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Discussions

Interactive comment on "A DNS study of aerosol and small-scale cloud turbulence interaction" *by* N. Babkovskaia et al.

Anonymous Referee #2

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This article studies the effect of aerosol dynamics on atmospheric small-scale turbulence using direct numerical simulations.

As I already pointed out in my original assessment of the article, my main concern with this article are the extreme initial conditions that were chosen for the simulations: While I understand the concept of fluctuations and the concurrent possibility of achieving extreme values, it is very hard for me to assess how relevant it is to study such an extreme case outside of that context. To elaborate on what I mean, let's take the article by Kulmala et al. that has also been cited by the authors: Kulmala et al. treat the saturation ratio (let's call it S' here, because S is used for the supersaturation in the present article) as a stochastical variable with a Gaussian distribution around an average value which varies from 0.995 to 1.0 with a standard deviation of up to 0.05. They then conduct a series of simulations where they allow the saturation ratio to vary

randomly according to the assumed distribution and find that particles can activate also in under-saturated conditions due to the temporal fluctuations in the saturation ratio. In the present paper, the authors pick one very extreme case out of this distribution, which corresponds to a saturation ratio of 1.1 or a supersaturation of 10 % (I can only guess that they still assume the average S' in the cloud to be equal to one). Just to put this into context, common supersaturation values at the base of a cloud are of the order of 0.1 to 0.5 %; according to a quick test conducted with a cloud parcel model that does not consider fluctuations, it reqires a particle number concentration of 1 cm^{-3} and an updraft velocity of 10 m/s to achieve a supersaturation of 10 %. Furthermore the authors chose a very high temperature difference between the simulation domain and its surroundings, which is not motivated in the text at all. According to these extreme initial conditions, the authors then also find that aerosols have a strong influence on turbulence, but I wonder how justifiable such a conclusion is without also considering more moderate supersaturations which are, after all, much more likely to occur. Furthermore, I am a little bit skeptic how reliable the results presented here are, as the simulations include the use of random numbers (this especially concerns the generation of the initial turbulence) and thus a single simulation may not be very representable of an average behaviour.

To conclude, I cannot recommend this article for publication in the current form. At the very least the paper requires one more set of reference simulations with a more conventional supersaturation of, say, 0.3 %, and a proper discussion on the issues I layed out above.

Concrete remarks

1. The English is not very good and needs to be reviewed. Some of the sentences are very hard to understand.

2. *lines 12–14:* Latent heat release is a time dependent process, but finally, close to equilibrium, the total cooling depends (nearly) only on the initial amount of supersaturation. It is therefore unnecessary to state that "even small amounts of aerosols increase the air temperature", and it is quite misleading to give the (very high) change in temperature of 1 K without also giving the value for the supersaturation used.

3. *lines 115–120:* Why is the chemical composition of the air important to this study? Wouldn't it be enough to state the total water content?

4. *lines 126–129:* Is this the total difference in temperature/water vapor content between the (upper and lower?) edges of the domain (e.g. 0.001 K/10 cm)? According to the units it cannot be a gradient.

5. *lines 133–135:* How exactly was this range of supersaturation derived?

6. *line 264:* I have never heard the word "unequilibrium" before – I believe the correct term is "non-equilibrium".

7. *Figure 1:* The velocity fields in panels (a) and (c) look curiously similar, while they are totally different in panel (b). Did you use exactly the same initial velocity fields in all three cases (or probably a different one for $N_{\text{tot}} = 555$ cm^{-3})? How much does varying the particle concentration actually affect turbulence?

8. *lines 306–317:* How does the initial supersaturation vary? Are these the values that you mention in lines 133–135? If so, it is not obvious how only the temperature is responsible for this variation in supersaturation, as you also set a gradient in water vapour mixing ratio. I think a figure that clearly lays out the initial conditions of the simulations would be very helpful in the overall understandability of this paper.

9. *lines 323–326:* Can you somehow quantify how long it would take the system to equilibrate, "more than 3 s" is pretty vague?

10. *lines 341–347:* As the condensational growth of particles is limited by the total surface area, it is not very surprising that the water vapor mixing ratio decreases faster if more particles are present. Also, as for this reason the supersaturation is always larger for smaller number concentrations, it should be expected that these particles grow larger. I don't know if this really "confirms the Twomey effect" or if it rather confirms that the model works as it should.

11. *line 359:* Replace "number" with "fraction".

12. *Figure 5:* Why does the fraction of activated particles go down after about half a second for $N_{\text{tot}} = 55.5$ cm^{-3}?

13. *lines 380–384:* I don't understand the relevance of this statement. As you have the same total water content in all three cases to begin with, the difference in LWC very strongly depends on the time at which you compare the simulations (apart from some small corrections due to temperature and droplet size), because in some simulations the droplets still grow while in others they don't. How would this difference change if you waited 6 instead of 3 seconds?

14. *lines 385–399 and Figure 6:* How exactly does the buoyancy force affect the turbulence in the simulation domain at scales smaller than 10 cm? This is not discussed at all in the manuscript. How valid is the assumption of a temperature difference of 7.6 K between the simulation domain and its surroundings, especially when the cloud parcel is colder than the rest? From Figure 6 I roughly estimate a deceleration of the air parcel of 25 cm s^{-2} (solid black line), so the air parcel drops more than 1 m (more than ten times the domain size) during the 3 s simulation, still the environment temperature never changes. How does this conform with the original statement that the temperature in the cloud is subject to

strong fluctuations? On a side note, if the temperature changes by 1 K, it is not surprising that the buoyancy force changes quite a bit and I wonder if Figure 6 is necessary at all – why not just give a value for B in the different cases? Furthermore, what does the comparison of the two simulations tell us. No aerosols means no cloud – should you rather compare between different aerosol loadings to somehow mimic what could happen at the edge of the cloud?

15. *lines 400–410:* To my eye the TKE in Figure 7 only differs significantly between aerosols and no aerosol during a short period of time around 2 s. Accordingly, Figure 8 shows a strong jump from roughly zero up to 80 % at that point in time and starts dropping off again thereafter. Still, from Figure 8 the authors conclude that aerosols affect turbulence strongly. How sure are you that these results are not sensitive to the specific set of random numbers used for the simulation (i.e. what would happen if you used a different random seed in the setup)?

16. *lines 406–410:* This sentence is very hard to understand. Do you mean to say that the temperature *difference* between the domain and its surroundings *decreases* because the temperature inside the domain *increases*?