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Interactive comment on "Turbulent enhancement of radar reflectivity factor for polydisperse cloud droplets" *by* Keigo Matsuda and Ryo Onishi

Anonymous Referee #1

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General evaluation: This paper reports development and application of a parameterization that describes coherent Bragg scattering of electromagnetic waves due to inertial clustering of cloud drops. This work extends the work of Matsuda et al. (JAS 2014) in two important elements: i) the formulation of the radar reflectivity is extended to include polydisperse drop size distributions, and ii) the parameterization of the clustering effect on the radar reflectivity is applied to a simulation of a cloud field of shallow precipitating cumulus clouds. Both elements are interesting and together they make the paper a useful contribution. However, the presentation requires some revisions before it is accepted. The most significant problem concerns the impact of turbulent mixing on the Bragg scattering in natural clouds and lack of its representation in the numerical simulation (the major point below). I also have a few specific comments that require

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authors' attention.

Major comment:

Bragg scattering comes from fluctuation of the refractive index at scales comparable to the radar wavelength. In natural clouds (e.g., Knight and Miller JAS 1998), the Bragg scattering leads to the called "mantle echo". The mantle echo likely comes from the temperature, water vapor, and cloud water fluctuation resulting from the turbulent mixing between a cloud and its environment. I am not sure if the issue of which field fluctuations (temperature, water vapor, or cloud water) contribute most to the mantle echo is settled. But I would think that inertial droplet clustering plays insignificant role in highly inhomogeneous volumes diluted by entrainment near cloud edges that undergo turbulent stirring. In fact, because no mantle echo is simulated by the model, I feel the cloud field simulation is flawed in this respect. I feel the revised paper should include a more thorough discussion of the problem, including the missing impact of the subgrid-scale heterogeneities due to entrainment and mixing on the Bragg scattering. For the general introduction to the problem, I found the introduction to Matsuda et al. JAS 2014 paper much better.

Specific comments (more serious with *).

1. P 2. I think here the issue of what causes Bragg scattering should be introduced and discussed.

2*. I consider the omission of the gravitational acceleration in DNS simulations a serious problem. There is an extensive discussion in the literature to what extent droplet sedimentation is important for the clustering problem in natural clouds starting with Grabowski and Vaillencourt (JAS 1999). I do not think one can dismiss the impact of gravity that easily. In fact, the volumes where I expect clustering to be more important than turbulent mixing (i.e., weakly diluted cloudy volumes away from cloud edges) should feature small dissipation rates (in contrast to what line 4 on p. 6 says). Weak turbulence makes the sedimentation more important.

3. The smallest Stokes number considered in DNS simulation (0.05) is probably still too large for small cloud droplets and low dissipation rates.

4. I only skimmed over theoretical sections of the paper and have no comments on them.

5. P. 15. I think explaining how cloud droplets are activated would be useful. The RICO case description only states CCN concentration should be taken as 100 per cc, but no details about the activation are provided. Please add.

6*. I do not like how the eddy dissipation rate in (39) is prescribed. To me, including "resolved" and "sugbgrid-scale" contributions does not make sense. If you do not agree, please provide a reference to a previous study or a textbook that used such an approach. I assume that the model has a parameterization of unresolved turbulent transport, correct? Then this can be used to derived epsilon. Please see how others have done that, for instance, Seifert et al. for a simple Smagorinsky scheme or Wyszogrodzki et al. for a TKE scheme. Also, what is niu in (40).

7. P. 15 L. 26: Please define optical thickness.

8*. I find the discussion of RICO simulations superficial. Fig. 8 is interesting, but its discussion should be expanded. I think it would help if the LWC is plotted using a different color scale or the log scale so the extent of a cloud is shown. I have already mentioned that the simulation does not show the mantle echo. My explanation is that the simulated Bragg echo includes only droplet clustering contribution. I do not feel this is realistic considering entrainment and mixing as in my view this is the main reason for the mantle echo. Moreover, by design, the model assumes homogeneous mixing for the cloud microphysics (i.e., parameterized subgrid-scale transport and numerical diffusion are followed by an immediate homogenization of the grid volume). This is clearly unrealistic for the scales the model is able to resolve. As for the reason for the strongest simulated Bragg echo being located near the cloud top, there are two effects. First, TKE typically increase with height in shallow convection. Second, droplet size increases with height in weakly diluted volumes as well. These two work together to increase droplet clustering and lead to the largest Bragg contribution near the cloud top. Perhaps it would be interesting to know which effect is more important: the increase of droplet size or the increase of turbulence. I think all these need to be discussed emphasizing model limitation, that is, exclusion of subgrid-scale contribution to the Bragg echo.

9. Appendix A is short and should be included in the main text.

References:

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