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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Reductions in aircraft particulate emissions due to the use of Fischer–Tropsch fuels" *by* A. J. Beyersdorf et al.

Anonymous Referee #3

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The paper presents results from a major field campaign aimed at understanding the effects of burning alternative fuels in aircraft engines, compared with conventional kerosene. The paper considers several different fuels and also the effects of aging by taking samples at 3 discrete distances from the exhaust plane. The paper is of relevance because of the current demand to find alternative fuels and the need to quantify the potential impacts switching to such fuels may have.

Several of the results from the paper are in agreement with similar results published in the literature, and the addition of the analysis exploring the effects of temperature are interesting and of scientific benefit.

The only major concern I have is a lack of discussion on the line losses. Whilst I

do not believe this will change the discussion or conclusion, I believe it could impact significantly on the absolute numbers and potentially on the relative differences. I would like to see this discussed in the paper (possibly in supplementary material for detail if needs be) with some upper and lower experimental limits applied to the results. Stating that the losses down the sampling lines used to connect the probe to the mobile lab has previously been shown to be negligible is not sufficient as it is often the values, the splitters and variable flow rates feeding the instruments at the end of the lines where the largest losses occur. I detail this more in the specific comments.

Specific comments:

Table 1, I would like to see a little more detail for completeness. For example, which make and model CPC were the authors using? TSI 3776? The EEPS, I would argue it does not measure total number, but an integrated number from ~6-560nm. A DMA does not measure size distributions, but a TSI 3080 SMPS with 3081 DMA does (for example). Which model AMS was it? Aerodyne C-ToF-AMS or HR?

Regarding the inlet, was it heated and how long were the sample lines connecting the probes to the mobile labs? Were they completely identical with the same flows, splitters etc?

Section 3.2 I am concerned that the authors have not measured or estimated the losses down the lines. Furthermore, the changes in EIn and EIBC that are reported between the control and test engine, are these the same for all sizes? Without knowledge of the differences between the lines as a function of size, can the authors accurately state the differences in EI or (for example) the change in diameter throughout the paper? If there is just one extra T junction in one of the sampling lines, this will change the relative transmission efficiency of the lines. I don't believe this changes the content of the paper, but I would like to see some consideration of the potential errors. For example, page 15114, line 7, the authors state the mean size increases from 47nm to 97nm. If this is the average of all fuels (is it?), then some estimate of the experimental error

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is needed to put upper and lower bounds on that very precise figure (not a standard deviation). Line 21 (the VMD), shows there is a difference in size between fuels. How much of the reduction in ELn is due to the fuel and how much is due to a potential size dependent loss down the sample line and indeed the relative difference between lines. All the sections need tightening up on what are quantifiable differences between fuels and how much are within experimental errors caused by, for example, line losses.

Section 3.3. Do the authors think it would be worthwhile comparing the numbers obtained in here with others from similar studies which also show a reduction in soot production from burning alternative fuels?

Are the data in figures 3 and 12 (and elsewhere if necessary) and ratios stated on page 15114 corrected for the relative differences reported between the control and test engine?

Do the authors see any effect on the size distribution as a function of dilution factor for a given fuel and condition? Have they ruled this out as a contributing factor?

Page 15116, line 12. How can the authors be sure that between 145m and 30m the maximum nucleation rate has been observed and that after 145m, a reduction in EIn through coagulation will occur rather than an increase in EIn because of continuing nucleation? Their data shows the further away from the exit plane you go, the more(small) particles you measure.

Page 15116, line 14. I do not entirely agree with the authors conclusions based on figure 8. At power settings > 50%, their statement is true and there is a clear temperature dependence. However, below 50% and certainly at the lowest settings, the data shows little or no temperature dependence, with one run at 0C having an EI the same as 20C. The warmest days always produce the lowest number, but conversely, the coldest do not always produce the most. Can they explain this?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 15105, 2013.