

Reply to Referee #1

Thank you for your encouraging and insightful comments on our manuscript. We have intensely revised our manuscript according to your comments, particularly to the four major criticisms. The revised manuscript now includes the discussion of the mantle echo and considers the influence of gravitational acceleration. Below we answer all your comments one by one.

[Major criticism 1]

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.

According to this criticism, we have extensively modified the manuscript in order to include the discussion on the “mantle echo”. That is, we have incorporated the influences of particulate Bragg scattering due to turbulent entrainment of environmental air into clouds and clear-air Bragg scattering, which is caused by the refractive index fluctuations (i.e., temperature and humidity fluctuations). We have confirmed that the influence of turbulent clustering is still significant even if the mantle echo physics (i.e., particulate and clear-air Bragg scatterings) is included. The detailed calculation method of the two kinds of Bragg scatterings has been described in Subsection 5.2 and the results in Fig. 9 have been updated.

As described in detail in Subsection 5.2 in the revised manuscript, particulate and clear-air Bragg scatterings are considered for the calculation of the radar reflectivity factor Z . The contributions are denoted by Z_{PB} and Z_{CB} . For the calculation of the entrainment contribution to Z_{PB} (denoted by Z_{PBe}) and Z_{CB} , the $-5/3$ power law of the scalar concentration spectrum is assumed for the inertial-convective range. Figure 9 (c) now shows the results of Z considering the above Z_{PBe} and Z_{CB} in addition to the clustering contribution to Z_{PB} (denoted by Z_{PBc}). The newly-simulated radar echo (Fig. 9 (c)) shows a radar echo layer at the height from 2.2 to 2.5 km. This echo layer is caused by clear-air Bragg scattering due to a large humidity gap in the inversion layer. The echo figure, however, does not show a clear mantle echo. Knight and Miller (1998) reported that the mantle echo is poorly observed on the most humid day. Our result is in accord with this report as the relative humidity of the environmental air is above 80% at heights below about 2.2 km in our cloud simulation data. As you pointed out, particulate Bragg scattering due to turbulent entrainment is a possible cause of the mantle echo. The influence of turbulent entrainment turned out to be, however, not significant in our simulation results. Our interpretation is that the large-scale cloud water inhomogeneity

at cloud edges could produce small-scale fluctuations due to the turbulent cascade, but such fluctuations were not significantly large at the scale of the half wavelength in the present simulation.

According to the comment on Introduction, we have added the introduction of clear-air Bragg scattering and discussion about possible causes of the Bragg scattering observed by Knight and Miller (1998) in the revised manuscript.

[Minor criticism 1]

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

Accordingly, in Introduction, we have added the introduction of clear-air Bragg scattering and discussion about possible causes of the Bragg scattering observed by Knight and Miller (1998) in the revised manuscript.

[Major criticism 2]

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.*

The other referee also criticized about the omission of the gravitational acceleration. We therefore decided to include it in the revised manuscript. The modeling of the influence of gravitational settling is described in Subsection 4.3 and the reliability of the model is confirmed in Fig. 7 (b). The radar echo simulation in Fig. 9 has been obtained with the updated model that considers the gravitational settling. It has confirmed that the influence of turbulent clustering is still significant even if the gravitational settling is considered.

In our previous work (Matsuda et al., 2014), we confirmed that the settling influence on the power spectrum for monodisperse droplets is insignificant for $S_v < 3$, where S_v is the settling parameter defined by the ratio of the terminal velocity to the Kolmogorov velocity. However, it is true that the gravitational settling modifies the cross-over length of the radial distribution function (RDF) for bidisperse particles. Lu et al. (2010) analytically proposed the cross-over length for gravitational settling bidisperse particles. Following this analysis, we have modified our parameterization for the critical wavenumber ξ_c in Eq. (32) to consider the settling influence on the coherence. Since ξ_c is inversely proportional to the cross-over length, we propose the following correction based on the equation of Lu et al. (2010):

$$\xi_c = \frac{0.191}{|St_1 - St_2|} \left[1 + \frac{1}{3a_0} Fr^{-2} \right]^{-1/2}.$$

In order to confirm the reliability of the parametrization, we have performed additional DNSs for polydisperse droplets considering the gravitational settling. As shown in Fig. 7 (b), $E_{r3np}^*(\xi)$ values obtained by the additional DNSs are smaller than those for the case without gravitational settling at large wavenumbers, indicating

that the coherence model is more important than the case without gravitational settling. $E_{r_{3np}}^*(\xi)$ values predicted by our modified parameterization show good agreement with those of the DNS results with gravitational settling. We have also confirmed that the RMS error e_{RMS} evaluated by Eq. (39) remains smaller than 1 dB even for the case with gravitational settling. These results indicate that the proposed parameterization can predict the influence of turbulent clustering for polydisperse droplets considering the gravity effect within 1 dB error.

We have added Subsection 4.3 to explain the modeling of the influence of gravitational settling. We have also modified the computational conditions accordingly in Subsection 3.1. The clustering influence in Figure 9 is also updated using the parameterization considering the gravitational settling influence. The results confirm that the influence of turbulent clustering is still significant even if the gravitational acceleration is considered.

