General response to both reviewers

We would like to thank you for the constructive comments and suggestions. We appreciate your time. As you will see, the manuscript has been revised follow each reviewer's suggestions. In the response, black are reviewers' comments and blue are our responses.

The major changes are:

1. Prompted by both reviewers, the cloud and rain water mixing ratios are now collocated, and the method is described in Appendix A in the revised manuscript. We combine remote sensing and adiabatic assumption to jointly estimate cloud and rain liquid water content (CLWC and RLWC) within the cloud layer. We also estimate the uncertainties in enhancement factor calculations came from our retrieval uncertainties and the results are shown in updated Figure 4.
2. Thanks for the suggestion by Reviewer 1, E_{auto} and E_{accr} in the revised manuscript are calculated at different layers of cloud to reveal the physical processes. We use averaged q_c in top five range gates to calculate E_{auto} and averaged q_c and q_r in five range gates around maximum reflectivity to calculate E_{accr}. Despite substantial changes in the data used in calculations, the trend of the new results is similar to previous one except that the values slightly increase. Thus, most of our conclusions still hold.
3. Instead of roughly assuming 10 m s^{-1} horizontal wind, we now use the mean wind speed within cloud layer from ARM merged sounding data. The terminology is changed from '2-hour…5-hour time intervals' to '60-km and 180-km model grids' as we mimic the specific model grid sizes instead of specific time intervals.
4. Mentioned by Reviewer 2, we did extensive literature reviews and rephrased sentences in both introduction and discussion sessions. Previous studies are properly cited and acknowledged.

Specific responses to Review 2

This paper discusses how variability of cloud and rain at the GCM sub-grid scale affect the parametrizations of autoconversion and accretion that are typically used. This has become a popular topic in recent years with many papers and modelling centres using this as a method of improving warm rain simulation. The current paper has some novel aspects, for example the use of data from the Azores to evaluate parametrizations, but I feel would require some significant modifications before it is acceptable for publication.

We made the point-to-point response and thank for your suggestions that help us a lot improve the manuscript. We appreciate the references that you provided and add them in the revision.

Major comments:

1. I don't feel this paper fully or correctly acknowledges the previous work that has been done in this field, which leads to many statements with are either misleading, incorrect, in contradiction to previous studies without explanation, or presented as new when actually they have been published before. Specific examples of this are:

Thanks for your comments and suggestions on literature review. We have revised the sentences and properly acknowledged previous studies.

a) L31, 284, 390 and elsewhere - repeatedly the authors refer to "GCMs", implying that they are stating a common feature of many models, whereas in actual fact they are referring specifically to the MG08 microphysics scheme which is only used in a very small number of GCMs. This terminology needs to be more precise, to highlight the fact that not all GCMs make the same assumptions as MG08.

Thank you for the comment.
The terminology has changed in the revision, we mainly used MG08 scheme in the calculation and discussion. We also give the values for 60-km and 150-km grid sizes for other parameterizations listed in Table 1. Same approaches can be repeated for other parameterization schemes used in GCMs.

To avoid confusion, we add the following sentences at the end of the introduction: "Most of the calculations and analyses in this study is based on Morrison and Gettleman (2008, MG08 hereafter) scheme. The enhancement factors in several other schemes are also discussed and compared with the observational results and the approach in this study can be repeated for other microphysics schemes in GCMs." in lines 117-121 in the revised manuscript.

b) L99 - this statement is incorrect - whilst some models do use prescribed values regardless of meteorological conditions, the whole point of Boutle et al (2014), which is cited as introduction to this statement, is to provide a parametrization depending on meteorological conditions which can be used in GCMs. This parametrization is improved upon by Hill et al (2015), who add in a regime dependence to the parametrization, and implemented in a model by Walters et al (2017). The authors need to acknowledge this work in the context of their own.

Thanks for the correction.
This part has been rephrased to "Boutle et al. (2014) used aircraft in situ measurements and remote sensing techniques to develop a parameterization for cloud and rain, in which not only consider the sub-grid variabilities under different grid scales, but also consider the variation of cloud and rain fractions. The parameterization was found to reduce precipitation estimation bias significantly. Hill et al. (2015) modified this parameterization and developed a regime and cloud type dependent sub-grid parameterization, which was implemented to the Met Office Unified Model by Walters et al. (2017) and found that the radiation bias is reduced using the modified parameterization." In lines 99-106 in the revised manuscript.

c) L293-294 - this statement is just repeating the previous conclusions of Boutle et al (2014) and Lebsock et al (2013).
Thank you for the comment.
The two studies are cited and acknowledged in the context of our results.

d) L335 - Hill et al (2015) also show regime dependence and should be cited here.
Thanks for the comment.
The sentence is rephrased to "Therefore, as suggested by Hill et al. (2015), the selection of E_{auto} and E_{accr} values in GCMs should be regime-dependent." in lines 364-365 in the revised manuscript.

e) L336-337 - I don't understand this statement - why is it difficult to vary enhancement factors in GCMs? Walters et al (2017) using the parametrizations of Boutle et al (2014) does exactly that - there is nothing difficult here and no reason why other GCMs could not do similar.

