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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Is a more physical representation of aerosol activation needed for simulations of fog?" *by* Craig Poku et al.

Anonymous Referee #1

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The authors investigate the effect of different aerosol activation schemes on the simulation of radiation fog. In this course the present a modified scheme that is better suited for fog than the available ones, basically because these were designed for clouds in which adiabatic cooling is the main source for cooling. In fog, however, radiative cooling is the major process leading to cooling and aerosol activation. Past simulation studies on radiation fog have shown that the simulated fog layers usually deepened too rapidly. The authors are trying to demonstrate that this is due to deficiencies in the activation parameterizations. I thus believe the value of this work is high and can be an important contribution to fog research; and it is well suited to be published in ACP.

I do have a number of issues and comments the authors should address in their revised manuscript. I suppose they can be considered to be overall minor, but one issue (see

below) is major.

Detailed comments:

1. line 13-14: the sentence reads odd. The minimum updraft velocity threshold overpredicts the droplet number in comparison to a cooling rate? What you are saying is that a cooling rate underpredicts a droplet number. How can a cooling rate/threshold predict something?

2. Introduction: You elaborate the current state of fog simulation in which often a bulk microphysics scheme is used, nowadays often with aerosol activation parameterization. However, you have missed important recent work that avoids aerosol activation parameterizations by using a Lagrangian cloud model (see Schwenkel & Maronga 202, https://www.mdpi.com/2073-4433/11/5/466). I do think it would be worth to discuss their paper in the introduction. How do your results relate to their LES? Would a direct comparison of the same case make sense? The same authors also recently published a related article in ACP (https://acp.copernicus.org/articles/19/7165/2019/acp-19-7165-2019.html). You do cite this close to the end of the manuscript, but I would say their work is so closely related to what you did that I would expect that you put your work into context of the two papers in the introduction.

3. Eq 6: What is the variable "L"? I think you do not define it at all.

4. Table 2 / case definition. To be honest, i had serious difficulties to follow your text at some places because the case definition appears a little chaotic. The is best seen in Table 2. Example: Case C_adiabatic has two tests, but for T_sip_ad has all aerosol modes and all environments. So how many simulations were performed? It is very difficult to read the table that way. It would be beneficial to list all simulations in a more straight forward order.

5. In the legend of Figure 1 you refer to "Aitken, accumulation, and coarse model aerosols". However, if you are not an expert in aerosol physics you might not know

what these modes are. And this is the first time these terms appear in the manuscript! Please provide an explanation to the reader at a suitable location. It might help to give a schematic diagram showing these modes.

6. line 224: How can you motivate a grid spacing without given the grid spacing? Furthermore, you cite studies that clearly state that 1 m grid spacings is best choice, but you use only 2 m. Given the very limited horizontal domain, I have to ask: why did you use such "coarse" grid spacing? It does not make sense to me.

7. This is one of my major concerns. Your paper is supposed to illustrate that the SMOD scheme gives a smoother/delayed transition to deep fog and thus is more realistic. But what is almost completely missing is evidence for this. First of all, you show the LANFEX observations, but you do not really compare your LES results against them. And what I see is actually, that all runs performed are way off from the observations. So how can you conclude that the SMOD scheme is more realistic? It appears a rather academic idea that is not proven by better agreement with observations for instance (you see rather bad agreement e.g in Figures 3, 5, and 7). Second, where can we see this smoother transition to deep fog? The longwave downwelling radiation is a good indicator. Looking at Figure 7, however, it looks like the fog layer in general remains less thick, no matter what scheme is used. The transitions, however, happen at the same time. And even worse: in the LES all runs go into deep fog mode quickly, while the observations indicate shallow fog or no fog until 21:00, and then a rapid thickening. I would say the Shipway scheme here is the only scheme given the same rapid jump; but at a different time and different radiation level.

8. line 382: with respect to my previous comment, I doubt that here is enough evidence for this.

Typos, language, etc.:

1. line 10: do you mean "initial fog droplet number concentration"?

2. line 30-31:This sentence appears trivial for ACP and can be removed.

3. throughout text and equations: put index letters in non-italic font unless they are variables themselves

4. line 154: "potentially"?!

5. Figure 1:label on y-axis for (a) should be s_max instead of S_max (to be in line with the nomenclature)

6. line 215: What is MONC?

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