Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-499-RC3, 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 #3

Received and published: 20 August 2018

ACPD Review of "Evaluate autoconversion and accretion enhancement factors in GCM warm-rain parameterizations using ground-based measurements at the Azores" Manuscript ID: ACP-2018-499 Authors: P. Wu, B. Xi, X. Dong, and Z. Zhang

This manuscript uses ground-based observations and retrievals from the DOE ARM Mobile Facility at Graciosa Island over the Azores to calculate and characterize shallow cloud autoconversion and accretion enhancement factors as a function of temporal and spatial size, which help establish the observational benchmarks with which to compare against autoconversion and accretion factors in GCM parameterizations. This is a worthy goal, as many GCMs precipitate both too frequently and too lightly relative

Printer-friendly version

Discussion paper

to observations, with the preliminary inference that the sub-grid enhancement factor for autoconversion in many parameterizations is too strong, and the factor for accretion is too weak. This is because autoconversion primarily determines precipitation initiation, and accretion primarily controls drizzle/precipitation intensity. The approach taken in this study both interesting and instructive, as the homogeneity of cloud/liquid water path (LWP) properties tends to be higher for smaller grid sizes, suggesting that GCMs with finer resolutions many have an even weaker accretion enhancement factor than coarser models. This appears to be an important consideration for models with a range of grid resolutions. While autoconversion and accretion biases are systematically characterized in this study, the additional novelty of this work is that the biases are not fixed, but rather regime dependent, with both lower tropospheric stability and precipitation playing a significant role. The layout and results of this study have the potential to inform existing and future parameterizations about how to tailor precipitation enhancement factors as a function of local thermodynamics, resolution, temporal length, and precipitation itself. While other regions will need to be studied as well to gather and calculate additional autoconversion and accretion enhancement factors in other regimes and/or large-scale dynamics, this study is a good start in providing potential guidance for the modeling and model analysis communities. While there is much potential in this manuscript, there are also a number of minor to moderate technical and science questions which need addressing prior to consideration for publication, and these are mentioned below. Though the manuscript was not intended to be an exhaustive study of all the factors that that may modulate the autoconversion and accretion enhancement factors, it might be complementary to the study to also consider a few additional large-scale factors, such as local vertical velocity profiles, as has recently been done in work examining entrainment velocities over the MAGIC campaign over the Northeast Pacific, in which even during the boreal summer, approximately 20% of the profiles were observed to have rising motion near cloud top. Whether rising motion would enhance accretion rates/enhancement factors might complement the findings presented in this study. Finally, at the end of this review is an enumeration of grammat-

Printer-friendly version

Discussion paper

ical suggestions/typos; the list is non-exhaustive such that a thorough proofreading will be essential prior to publication.

Science/Technical Questions and Comments: 1) On lines 33-34 (and also lines 291-293), the authors state that "the ratios of rain to cloud liquid water at Eaccr=1.07 and Eaccr=2.0 are 0.048 and 0.119, respectively, further proving that the prescribed value of Eaccr=1.07 used in GCMs is too small to simulate precipitation intensity", but it is somewhat unclear to me how this proves this, unless the authors include (for clarity) the respective GCM RLWP-to-CLWP ratios as well in this statement. How does this range of ratios compare to those from Lebsock et al. (2011)? Based on their Figure 6, it seems that the RLWP-to-CLWP ratio is generally higher than 0.119, though of course other factors are work as well (e.g. cloud top effective radius, included in their Figure 8). If I am interpreting these differences correctly, what do the authors attribute to the apparently higher ratios in that observational study? Reference: Lebsock, M. D., T. S. L'Ecuyer, and G. L. Stephens, 2011: Detecting the ratio of rain and cloud water in low-latitude shallow marine clouds. J. App. Meteor. Climatology, 50, 419-432, doi: 10.1175/2010JAMC2494.1. 2) Lines 77-78 (and elsewhere): The authors use primes (') to denote grid means, and are consistent about this, but wouldn't an overbar typically denote grid mean values? Often, but not always, primes are designated for deviations from the mean. Overall, this is a fairly minor comment. 3) Lines 144-145, and more generally the implications for the findings of this study: Is the shape parameter, γ, somewhat analogous to the LWP homogeneity from Wood and Hartmann 2006 (which was simply the squared quantify of the ratio of the mean LWP to its standard deviation)? In that study, precipitation was not explicitly examined, but instead the brokenness of cloud fields was found to increase (total CF decrease) with decreasing LWP homogeneity. The greater shallow convective cellular structure with decreasing LWP homogeneity, however, may have been more conducive for heavier precipitation.