[Minor criticism 2]

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

Both in Fig. 7 (a) and (b), the spectra obtained by the proposed model show good agreement with those obtained by the DNS for the case of CUMA_eps100, where the Stokes number for the modal radius was as small as 0.035 and the energy dissipation was as small as $100 \text{ cm}^2/\text{s}^3$. Thus, our parameterizations for the clustering influence for both gravity and non-gravity cases still work for particles with $St < 0.05$. It should be noted that the model prediction is less reliable for $St \ll 0.05$ but the influence of clustering on the radar reflectivity factor for such small droplets becomes negligibly small as well.

[Comment 1]

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

We appreciate your attention to the theoretical part.

[Minor criticism 3]

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.

Accordingly, we have added the explanation about the activation model in Subsection 5.1 in the revised manuscript as follows:

“The activation process of cloud condensation nuclei (CCN) was considered based on the Twomey's relationship between the number of activated CCN and the saturation ratio (Twomey, 1959). The activated droplets were added to the bins using the “prescribed spectrum” method (Soong, 1974). Detail of the model configuration is described in Onishi and Takahashi (2012).”

[Major criticism 3]

6*. *I do not like how the eddy dissipation rate in (39) is prescribed. To me, including “resolved” and “subgrid-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 ν in (40).*

We have removed the “resolved scale” contribution to the dissipation rate as it is indeed negligibly small compared to the “subgrid scale” contribution in the present LES. The corresponding equation, Eq. (44), and description have been modified accordingly.

Wyszogrodzki et al. (2013) used a TKE-based scheme to calculate the epsilon, and actually Seifert et al. (2010) used the same scheme to obtain the epsilon (Seifert et al. used the Smagorinsky scheme to obtain the TKE). Our scheme is based not on the TKE but on the velocity gradient tensor in the resolved scale. The epsilon in this scheme is directly derived from the LES filtering and the Smagorinsky eddy viscosity, and the contribution of the resolved scale is given separately using the molecular kinematic viscosity, which is ν in Eq. (40) in the previous manuscript.

[Minor criticism 4]

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

We have added the definition of the optical thickness in Subsection 5.3 in the revised manuscript:

“The optical thickness of each grid cell, τ_Δ , is visualized by volume rendering to mimic human-eye observations of clouds. Here, the optical thickness is defined by $\tau_\Delta = Q_{\text{ext}}\pi\langle r_p^2 \rangle n_p \Delta_z$, where Q_{ext} is the extinction efficiency for Mie scattering ($Q_{\text{ext}} = 2.0$ in this study), and $\langle r_p^2 \rangle = \int_0^\infty r_p^2 q_r(r_p) dr_p$. Note that the optical transmittance of cloud volume is approximately equal to $1 - \tau_\Delta$ when τ_Δ is sufficiently smaller than unity.”

[Major criticism 4]

8*. *I find the discussion of RICO simulations superficial. Fig. 8 is interesting, but its discussion should be expanded.*

- (1) *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.*
- (2) *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.*
- (3) *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.

- (1) Accordingly, the LWC distribution in Fig. 9 (a) has been replaced to the log scale to show the extent of clouds and precipitation.
- (2) As mentioned above (Major criticism 1), we have incorporated the particulate Bragg scattering due to turbulent entrainment of cloud volume with clear air and the clear-air Bragg scattering. We have added the discussion on the mantle echo and the influence of cloud water inhomogeneity in Subsection 5.3.
- (3) We have added the discussion on the height dependence of the clustering influence accordingly. The strong influence of turbulent clustering is caused not only by the energy dissipation rate (the contribution of turbulent intensity) and droplets size but also by the LWC. We have added Figure 9 (e), which shows the raw value of particulate Bragg scattering attributed to turbulent clustering, $Z_{\text{PBC}}^{\text{dB}}$, and explained the possible reason at the last paragraph of Subsection 5.3 in the revised manuscript as:

“In order to discuss the reason of the strong clustering influence at the near-top of the clouds, the raw value of $Z_{\text{PBC}}^{\text{dB}}$ is plotted in Fig. 9 (e). $Z_{\text{PBC}}^{\text{dB}}$ is larger than -10 dBZ inside the turbulent cloud region, where the LWC is larger than 0.1 g/m^3 and the energy dissipation rate ϵ is intermittently larger than $100 \text{ cm}^2/\text{s}^3$. Large values of $Z_{\text{PBC}}^{\text{dB}}$ are shown at the near-top inside this cloud region. We have confirmed that the droplet size in this cloud region was almost homogeneous: The volume-averaged droplet radius ranged within 7 to 11 μm . As a result, large values of the Stokes number (up to 0.05) distributed intermittently corresponding to the distribution of ϵ . The main factor of the height dependence of $Z_{\text{PBC}}^{\text{dB}}$ is the LWC, which is larger than 1 g/m^3 at the near-top of the clouds. Note that Z_{PBC} is proportional to square of the LWC as Eqs. (9) and (36) implies $Z_{\text{PBC}} = 2^3 3^2 \rho_p^{-2} \kappa^{-2} (\text{LWC})^2 l_\eta E_{\text{r3np}}^* (\kappa l_\eta)$. Thus, the significant influence of turbulent clustering is caused by sufficiently large values of the energy dissipation rate and the LWC.”

[Minor criticism 5]

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

Accordingly, we have moved the description in Appendix A to the end of the first paragraph of Subsection 4.2 in the main text.