Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-499-RC1, 2018

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Evaluate autoconversion and accretion enhancement factors in GCM warm-rain parameterizations using ground-based measurements at the Azores" *by* Peng Wu et al.

Anonymous Referee #1

Received and published: 17 July 2018

The goal of this study is to extend the results of studies such as Lebsock et al. (2013) and Boutle et al. (2014) on quantifying the effects of sub-grid scale inhomogeneity on microphysical process rates applied in GCMs from observations. The central tenet is that inhomogeneity varies with length scale and meteorological regime, thus the currently standard use of "universal" constants to characterize inhomogeneity cannot adequately describe subgrid-scale variability across a range of horizontal grid sizes or environmental conditions. The authors use a temporally extensive remote sensing dataset primarily sampling shallow convection over Graciosa Island in the Azores to develop "scale-aware" enhancement factors for the autoconversion and accretion processes (E_{auto} and E_{accr}, respectively) for several commonly used bulk microphysical

parameterizations. These enhancement factors are estimated from compositing of variances and covariances of instantaneous retrievals of cloud and rain liquid water path (CLWP and RLWP, respectively) and cloud drop number concentration N_c over varying time windows, which the authors argue are roughly equivalent to a GCM horizontal grid length if a constant wind speed is assumed.

I agree with the authors' basic premise that the use of constant values for E_{auto} and E_{accr} in GCM microphysics schemes is unrealistic and likely introduces precipitation biases similar (perhaps in magnitude if not sign) to assuming that grid-mean quantities (e.g. of N_c and cloud and rain liquid water mixing ratios q_c and q_r) are applicable to calculation of process rates in models with coarse grids (say horizontal grid length L greater than a kilometer or so). Furthermore, their assertion that enhancement factors should vary as a function of L as well as meteorological regime is well-stated, although they are not able to access independent information on aerosol-cloud interactions, which I suspect may be of comparable importance to the stability and LWP criteria analyzed.

Despite agreeing with the importance and timeliness of the premise of the manuscript, I have several major issues with the relevance of the observations to diagnosis of microphysical process inhomogeneity. Most importantly, the retrievals of cloud and rain/drizzle properties are not collocated; drizzle properties are only retrieved below cloud base. Cloud and drizzle properties are convolved within cloud such that what is classified as CLWP in fact includes contributions from in-cloud drizzle as well. Microphysical process rate equations assume coincident cloud and rain water mixing ratios (accretion) and coincident cloud water and drop number concentration (autoconversion), so unless it could be shown from some other dataset (LES? Aircraft observations? Maybe even a simplified 1D model?) that subcloud RLWP correlates highly with in-cloud RLWP and has similar magnitude, I have serious doubts about the physical relevance of the retrieved covariances. This may explain the apparently low ratios of cloud to rain water presented in the paper (see lines 33-34 and 291-293, Fig. 2e-f), although the authors give no "expected" value of this ratio for comparison.

The use of column-integrated liquid water paths introduces further uncertainty because the partitioning of the collision-coalescence process into autoconversion and accretion sub-processes is heterogeneous in the vertical. In the shallow clouds typical of the ENA site, autoconversion will be dominant near cloud top where cloud droplets have reached a maximum size due to condensation and larger drizzle drops are rare while accretion dominates lower in cloud, where the drizzle drops initially formed at cloud top sediment and continue to grow by collecting cloud droplets. Erasing this coherent vertical variability by the use of integrated water paths may bias the results presented: in stratiform clouds, liquid water is at a maximum near cloud top (i.e. CLWP is weighted toward cloud top), such that the E_{accr} values in particular are using over-inflated liquid water values. I'm also confused about how the authors transformed liquid water paths to mixing ratios. They state that "CLWC [cloud liquid water content] values are transformed to q_c...by dividing by air density" (lines 191-192) and similar for q_r (lines 194-195) but never define how they calculate CLWC or drizzle LWC. Are they dividing water path by cloud/drizzle shaft depth for an average value? Or are they applying the methods of Xie and Zhang (2015) and Wu et al. (2015) to the retrievals? Is the retrieval of N_c vertically resolved? This part of the methodology is insufficiently described to understand what the authors did, and regardless, it doesn't address the issue that drizzle properties can only be retrieved below cloud using their approach.

Finally, the authors made no attempt to quantify the uncertainty of the reported enhancement factors, such that I cannot make a determination as to whether their E_{auto} and E_{accr} are statistically distinct from the constant values introduced by Morrison and Gettelman (2008). This is particularly relevant to Figure 4. I would also have liked to see the authors show the quantitative impact of treating q_c and N_c individually with respect to calculating E_{auto}, as their derivation of Equation 4 assumes that the covariability of q_c and N_c can be ignored. While the magnitude of E_{auto} is comparable for q_c or N_c individually, I don't have a good sense for what including variability of both

variables implies for the predicted E_{auto} values. It's certainly a problem that CLWP and N_c are correlated in the ARM dataset employed, but that doesn't change the fact that variability of N_c is likely substantial, especially for the longer time periods analyzed or in more cumuliform precipitation.

In light of these concerns, I must recommend that this manuscript be **rejected** in its current form. A revised version of the manuscript only addressing autoconversion would be more feasible and would also be very useful to the parameterization development community, although as mentioned above, I would ask that the authors address the question of whether ignoring covariability of q_c and N_c is a reasonable assumption. I would be happy to review a revised and refocused manuscript.

Until remote sensing datasets can unambiguously partition in-cloud condensed water into cloud and drizzle components, analysis of cloud-rain covariance from the present spatially disjoint cloud and rain retrievals cannot be used to inform accretion parameterizations. A technique like that of Luke and Kollias (2013; doi:10.1175/JTECH-D-11-00195.1) that uses skewness of the Doppler spectrum to differentiate between cloud and drizzle could be combined with a method similar to Frisch et al. (1998; doi:10.1029/98JD01827) to retrieve vertically-resolved profiles of cloud and rain water, albeit likely only in stratiform clouds. If such an approach could be developed, the analysis performed in this manuscript would be more tractable although it would likely need to be validated before application to the GCM cloud inhomogeneity problem given the amount of technical work necessary to provide confidence in the retrievals.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-499, 2018.