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Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Retrieval of ice nucleating particle concentrations from lidar observations: Comparison with airborne in-situ measurements from UAVs" *by* Eleni Marinou et al.

Anonymous Referee #1

Received and published: 11 January 2019

General comments:
Marinou et al. show nicely the combination of field and laboratory work. Aerosol surface area concentrations derived from lidar observational data are used together with ice nucleation parameterizations derived from laboratory experiments to determine vertical profiles of aerosol-specific INP concentrations. The method is not know, but the authors included more state-of-the-art ice nucleation parameterizations and compared the INP number concentrations with offline analyzed filter samples taken with an UAV.
The comparison looks very promising. The authors show both immersion freezing and deposition nucleation nINP, although in the presented cases deposition nucleation

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would be very unlikely. It would be nice to see a follow-up study for a real deposition nucleation case.

The manuscript is well structured, however some paragraphs are unnecessarily long, e.g. description of the differences of the parameterizations. Your focus is the case 20-22 April. So in my opinion you should shorten section 4.1 (description of the other cases) or discuss the other cases similarly.
The manuscript is well written, but I would propose to the authors going carefully through the paper and eliminate the typos and grammar error (some of them listed in the Technical comments section).

Specific comments:

1. Abstract A major point in your work is the comparison with the FRIDGE INP measurements from filters taken with a UAV. However, this is not mentioned in the abstract.
2. p. 2 l. 19 “about 1 in a million aerosol particles act as INP” This statement is well known, but I would prefer a reference.
3. p. 3 l. 1-3 This finding is not limited to field studies.
4. p. 4 l. 5 As far as I see, this listing is general. If so, than you might add the review by Murray et al. (2012) for another soot (immersion freezing) parameterization.
5. Table 1 First, to increase consistency you should use either K or degC. Second, the parameterization function U17-imm dust is wrong, if T is in K (as in the other equations)!

6. p. 4 l. 33-35 This statement is true, but D15 uses for its parameterization next to lab data also field data and therefor, the explanation for the discrepancy is not appropriate.

7. p. 5-6 First, the ordering is confusing, because the two nucleation mechanisms are mixed. Second, for the reader community a less technical description of the parameterizations would be valuable. It is obvious that soot and dust have a different ice nucleation behavior. I would suggest discussing the differences of the parameterizations in terms of the future outcome in your study. That means, when S15 shows a significant higher activated fraction then you would expect that the number of INP is much higher than for the U17-dep dust.
   However, the error discussion is very good.

8. p. 7 l. 3 "(. . .) several in-situ instruments were operated" for what? What did they measure? Be more specific or remove that, because you do not use these instruments.

9. p. 8 l. 10 Can you give a reference for the SAMUM experiment?

10. p. 8 l. 11 This number was not given in percent, right?

11. Section 4 and 4.1 In the very first part of Section 4 you describe detailed the case of 21 April, but not the other cases. These cases are discussed in Section 4.1. This is confusing for the reader.

12. p. 11 l. 27-28 What was the height for 5 April?

13. p. 11 l. 34, Figure 7 Ok, but there is a deviation from the 1:1 line especially for the high concentrations (or case 9 April). Do you have an explanation or can you comment on that?

14. p. 13, Figure 8 and 9 The discussion of the two figures is quite similar and at some point you repeat the findings. Maybe you can shorten this part.

15. p. 13 l. 18ff You did not this detailed discussion for deposition nucleation. Be more consistent.
16. p. 15, Figure 10 From the campaign, are there temperature and/or relative humidity measurements available e.g. from radiosondes? From the WRF temperature profiles, you could argue that deposition nucleation will not be the case for your study. Furthermore, you could add the approximate cloud base and top height in Figure 10.
17. Summary section The conclusion are very short. Maybe you can discuss in more detail what improvements you or the community can do to improve the outcome, e.g. collocated temperature/ humidity profiling for calculating the INP concentration at real conditions, or combined in-situ ice concentration measurements.

Technical corrections:

1. p. 1, l. 6 Either “(. . .) lidar measurements with a INP efficiency (. . .)” or “(. . .) lidar measurements with INP efficiency parameterizations (. . .)”
2. p. 1 l. 12 14 agrees
3. p. 1 l. 12 nINP not yet introduced
4. p. 2 l. 6 “Our analysis” either has shown or shows “that (. . .)”
5. p. 2 l. 8 gives
6. p. 2 l. 8 Neither n250,dry nor Sdry introduce
7. p. 2 l. 30 citation style in the brackets
8. p. 3 l. 32 UAV comes first here, write out in full

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9. p. 4 l. 3 citation style
10. p. 4 l. 16 AIDA comes first here, write out in full
11. p. 4 l. 27 “need to be transferred”
12. p. 4 l. 32 “(. . .) from Arizona, which have been (. . .) and are much more (. . .)”
13. p. 5 l. 5 devices
14. p. 5 l. 7 citation style
15. p. 5 l. 13 shown
16. p. 6 l. 4 desert
17. p. 6 l. 10, 13, 16 5 degC
18. p. 9 l. 28 “(. . .) and the Arabian Peninsula to the Eastern Mediterranean (. . .)”
19. p. 11 l. 28 seems
20. p. 12 l. 24 “(. . .) microscopy, which shows that (. . .)”
21. p. 13 l. 30 than instead of that
22. Figure 2 right figure Sdry has a wrong unit

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