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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.

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We thank the reviewer for the helpful and detailed comments. Comments are summarized and responses are in italics below. We apologize for any missed opportunities to improve the manuscript and welcome any feedback.

1) 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.

Line losses are now discussed in the "Experimental Design" section and a figure added to the supplemental section detailing losses in the right (#3) engine. Transmission efficiencies were found to be on average 60% for both the 1m and 30m probes on the right engine. A small size dependence was seen with efficiencies of 45% for 10nm particles, 50% for 20nm and 70% for 100nm soot. The data has not been corrected for this loss but it has been noted in the section as a possible issue. For comparing the different fuels only data from the right #3 engine was used – thus inlet loss should not affect the differences seen. #2 engine data was only used for studying the effects of temperature on volatile aerosol formation (this is because the #2 engine always burned JP8 and thus a larger data set was available).

2) 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?

Table 1 has been updated to include this information and a note stating that the EEPS number concentration is found by integrating the size distribution. It was the C-ToF-AMS.

3) 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?

The sample lines were not heated. The sample lines were 40 m (left 1m), 24 m (right 1m), 46 m (left 30m) and 38 m (right 30m). However, it should be noted that the right 1m and 30m lines had similar transmission efficiencies. A comparison between the left *and right engines is not used in determining the alternative fuel emissions.*

4) Section 3.2 I am concerned that the authors have not measured or estimated the losses down the lines. Furthermore, the changes in EI_N and EI_{BC} 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 is needed to put upper and lower bounds on that very precise figure (not a standard deviation).

Line losses are now discussed in the "Experimental Design" section. Emissions from the #3 engine alone are used in the analysis of alternative fuel effects. Thus differences between losses in the 2 lines cannot affect the results.

5) Line 21 (the VMD), shows there is a difference in size between fuels. How much of the reduction in EI_N 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.

A small size dependence was seen in the transmission efficiency. However, this correction would only result in a very small shift in the size distribution. Figure #1 (attached but not included in the paper) shows the relative EI_N for FT1 (FT1/JP8 for EI_N). The corrections resulted in changes in reductions of less than 2.5% for all powers and all fuels. Also, Figure #2 (attached but not included in the paper) shows the change in size distribution resulting from the line loss correction. It is hard to see as the VMD

changed by 0.5 nm.

6) 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?

Included

7) 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?

All data used with the exception of figures 8-10 use the #3 (test) engine solely. Figures 8-10 use the #2 (control) engine solely. Text has been added to the paper to clarify this.

8) 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?

During initial testing with JP-8 fuels, the dilution was varied in order to see if there were any effects on the particulate emissions (and therefore emission indices). No effects were seen.

9) 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 EI_N through coagulation will occur rather than an increase in EI_N because of continuing nucleation? Their data shows the further away from the exit plane you go, the more(small) particles you measure.

This statement was speculation and has been removed.

10) 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?

The original figure was made using limited data from the right engine. This has been replaced with data from the left engine which was always burning JP8. This allows for more data points which show the trend clearer. Figures 8-10 have been updated. A few data points do not follow the trend. These may be due to transient power changes but have not been removed because it is not certain they are in error.

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

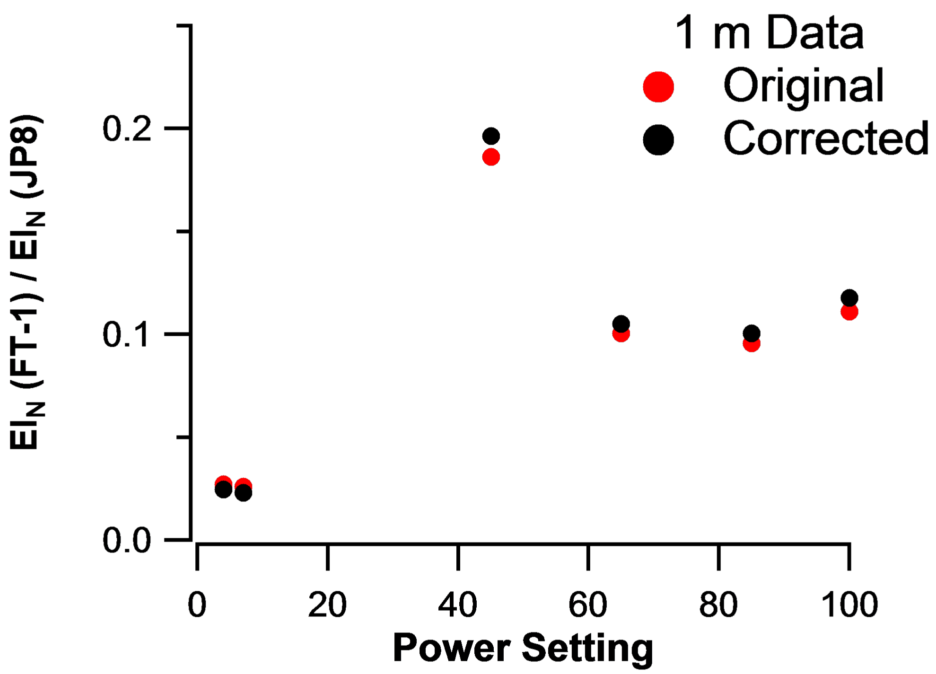
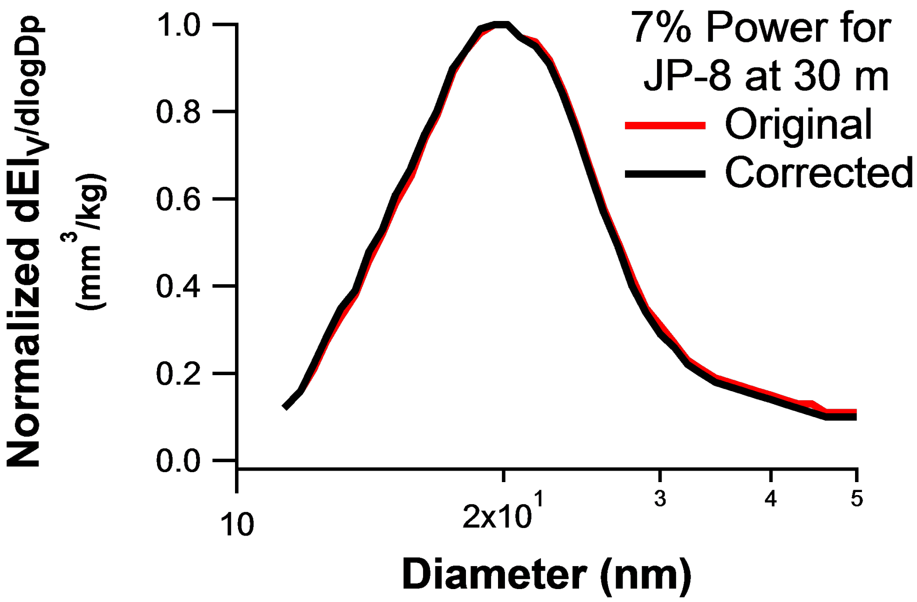


Fig. 1.



Normalized $dEI_{V}/dlogDp$
(mm^{3}/kg)
1.0
0.8
0.6
0.4
0.2
0.0
10
$2x10^{1}$
3
4
5
Diameter (nm)
7% Power for
JP-8 at 30 m
Original
Corrected

Fig. 2.