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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Theoretical basis for convective invigoration due to increased aerosol concentration" *by* Z. J. Lebo and J. H. Seinfeld

J. Fan

jiwen.fan@pnl.gov

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I found some of statements about the literature modeling studies on aerosol effects are not true. Also, the authors seem to be a little arbitrary in drawing some of conclusions. Some discussions or results could change given a different case (see below for some details). I would suggest the authors tighten their discussion or conclusions to their case conditions (vertical wind shear, humidity profiles, isolated clouds or cloud ensemble, etc.) instead of generalizing them.

First, about model resolution, the statement "The model used in the present study differs from those of previous works, (e.g., Fan et al., 2009; Khain and Lynn, 2009), in that we simulate the evolution of deep convective clouds at a much higher resolution" is not right. Fan et al 2009 (JGR, 114, D22206) actually used the model resolution of

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500 m. In fact, many of Khain's past studies such as Khain et al. 2004, 2005, and 2009 used resolution of less than 1.5 km, the resolution used in this study. With 3-D bin microphysics, Fan et al 2009 (J. Geophys. Res., 114, D04205) and Fan et al 2010 (ERL) used the resolution of 100 m for mixed-phase stratocumulus and 500 m for deep convective clouds in terms of aerosol effects, respectively. Many studies from C. Wang and A. Ekman (such as Ekman et al. Q. J. R. Meteorol. Soc. 133) also looked into aerosol effects with 3-D bulk schemes using resolution 500 m or 1 km. So, the statements on p2278 "some of the previous studies have used two-dimensional models (e.g., Khain et al., 2004, 2008; Teller and Levin, 2006) and others that have simulated all three dimensions (e.g., Khain and Lynn, 2009) have been performed at rather low spatial resolution, i.e., >2 km in the horizontal" is also not true. There are many other places discussing resolution in the paper, which need to be corrected consistently.

Second, the statements about the findings in Fan et al. 2009 (JGR, 114, D22206) are not correct either. About "Fan et al. (2009) showed that in regions with high vertical wind shear, additional aerosol particles are unable to significantly alter the cloud microphysics and thus little change in the surface precipitation is predicted. When the vertical wind shear is reduced, convection is shown to be invigorated due to increased aerosol loading", the authors repeated it a few times in the manuscript, but where the authors got this? The paper has figures showing vertical velocity and precipitation are significantly reduced by CCN with the high vertical wind shear, which is one of the major conclusions of the paper. I do not understand why the authors changed the conclusion from Fan et al. 2009 (JGR, 114, D22206) by saying something like "additional aerosol particles are unable to significantly alter the cloud microphysics and thus little change in the surface precipitation is predicted".

Third, in Introduction, "The ice phase presents significant complexities not present in warm clouds (i.e., riming, aggregation, accretion, heterogeneous and homogeneous freezing, melting, etc.), and the cold-rain process is the predominant mechanism by which rain forms (not collision-coalescence of liquid drops)". Where is the evidence for “the cold-rain process is the predominant mechanism by which rain forms”? Whether the cold-rain process is predominant really depends on cloud types and meteorological conditions. It is dangerous to generalize it from a single case or study. In addition, the first half of the sentence is not accurate. Strictly, you could say “Mixed-phase and ice phase clouds present significant complexities not present in liquid-only clouds”.

In Abstract, “. . .detailed two-bulk microphysics scheme, which is more computationally efficient than bin microphysics schemes, may not be sufficient, even when coupled to a detailed activation scheme, to predict . . .”. I do not think you can say this by this study, there are many other processes besides droplet activation in the bulk scheme that can be improved. Autoconversion, riming, diffusional growth, etc, all this processes in bulk schemes are of large uncertainty, which can be improved. Moreover, there are different parameterizations for these processes that can be tested. How do you know that the bulk scheme would not work without doing thorough tests of other parameterizations and without implementing further improvements? Seifert et al (2006, Atmos Res, 80, 46-66) did tuned a bulk scheme which can predict similar results as the bin scheme.

Additional suggestions/comments

1. In Section 2.3, Khain et al 2008 and Fan et al 2009 can be listed together there in terms of the significant findings on aerosol effects under different environmental conditions. The authors tried to simplify their study without involving in wind shear effects, then how can you provide the theoretical basis for convective invigoration by CCN? Many microphysical processes such as droplet evaporation and ice sublimation and dynamic processes such as entrainments and cool pool are highly dependent on vertical wind shear.

2. About the IN effect, the conclusion “The effect of an increase in the IN number concentration on the dynamics of deep convective clouds is small. . .” agrees with what Fan et al. 2010 (ERL) found about IN effects on convection. Fan et al. 2010 (ERL) also indicated that IN effects is highly dependent on mid-tropospheric humidity. So, much

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discussion in Section 6 should be only limited to the specific case that the authors simulate and can not be generalized.

3. In Section 6, "as the IN number concentration increases, physically the number of droplets that freeze and consequently grow via vapor diffusion should increase at warmer temperatures…". The number of droplets that freeze increases with IN can only happen through immersion or contact freezing. Is your immersion freezing scheme employed in the model connected with both IN and droplets? If so, which scheme? Generally, the increase of IN would increase ice crystal concentrations a lot through deposition/condenstional freezing (i.e., IN become ice crystals instantly), which would reduce/deplete liquid water by droplet evaporation.

4. The first study detailing how aerosol effects are changed with the increase of RH is Fan et al 2007 (JGR, 112, D1420400). Many results shown in Section 5.1.1 and 5.1.2 are similar to those in that study.

5. "We show in Fig. 19 that in fact the mean sedimentation rate of condensed water is suppressed for a doubling in the number of active IN diagnosed in the bin microphysics model compared to both the "Clean" and "Semi-Polluted" cases" in p 2808. Why is the mean sedimentation rate of condensed water suppressed? You showed that with bulk the sedimentation rate is increased since IN increase ice growth, then why it is opposite with the bin scheme?

6. Since the bin scheme is not new, it is not necessary to detail all the processes, given the paper is already too long.

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