

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-1100

Anonymous Referee #2

Referee comment on "Black carbon aerosol reductions during COVID-19 confinement quantified by aircraft measurements over Europe" by Ovid O. Krüger et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1100-RC2>, 2022

This study evaluates the effect of lockdown on the BC emission using aircraft-measured vertical profiles and modelling adjustment. It is a concise and well-presented study and contains substantial valuable work. Although the conclusion itself is not particularly exciting, the dataset is valuable and would worth publishing after addressing the following points.

My main concern is how the 40% reduction of the overall emissions has been derived. How robust this value is. Why only a single value to adjust on the old inventory to apply for all over the regions in Europe. What is the criterion, has the comparison been performed with the measurement in the boundary layer or free troposphere to derive this conclusion? Would some sensitivity tests about this 40% be required?

Other points:

- 1) One important point I think is from the plot, it seems the surface BC concentration has not been reduced significantly, what is the reason for this?
- 2) It would be useful to indicate the mean boundary layer height during flights, as you were mainly focusing on the pollution reduction in the boundary layer.
- 3) Line 75-80, has biomass burning significantly changed between both years? You mentioned the high-altitude was more influenced by biomass burning, but the later discussions have not mentioned it. It may be useful to simply show the fire points to imply how they have changed.

4) How about the other fractions besides the meteorological and emissions, as the sum is not really close to 100%.

5) Have you considered the diurnal variation of BC mass loading the boundary layer, i.e. the flight time in the day.

6) In Figure 2, I would suggest adding the inventory information in the plot legend, not just mentioning it in the caption. Has "grey and solid line" been shown in both Fig. 2a and d? The concentration normalized by non-emission factors between 2017 and 2020 needs to be more clearly clarified. Have you modelled the 2020 case using exactly the same met data with 2017? Have the flight path been set the same. I would suggest a rather simple and clear plot to show the procedures step by step how the met influence has been neutralized.

7) The fact is that most of the light tracks have not been overlapped, some discussions are required to explain the reasoning to allow for this comparison.

8) It would be useful to discuss the point that reduced BC concentration corresponded with the reduction of BC core size (maybe due to reduced chance of coagulation between BC particles) and its potential implications. This statement can be made by comparing the BC core size with some regions which are significantly influenced by anthropogenic emissions and how this has been related to BC mass concentrations (Liu et al., 2020; Ding et al., 2019). It would be helpful to comment whether precipitations had affected the BC core size.

References

Liu et al.: Black carbon emission and wet scavenging from surface to the top of boundary layer over Beijing region, *JGR - Atmos*, 125(17), 2020.

Ding et al.: Size-related physical properties of black carbon in the lower atmosphere over Beijing and Europe, *ES&T*, 53(19), 11112-11121, 2019.