Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Feedlot is a unique and constant source of atmospheric ice-nucleating particles" *by* Naruki Hiranuma et al.

Anonymous Referee #2

Received and published: 25 January 2021

This paper is focused on the potential of domesticated animal feeding facilities as sources of atmospheric ice nucleating particles (INPs). Assessing anthropogenic sources of INPs is an important area of research. Hence, this is an area of research suitable for publication in ACP.

However, there are some significant problems with this paper. Most importantly, the paper is not well written. These authors, including the first author, have produced some excellent pieces of work in the past so I know they are capable of much better. I do not wish to spend a great deal of time going from line to line trying to edit the manuscript for them. Instead I will focus on several key areas, which I'll work through here:

1. 'Feedlot': it may be obvious what this is to a farmer in the USA, but it is not obvious

what this is to the wider community. I had to google the term to find out. An alternative title could be 'Cattle feeding facilities in the USA are a sources of . . . .'

2. The abstract needs to be rewritten. Tell the reader about the conclusions of your work, not the topics you cover without an indication of what the key results and conclusions are. E.g.: 'New data on the ice nucleation (IN) properties of agricultural dust at heterogeneous freezing temperatures (Ts > -29°C) were generated, providing statistical context.'; 'Overall, we successfully characterized physical, chemical, and biological properties of aerosol particles found at a cattle feedlot'; 'The relationship between these measured properties and atmospheric IN parameterization relevant to mixed-phase clouds is discussed.'; 'Our INP parameterization and ICR characterization are meaningful for improved understanding of INP emission and cloud microphysical processes in the supermicron-particle laden region'.

3. Intro: these paragraphs are far too long and confusing. Break up into topics and build a logical case for this study.

4. P2 ln 45. 'Milling and grinding'. It is inappropriate to mill natural samples of material where the larger sizes are likely made of different materials to the finer aerosolisable fraction. If, for example, the largest grains are ice active and they are milled down to sizes of atmospheric relevance then the ice nucleating ability you measure is likely to be simply a product of the milling process. Milled natural dusts like these samples are therefore not a meaningful proxy for the dust that may be aerosolised from this source, unless there is some justification for treating the sample in this way.

5. Ln 46: Dry heat tests: what precedent is there for 100 C being a suitable test for deactivation of INP proteins? Ideally we would be shown control experiments with a biological ice nucleator.

6. Ln 48: Wet heat tests: Clarify the procedure here. The normal practice is to place a sealed vial in a volume of boiling water. The way the text reads is that the sample itself was boiled for 20 minutes. If this was the case, then how was the loss of water from

the sample accounted for?

7. P7. Section 3.3/ The first two paragraphs have little to do with the heading of this section.

8. Section 3.4 and Table 5. Why is there a parameterisation for each sample? This seems excessive. It would be more useful to have a single parameterisation with an indication of variability.

9. P7, ln 34. 'this parameterization can be easily incorporated in many model platforms'. The authors need to be more specific how they envisage that this might happen. Do relevant models have this source of dust in them already with the emission already set up as an independent tracer? I suspect the answer is no. So, what else would we need to know in order to be able to represent this source of INP?

10. Ln 46. ' no notable difference after dry-heating was observed for both TXD01 and TXD05, representing an important negative result'. Why is this important? Is it significant that there is no effect of heating the sample dry to 100 C. It would only be significant if IN proteins are known to deactivate on heating to this temperature.

11. P 8 section 3.7: State which ns parameterisation was used in this calculation

12. P 8. Section 3.7: Equation 1 is only appropriate if a small fraction of INP at any one size is activated to ice. This equation does not take into account the number of dust particles. For example, if there were 10 dust particles per cm3, this parameterisation might predict 1000 dust cm-3, which would clearly be nonsense. To predict INP concentrations using an ns (or similar) parameterisation, you either need to be able to prove that only a small fraction of particles at one size over the whole size range activate to ice or integrate over the size distribution.

13. P8. Section 3.7: Why use ns. The data seems to spread out in ns substantially, but in nm, they collapse. Hence, nm seems to be a better way of doing this calculation.

14. Ln 50. 'which is three orders of magnitude higher than typical ambient INP con-

centration from continental sources'. This is a selective reading of the literature. The values are certainly high, but they are not 1000 times higher than recent literature values. For example, other studies also report high INP concs: Petters and Wright (2015) show values up to 1000 L-1 and O'Sullivan et al. (2018) report values approaching 100 L-1 and Suski et al. (2018) report values in excess of 100 L-1.

15. P 9 ln 4. What are 'controlled-experiments'?

16. Conclusions: I found this hard to follow. There is a lack of structure and several statements do not seem to follow on logically. E.g.: In ' The insignificance of dry-heating was demonstrated with the increase of organics found for the ICR of dry-heated samples' the second statement does not follow on from the first.

References

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