

Interactive comment on “Airborne and ground-based measurements of aerosol optical depth of freshly emitted anthropogenic plumes in the Athabasca Oil Sands region” by Konstantin Baibakov et al.

Anonymous Referee #1

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This paper reports airborne measurements of aerosol properties in the Athabasca area as part of the Oil Sands Measurement Campaign (OSMC) during 2018. Ground-based Sun photometry data collected in the area are also used. The manuscript is in scope for ACP. It is written clearly. The airborne data provide a good amount of detail on various aerosol plumes measured during the flights, which will be useful to the broader community, since (as the paper notes) they haven't been studied in as much detail as some other aerosol systems. One key point here is that the spatial scales of these plumes are such that they can be missed by the ground-based measurements. I would

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have liked to see a bit more satellite imagery and possibly retrievals to provide a bit more context about spatial variation here. Additionally, the measurements revealed that the plumes had different size and absorption characteristics from one another, i.e. not all the plumes in the area are similar.

I don't have any big issues with the material presented here. I recommend publication following minor revisions. I would be happy to review the revised version, if the Editor would find it useful. My comments are as follows:

1. Throughout the paper, it was difficult to judge the scale of the area and the plumes. I suggest adding a scale in km to Figure 1 so the reader has a sense of size of the overall domain size.
2. I suggest a new figure (either one multiple panel, or one for each of the main flights discussed) be added to show a true-color image around the time of the flights? This would help the reader visually see what was going on. I am not sure if MODIS or VIIRS overflew around the right time (or maybe we will have got lucky and there's Landsat or Sentinel 2), but if not there are the new GOES sensors which are every 10 minutes or so. I looked on NASA Worldview for the days but wasn't sure if I could see the plumes – there were lots of clouds on some days too – so if the authors can provide the relevant imagery so we know what we are looking at, it would be helpful. Here is a link to June 9 imagery, not sure if the plume is visible here (there's a lot of cirrus too), or if the long url will make it through the ACP comment system unmangled: https://worldview.earthdata.nasa.gov/?v=-114.52655240204368,55.351947254962724,-108.18835611557154,58.463275379962724&06-09-T21%3A46%3A08Z&l=Reference_Labels,Reference_Features,Coastlines,VIIRS_NO
3. Satellite retrievals of AOD would also be interesting to show, to reveal whether they resolved the plume structures or not. The MODIS Dark Target 3 km product could be useful here as it is finer than most others. Again, it's hard to know what the spatial scale is from the paper, so it's possible this would be too coarse already? And

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if satellite products don't resolve the plume (either structurally or even as a hotspot) that is another interesting point (analogous to the AERONET spatial representation discussed by the authors for this area).

4. Page 5 line 29: the authors mention O'Neill et al (2016) as a reference for cloud screening based on the SDA (i.e. that the fine mode is unaffected). However Smirnov et al (2018) indicate that in the presence of cirrus (or dust) the SDA fine mode AOD can still be biased: <https://www.sciencedirect.com/science/article/pii/S0022407317306131> Perhaps the authors can comment on this, particularly as there seemed to be some cloud cover in the satellite images on Worldview.

5. Page 6 lines 3-4: it might be covered in the references cited, but could the authors mention here whether the UHSAS size distribution retrieval requires assumptions about refractive index and if so how sensitive it is to that? This could be relevant as it is an optical sensor, and differences between plume refractive index could mask or magnify differences in particle size between plumes.

6. Any other caveats or relevant uncertainty sources associated with the in situ measurements should also be mentioned in Section 2.4.

7. Figure 3: Panel 3 shows UHSAS effective radii around 0.4 microns in Plume A. However, for the same flight (9 June), the lower panel of Figure S4 has all UHSAS data between 0.1 and 0.2 microns. Is this a plotting error in one of the figures, or am I misunderstanding what is shown?

8. Figure 5: do the authors believe the narrow peak in plume A around 0.42 microns is real, or could it be an instrumental/retrieval artefact? Any thoughts on what could cause this sharp feature?

9. Page 9 line 6: I am not sure it is quite right to say that AERONET sites are generally assumed to be representative of a distance 100 km around them. Most satellite retrievals use an averaging circle of order 25 km. Even for a model comparison, if it is at

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1 degree, then the grid boxes are still only 110 km (i.e. a 55 km box if centered around the site) at the Equator and smaller at the poles. I understand the authors' point here but suggest revising the wording to not say "this distance is often taken as 100 km" because I don't believe that is true.

10. Figure 7: the caption notes that the horizontal bars on the AERONET panel here are standard deviation. What are the horizontal bars on the upper panel? This should be stated.

11. Figure 9: I think the caption should read 0.02 here, not 0.002, unless I am misunderstanding.

12. Figure S5: I do not think that the regression is valid here. The fitting, p value, and uncertainties are based on the assumption of independent draws from one population of data. What we have here is data from 4 separate flights. Each flight is likely to have some autocorrelation between observations from that flight, and it's not necessarily true that the difference vs. distance would be consistent between all flights. I suggest redrawing this to color code points from the individual points, and add a zero line but perhaps not a regression. The reader can draw their own conclusions and I'm not sure that the regression is needed for the understanding of the paper: I agree that there seems to be some relationship, but caution against over-interpretation based on a small sample of flights.

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