

Interactive comment on “Chemical composition, optical properties and radiative forcing efficiency of nascent particulate matter emitted by an aircraft turbofan burning conventional and alternative fuels” by Miriam Elser et al.

Anonymous Referee #1

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The manuscript presents measurements of the organic/elemental carbon fractions and optical properties (scattering, absorption, extinction) of aircraft engine soot sampled at the exhaust plane of the engine while in a ground-based test cell. Four different fuels were examined, including both Jet A-1 and mixtures of Jet A-1 with a HEFA jet biofuel. The manuscript is relatively short and well written. There is a quite a bit of methods information in the supplementary material that should be moved to the main text to improve readability. The paper should be publishable after the following comments are addressed:

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1) Emissions data are reported in terms of concentrations (e.g., mg m^{-3} or Mm^{-1}), which are not particularly informative to the reader for interpreting the differences between thrust settings and fuels. The authors should normalize the results using the CO_2 concentration data to report the data in terms of emissions indices (e.g., mg [kg fuel]^{-1} for mass emissions and $\text{m}^2 [\text{kg fuel}]^{-1}$ for optical coefficients). While this makes the data more useful and accessible to the readers, it won't change the intensive parameters, and therefore the conclusions of the paper.

2) It would be worthwhile to split the panels in Figures 2 and 3 into multiple figures that emphasize key relationships in the analysis. For example: 1) a new figure combining Figures 2d and 2e with Figure 3d to emphasize the MAC calculations and the contribution of EC and OC to the MSS BC mass EI. Similarly, a new figure combining Figure 2f and Figure 3f emphasizes the transition at $\sim 50\%$ thrust from highly scattering, organic-carbon-dominated exhaust particles to highly absorbing, elemental-carbon-dominated exhaust particles. I would encourage the authors to think critically about how best to present these figures in order to support their discussion rather than just lump them into large, multi-panel figures with different (and not really comparable) x-axes.

3) The article is rather short as it is currently structured, so much of the additional discussion in the supplementary material could and should be moved to the main text. This is particularly true of the text in Section S1. The tables and figures in Section S1 could remain in the the supplementary material with direct referencing from the main manuscript. Section S1.4, in particular, is rather theoretical, speaks to the measurement data quality, and would be valuable to placed in the more prominent location of the main manuscript.

4) I agree with the other reviewers that the simplified radiative forcing model and calculations presented in Section 3.3 do not really add much to the paper. This is evident from the merely qualitative discussion of this section in the abstract and conclusions. One would expect *a priori* that introducing absorbing aerosols such as aircraft soot above a reflective surface would produce a warming effect and that the magnitude

C2

of this warming effect would decrease over darker surface types. I suggest that this section be removed.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1171>, 2018.