numbers are not. This will be useful to publish after addressing the points below.

I understand the reluctance for detailed indirRF calculations since they are not likely to be additive, but it might still be useful to see the regional variation. A version of fig 4 for indirRF could be added to the supplement. Why is the indirRF positive? Does mixing with BC reduce the nucleating ability of SO<sub>4</sub>, or does it reduce the SO<sub>4</sub> lifetime? How is indirRF calculated? Is it a double call as in Ghan 2013?

Even though they can't be addressed in this study, there could be a bit more mention of meteorological adjustments to BC, for instance the increased stabilisation of the atmospheric profile, and how they might affect the conclusions. Stjern et al. is cited, but not the discussions in that paper, also there are Samset papers. These meteorological adjustments will be included implicitly in the Shindell and Faluvegi coefficients. What they term "efficacy" is really an accounting for adjustments.

Introduction: Could also cite Aamaas et al. 2016 and Bellouin et al. 2016 papers from ECLIPSE.

Line 85: This should mention the magnitude of indirRF here - it seems to be ~25% of dirRF.

Line 130: Suggest to use "meteorological adjustments" or "rapid adjustments" rather than "semi-direct effect" to align with IPCC terminology.

Line 136: Give some explanation of how  $F_{\text{air}}$  is calculated. Presumably this is from a double call to the radiation scheme with zero BC in the advancing step.

Section 2.3: This might comment on how the mixing affects other species. Does mixing with SO<sub>4</sub> affect the lifetime of BC, i.e. do BC emissions have a lower lifetime if they are emitted in a high SO<sub>4</sub> region? Does mixing with BC affect the lifetime of SO<sub>4</sub>, i.e. is there an indirect effect of BC on SO<sub>4</sub> RF and if so is this included in  $F_{\text{air}}$ ?

Figure 1. I suggest splitting into COARSE-REAL which shows the impact of resolution, and then RECONST-REAL which shows the additivity. I'm not sure RECONST-REAL is that useful since it mixes these two effects.

Line 240: Why aren't the reconstructed fields for REAL and COARSE actually identical as opposed to "virtually" identical. Similarly, in fig S1 are the emissions for RECONST and COARSE identical, and if not, why not?

Figure 3: Why doesn't dirRF scale with column burden? I would have expected 3(a) and 3(c) to be much more similar since section 6.3 suggests little non-linearity in dirRF. A plot of dirRF/columnBC would be useful in the supplement.

Line 377: The longitudinal variation seems interesting, and very policy relevant.

Section 6.1: Suggest to also compare with ECLIPSE project, Bellouin et al. 2016

Section 6.2: Suggest to also compare with ECLIPSE project, Aamaas et al. 2016 and 2017.

Lines 389: This paragraph would be clearer if it included discussion of meteorological adjustments and ERFs. The reason dirRF for BC has less effect on climate is not because it has lower "efficacy", it is because there are adjustments that oppose the dirRF so that the ERF is lower than dirRF (e.g. Stjern et al. 2017). Studies of BC efficacy defined in terms of ERF (E.g. Richardson et al. 2019) show an efficacy of around 1.0 when compared to CO<sub>2</sub>.

Figure 7: This figure is presumably very sensitive to the assumed factor of 3 efficacy for

snowRF. What is the uncertainty in this factor of 3? Would the conclusions be qualitatively the same with a lower factor?

Line 518: Suggest to use "meteorological adjustments" or "rapid adjustments" rather than "semi-direct effect" to align with IPCC terminology.