In this study, the relationship appears to be the link between reduced LWP homogeneity or other cloud field homogeneity and an increase in precipitation intensity. The latter is often too small in climate models, and too homogeneous of fields may be the culprit. Perhaps even though the authors have cited Barker et al. (1996); Pincus et al. (1999), and Wood and Hartmann (2006), an even stronger parallel/analogy should be made between the objectives and findings here and those from those studies, particularly in the discussion section later on as appropriate. 3) Results in Figure 1: Traditionally, we think of the large number concentrations decreasing with precipitation, as heavier precipitation is dominated by fewer, larger drops. It perhaps seems a little surprising that a peak (red bin denoting Nc) emerges during the heavy drizzle phase of Nc values of 100 cmˆ-3 (Figure 1d). However, there are fewer very high values of Nc (e.g. >150 cmˆ-3) during the hours of moderate to heavy drizzle. From Fig. 1b as well, there does seem to be high values of Nc just before or after periods of heavier drizzle, but a suppression of Nc is observed somewhat during the stronger pulses of precipitation. Can the authors discuss perhaps why more of the larger number concentrations are not removed during the heavier drizzle events? On the other side of the spectrum, during most of the non-precipitating phase, there is a local maximum of Nc at 150 cmˆ-3. Have the authors considered looking at the corresponding time series of effective radius to include as perhaps another panel for Figure 1? This may complement the Nc observations quite nicely.

4) Lines 196-197: Is this the traditional LTS definition of potential temperature at 700 hPa minus potential temperature at near the surface (or 1000 hPa)?

5) Figure 2: Generally, the precipitation frequency ranges from about $\sim$0.1 to just above 0.4, which seems a little low (at least the highest end of precipitation incidence) compared to other observational studies (e.g. Kubar et al. 2009). What is the definition of precipitation frequency here – is it any precipitation observed in the column, or only that which reaches the surface? Reference: Kubar, T. L., D. L. Hartmann, and R. Wood, 2009: Understanding the importance of microphysics and macrophysics in marine low clouds, Part I: satellite observations. J. Atmos. Sci., 66, 2953-2972, doi: 10.1175/2009JAS3071.1 6) Is the E_auto critical threshold of 4 of converting cloud to

drizzle drops found by the authors from the results in Figure 2 considered a novel finding, or has this been reported elsewhere as well (or perhaps a slightly different threshold)? 7) Lines 284-285 and Figure 4: The authors cite and use 1.07 as a representative value for E_accr (based on Morrison and Gettleman 2008); is this a fairly common value used in other GCM parameterizations as well? Presumably, based on this study, the range for E_accr in climate models is smaller than the observed/calculated 1-4 range for E_accr found in this study. 8) Lines 354-354 and implications of study: As alluded to in the Introductory remarks of this review above, an interesting finding of this study is that the enhancement factors are even more different/biased in finer-resolution GCMs versus observations than coarser-resolution models. Thus, even though a frequent goal is improving resolution in simulations, more care is needed to address the "too frequent/too light" precipitation problem. This study appears to be instructive in how to potentially overcome this barrier. 9) Also, as alluded to in the introduction, while the autoconversion/accretion enhancement factor dependence on both scale and LTS regime is very intriguing, the authors may also want to expand (or propose for future work) the dependence on either near cloud-top vertical velocity (e.g. from reanalysis data such as ECMWF) and/or boundary layer vertical velocity. My assumption might be that the behavior may be similar to stability; for upward motion near cloud top, both autoconversion/accretion factors may be higher (as they are for reduced LTS), but it would be interesting to know how comparable such an effect may be to LTS. In a somewhat similar vein, I do commend the authors for discussing that other variables (e.g. aerosol type and concentration) may be important as well for the two enhancement factors studied in this investigation in the very last paragraph. This at least sets up where the authors or others can proceed to continue to expand this line of research. 10) Lines 398-399: The authors list a number of studies from the ~mid-2000s which discuss existing parameterizations, but are the latest GCMs quite similar as well? Is there a good recent paper or series of papers which discuss recent parameterization updates, if they exist?

Minor Notes and Grammatical Suggestions/Typos: (*Note – this is a thorough, albeit still incomplete list of typos. Please professionally edit this manuscript prior to resubmitting.) 1) Line 28: change “increase” to “increases” 2) Line 50: change “drizzle are” to “drizzle is” 3) Line 62: change “Example” to “example” 4) Line 65: change “process that drizzle drops” to “process of drizzle drops” 5) Line 212: change “19-month” to “19 months” 6) Line 221: add an “a” before “more homogeneous” 7) Line 228: add an “a” prior to “similar” 8) Line 231: change “contribute” to “contributes” for proper subject (“combination”)-verb (“contributes”) agreement 9) Line 240: add “the” before “cloud” 10) Line 246: change “5-hour” to “5-hours”; similarly, for line 251: change “2-hour” to “2-hours” 11) Line 259: add “the” prior to “autoconversion” 12) Line 273: add “a” before “similar” 13) Line 317: Consider changing “seem easier to produce drizzle” to “more easily produce drizzle” 14) Line 372: change “are representing” to “represents”. Also, in general the sentence from Line 371 – 373 is slightly awkward and probably should be rewritten. 15) Line 378: change “associate” to “associated” 16) Line 385: add “a” before “variety” 17) Line 659: add “are for” before “2-hr”

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

Printer-friendly version

Discussion paper