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

Anonymous Referee #1

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General:

The study on aerosol particle dynamic effects is a spectacular idea and performance on small scale variation effects on cloud properties such as activated particles and temperature effects usually either ignored or simply parameterized. The approach and the implications for example for larger scale aerosol particle – cloud effect calculations matches nicely in the scope of Atmospheric Chemistry and Physics and the results are quite interesting. However before accepting the present study I would recommend several technical improvements and clarifications in order to support readers not essentially familiar with all the details to follow the arguments and the implications for larger scale simulations. Those include first of all the English. Please have a native English speakers check on the sentences!

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Discussion paper

Detailed questions and comments:

- The total number of particles was varied between two orders of magnitude, which was extracted from reasonable values measured. This is appropriate and reasonable. However, what about the impact of different size ranges e.g. mode concentrations on the results? Do the results change notably for particles in the accumulation and in the coarse mode due to critical sizes for activation for the salt particles assumed? Would results differ for changing certain size bin concentrations (i.e. modes) instead of the whole number? Are these salt particles already "activated" or assumed "dry" for the simulations conducted? I guess once any of these particles has faced substantial humidity it will grow much easier than if it has to dissolve first.

- Abstract, p.1: "The system comes to an equilibrium faster and the relative number of activated particles appears to be smaller for larger N_{tot}." seems to be formulated very simple. I doubt that for a large part of atmospheric processes equilibrium conditions are hardly reached. What is the criteria for achieving an equilibrium condition in this case and for which simulation conditions the equilibrium approach becomes invalid?

- Description of the model, p. 2: The order of figures seems somewhat arbitrary, as Fig. 3 appears earlier than Fig. 1.

- p. 2, l. 106ff: The particle size distribution displays a sharp maximum close to a diameter of around 5 micron. Please refer to the origin of observations (reference, location etc.) mentioned in the text.

- p. 2, l. 127: It's being referred to a temperature gradient of 0.001 K. Two questions on that: (i) which gradient, i.e. temperature change over which distance, horizontal, vertical etc.? Only a temperature unit is provided. (ii) This temperature change is pretty tiny although important. What is the reliability range of this because of numerical diffusion and linearization of equations for simulation? Please provide a temperature gradient and either a short statement of simulation uncertainty or a $\pm$ value.

- p. 3, Fig. 2: I do understand the intention to maximize differences in the colour scale to make aspects visible. However, since in here three situations are compared with, please use the same scale for all the three upper and all the three lower plots. This would allow a better comparison and an even improved identification of the changes.

- p. 3, l. 163ff: Please reformulate: "... and the usual equilibrium supersaturation would be restored.". I doubt an equilibrium supersaturation, as water tends to equilibrate at saturation. If you mean different, please reformulate to make it clearer.

- p. 3, l. 172ff: Please check: "If the phase relaxation ... would be applicable." There seem to be too many words. Is the word "than" dispensible?

- p. 4, l. 229: You state that the number of particles stays constant. This contradicts the explanation of an aerosol particle dynamic study. Are changes if calculated in the corresponding simulation time negligible? Otherwise this may matter as e.g. larger cloud droplets grow on the expense of smaller droplets and they modify the size spectrum and number density.

- p. 4, l. 266 and p. 2, Table 2: The change in temperature between equilibrium and unequilibrium case seems fairly huge! 8K would cause a strong vertical uplift, a strong local mixing (dilution), which would require a remarkable mass of condensed water vapour (several grams per m^3). Did I get something wrong?

- p. 4, l. 278f: The temperature is averaged in y-direction. If you have notable differences in x- and z-direction, how does this assumption affect the results? To a negligible extend?

- p. 5, Table 3: I don't understand the listed maximal and minimal values of supersaturation S as they are negative. This would imply a subsaturation as S = 1-p/psat0 with p and psat0 the vapour pressures of water at present and at saturation level. Second, very interesting is the change between cases 1 and 2. There seems to be a tipping point at a certain total particle number concentation. Could you provide a comment on that as the changes by a factor of ten is substantial?

- p. 6, l.298f: You state that the simulated results occur because of the effect of total number concentration. Why? I guess a certain limit of aerosol particles – here all assumed to be identical in chemical composition and water solubility – exists, below which the time of diffusion of water vapour to the next aerosol particle is too long to achieve the same amount of condensation. Because of the particles size (predominantly beyond 1 micron in diameter) hardly any curvature effects on saturation vapour pressure can be expected. If so, could you name the cutting point for the conditions simulated in here?

- p. 6, l. 330: The point mentioned above feeds back to the statement dealing with the activation radius assumed. Why exactly 1.75 micron? This should depend on supersaturation. “. . .the results of this study were not sensitive on the choice of “ the activation radius. My guess (!) is that this is valid for the cases 2 and 3 but not for case 1. Do you agree or disagree?

- Fig. 6: The calculated vertical velocities of 0.6 to 0.7 m/s at maximum are remarkable. It is indicated that this intensifies over time although a steady-state or “equilibrium” is to be achieved after a second or somewhat more.

- p. 6, Fig., 7: “The dependence of the average...” turbulent “kinetic energy. . .”. Please insert.

- p.7, 353f: Aerosol dynamics are neglected. This sounds different in the abstract as it is stated that in order “to study effects of aerosol dynamics on the turbulence we vary. . .”. Please name explicitly in the methods section not to use aerosol dynamics and state that this is valid because of the short total simulation time used.

- p. 7, l. 380ff: “We find that the number . . . linearly depends. . ..” Please check the English and be careful when using three simulations only. Especially Table 3 (p. 5) contradicts. Better skip that sentence or perform more simulations in more narrow Ntot steps.

- p. 7, l. 400f: “We find that the vertical motion of air is decelerated because of aerosol dynamics.” This contradicts to the statement of neglecting aerosol dynamics (condensation and coagulation) during the period of simulation (p. 7, l. 353)! Please check.

- p. 8, l. 405ff: You explain the air temperature change driven by the condensation of water vapour onto the aerosol particles and the release of latent heat. But since the aerosol particles are rather huge size shouldn’t matter and the condensation should occur independent on the number if any particle number and time are available. But the change differs notably between 55 and 550 cm^{-3} and I can only think of not sufficient time for condensation.

- p. 8, l. 436f. The information on the model sizes is very nice but would be best to include it earlier in the methods section for a better understanding on set-up and interpretation of results.

- p. 8, l. 450ff: Very nice indeed. But simulating a 10x10x10 cm^3 volume this would cause dramatic horizontal and vertical gradients and motion. Is this still applicable by the present method including the problematic areas along the edges of the finite volume?

- p. 8, end: Very nice and interesting results indeed. I would recommend a short statement to potential implications for cloud simulations and weather prognosis. This would definitely increase the range of potential readers, for which the area is highly relevant.

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