

Interactive comment on “The unique properties of agricultural aerosols measured at a cattle feeding operation” by N. Hiranuma et al.

Anonymous Referee #2

Received and published: 8 July 2011

I apologize to the authors for the length of time taken to complete this review. This is an interesting and useful contribution that describes characterization of the primary aerosol particle emissions from a cattle feed-lot. It is interesting because it is a relatively unique study and it was found that the coarse particles generated from the feedlot were composed predominantly of carbonaceous material, as opposed to common dust components. In my view, that is the main result: the feedyard sampled produced a large increase in the concentrations of coarse particles that were principally composed of organic material. I have some concerns, expressed below, about some of the inferences related to the importance of fine particles from the feedlot. The paper is generally well written and clear, and the measurements were carefully done. My specific comments follow.

C6066

1. You have not demonstrated a clear impact of the feedlot on the fine particle concentrations, and certainly not a significant impact: there were no measurements of the concentrations of particles smaller than 500 nm at the upwind site, the comparisons in Figure 3 show that the downwind fine particle concentrations were no different than a “semi-urban” site and much less than an “urban” site, and Figure 5 shows no evidence of a fine particle increase. The statement in the conclusions (page 14437, lines 4-8) that primary emissions of fine particles from this type of source need to be included in regional assessments is unsupported.
2. I think that the paper title (“The unique properties. . .”) does not convey the unique aspect of the results. I suggest something like “High concentrations of coarse carbonaceous particles from a cattle feeding operation”.
3. Abstract, page 14418, lines 21-22 – You have demonstrated that “serious errors in estimates” is relevant with respect to public health, but you have not demonstrated that it is relevant in terms of climate. I would guess there is a significant impact on local visibility, but the impact of feedyards farther downwind is not considered in this paper and this statement needs correction.
4. Page 14421, line 1 – “transport” is not really what this paper is about. It is about characterization of primary emissions from a specific source. I suggest something like “. . .provides insight into the size and chemical nature of particles emitted from a feedlot”.
5. Page 14422, lines 21-23 – The other reviewer questioned this, and I feel that the posted response (“The optical diameter of a sphere is identical to its volume equivalent diameter.”) is still unclear. The optical diameter interpreted from a light scattering measurement, assuming that the particles are spherical, depends on the wavelength of the light and the index of refraction of the sampled particle(s). When you refer to ‘optical diameter’, do you mean the diameter interpreted based on the known wavelength and the known index of refraction? I have not seen the Peters et al. reference used here, but

C6067

if their comparisons were done for particles with known indices of refraction then this statement might be reasonable in that context. However, I think that in your application to particles with unknown refractive indices you can not be so confident.

6. Page 14424, line 2 and elsewhere – were these not impactor rather than filter samples?
7. Page 14425, lines 6-7 – Is the “detailed strategy” that which is described in the prior few sentences, or is it more than that?
8. Page 14425 – I think that the first paragraph in section 3.1 belongs in section 2.1.
9. Page 14426, line 9 – This is the first mention of “Feedyard C”. It should come earlier.
10. Page 14426, line 21 – “. . .and by the PAS. . .”
11. Page 14427 – perhaps “e.g.” before the Baum et al. ref.
12. Page 14430 and Figure 5 – the comparison of the downwind and far-field sites is very nice, showing the smaller diameters at the far-field location. The relative reduction at the far-field site is much greater around 21:20 compared with 21:45. Can you explain this? You say that the wind speed was fairly constant, but did it vary enough over that period to explain the variation? Could volatilization of some of the organic components have contributed to the smaller diameters also?
13. Page 14430, line 25-26 – The sentence “However, . . .” is open ended with a rather vague implication before it that enhanced microbial activity in the soil might be the source. Is there a reason to expect the feedlot was a source of increase fine particles? It is not possible to see any difference in fine particle concentrations between the upwind and downwind sites in Figure 5.
14. Page 14431, lines 1-3 – Again, here is a reference to fine particles weighing “heavily in regional assessments”. I don’t see sufficient evidence for this.
15. Page 14431, lines 18-19 – Remove the 150 ug/m3 in brackets as it is defined in

C6068

the sentence immediately before.

16. Page 14431, line 24 – The 45 ug/m3 is from the upper density limit, and it is not observed.
17. Page 14436, lines 5-6 – Aerosols impact air quality . . . as well as scattering and . . .
18. It appears that Figure 9 is discussed before Figure 8. If so, then you should switch those around.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 14417, 2011.