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

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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 S0 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

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Discussion paper

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 S0 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 cmôĂĂĂ3 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.

According to the referee's comment we have made three additional runs

- the difference between the temperature in the simulation domain and its surroundings of 2.5 K and aerosol particles are included

- the difference between the temperature in the simulation domain and its surroundings of 2.5 K and aerosol particles are not included

- humidity is 10 % smaller than it was in the previous simulations and therefore, supersaturation averaged over domain is 0.6

The last section about the effect of aerosol on the turbulent motion is rewritten based on new input parameters.

Concrete remarks

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

We double checked the English and rewritten unclear parts.

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.

Reformulated:

We find that the even small amount of aerosol particles (55.5 cm-3) strongly affects the air temperature due to release of latent heat.

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?

The model used in this study was prepared for problems with more complicated chemistry. We are planning to develop it in future.

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.

Reformulated:

Based on data of CARRIBA observations typical for the upper parts of clouds / cloud edges in a height of 2000 m, we set the initial conditions for air temperature ($T0 = 285.4$ K) and water vapour mixing ratio ($q\_0= 0.0124$). The small vertical gradients of temperature and water content are also based on the CARRIBA measurements: the total difference between values of air temperature and water vapour mixing ratio at the upper and lower edges of the domain are (Delta T = 0.001 K) and (Delta q= 4 10-5), correspondingly.

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

The take measurements for temperature and absolute humidity (AH). S=(AH*rho*pw/ps-1)*100%

AH=Yw/rho Yw is water vapour mass fraction rho is air density pw=rho RT/18 is water vapour pressure psf is saturation water vapour pressure

rho=9.6 10-4 g/cm3 Yw= 0.0123759 T=285.4 K

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

We agree that the phrase “non-equilibrium” is more common and we change accordingly

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 Ntot = 555 cm-3)? How much does varying the particle concentration actually affect turbulence?

Velocity field is taken the same in all free cases. Panels (a) and (c) look similar because the positions of warm/cold layer are similar there (comparing with panel (b)). But it was double checked that the difference between (a) and (c) exists.

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.

In the first version of the manuscript such figures were included but the other referee asked to remove them because in his opinion they were not informative.

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

We added a new figure to show time dependence of supersaturation averaged over domain for Ntot=55.5, 555 and 5550.

In this Fig. we present the supersaturation averaged over domain for {\it cases 1, 2, 3 and 4} (see Table 1) and analyze the relaxation time of supersaturation tr for different values of initial supersaturation and total number of particles. Analyzing numerical results we get the relaxation time of about 0.77 s (case 2), 0.17 s (case 3), and 0.6 s (case 4). In case 1 the relaxation time is larger than time of simulations. We fit the corresponding curve by the function exp(-t/tau) and estimate the relaxation time of 4 s.

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.

Removed the sentence about Twomey effect

11. line 359: Replace “number” with “fraction”.

replaced

12. Figure 5: Why does the fraction of activated particles go down after about half a second for Ntot = 55:5 cm-3?

This is most probably the effect of nonlinearity of activation process and periodic BC. There are no such fluctuations in other two cases because in these two cases the system equilibrates very fast.

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?

Added to the text that this result depends on time

We find that at t= 3 s the number of activated particles is proportional to the total number, whereas the change of Ntot by a factor of hundred increases LWC by approximately 40 %.

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 s2 (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?

As it was mention at the beginning of this respond we have prepared results of simulations in a case of smaller difference between the temperature inside the domain and its surroundings (delta T= 2.5 K). Moreover, now the temperature inside the domain is larger than outside the domain and we get updraft instead of downdraft. We agree that new parameters are more realistic in atmosphere. However, our general conclusion concerning the effect of aerosol on turbulence does not change. We showed in the last section that the aerosol affects the turbulence through the buoyancy. However, since now temperature difference changes sign the vertical air motion is accelerated (rather than decelerated as it was before) if aerosol particles are present.

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)?

We agree that to make the general conclusions about the effect of the turbulence one set of parameters are not sufficient, and since the turbulence is very complicated process some statistics is needed. But the DNS are very computationally demanded and even one set of parameters needs huge amount of time and resources. In this study we illustrate how aerosol important or not important for correct description of turbulent motion, taking conditions typical for atmosphere.

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?

Yes.

But now we take warmer domain and cooler its surroundings and instead of decrease/deceleration we get increase/acceleration effect.

This part is reformulated:

The air temperature increases because of release of latent heat caused by condensation onto drops, and therefore, the difference between temperatures inside and outside the domain is enlarged. It results in increased buoyant force and acceleration in vertical direction.

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