Thanks for the comment.
We deleted this sentence and rephrased this part to "To properly parameterize sub-grid variabilities, the approaches by Hill et al. (2015) and Walters et al. (2017) can be adopted. To use MG08 and other parameterizations in GCMs as listed in Table 1, proper adjustments can be made according to the model grid size, boundary layer conditions, and precipitating status." in lines 366-369 in the revised manuscript.

f) L364-368 - I don't fully understand what is being claimed here, and it certainly is not supported by any evidence presented in the paper. But what I think the authors are saying is that in more cumulus-type (less stratiform) clouds, E_auto should be smaller. This appears contradictory to the results of Boutle et al (2014) (their Fig 10) and Hill et al (2015) which show that E_auto is higher in convective type cloud regimes. It also appears in contradiction to the authors own statement on L429-430 (a statement that appears with no justification or background), that unstable boundary layers give rise to larger E_auto values. Please clarify this.

Thanks for the comment.
For this statement, we are trying to say for the 'cloud type under this study' e.g., MBL stratocumulus in this study, the E_{auto} values should be smaller over land than that over ocean. To avoid confusion, we deleted this statement.

The statement in L429-430 of original manuscript "The E_{auto} values in both stable and mid-stable boundary layer conditions are smaller than the prescribed value of 3.2 used in GCMs, while those values in unstable boundary layers conditions are significantly larger than 3.2 regardless of whether or not the cloud is precipitating." is the conclusion we draw from the values in Table 2 where the boundary layer is classified into three categories using lower tropospheric stability (LTS). For clarification, we added '(Table 2)' at the end of this statement. This statement is now in lines 455-458 in the revised manuscript.

g) L433 - as is done in Hill et al (2015) and Walters et al (2017).

Thanks for this comment.
The sentence is rephrased to "Therefore, the selection of E_{auto} and E_{accr} values in GCMs should be regime-dependent, which also has been suggested by Hill et al. (2015) and Walters et al. (2017)" in lines 459-460 the revised manuscript.

h) Fig 4 - despite the constant criticism of MG08 for using a fixed value of E_auto=3.2, this figure shows that at larger grid sizes, this value is actually incredibly good – some credit should be given to MG08 for this!

Thank you for the comment.

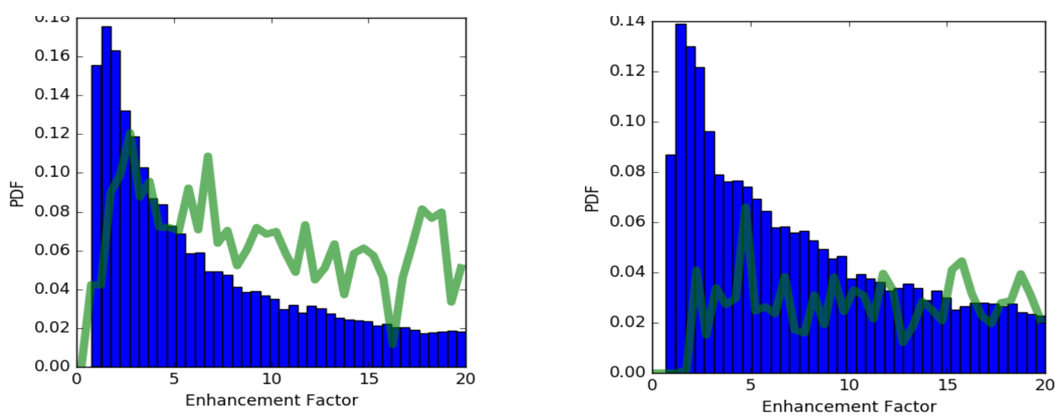
Yes, E_{auto} prescribed in MG08 is getting more and more close to those calculated from observations. In the revised Figure 4, the mean value from observation is exactly the same for 180 km model grid.

We rephrased the following "After that, the E_{auto} values remain relatively constant of ~3.18 when the model grid is 180 km, which is close to the prescribed value of 3.2 used in MG08. This result indicates that the prescribed value in MG08 represents well in large grid sizes in GCMs" in lines 373-376 in the revised manuscript.

2) L148, L151 - equations 4 and 5 are incorrect, the term in the denominator should be Gamma(nu) not Gamma(a) as written (see Eq 7 of Boutle et al (2014) or Eq 6 of Pincus and Klein (2000)). I hope this is only a typo and not a problem with all of the data analysis! Also, I'm confused about whether or not you are investigating variability of Nc - the text seems to suggest you are, but this equation ignores any variability in Nc - please clarify the text and correct the equation if necessary.

Thanks for the corrections. These are typos and have been corrected. All the calculations and analysis in the original and revised manuscripts used the correct formulas.

We indeed include N_c in the calculation and the equation has been changed in the revision. We only assess the individual effect of N_c to E_{auto}, not for the covariance of q_c and N_c because q_c and N_c are highly correlated in the retrieval method and it is difficult to tell if the results are due to natural variability or due to mathematics in the retrieval. We can use the following two figures to show why these results are artificially high: the E_{auto} calculated from the covariance of N_c and q_c for 60 km (left panel) and 180 km grid (right panel) size superimposed by average precipitation frequency in each bin can reach 40-50. Therefore, we only assess the individual effect of N_c as shown in Figures 2c and 2d, which are similar to the effect of q_c as shown in Figures 2a and 2b. For simplicity and clarity, only E_{auto} calculated from q_c are included in the discussions afterword.



For clarification, we added the following to lines 305-308 in the revised manuscript "Because the E_{accr} values calculated from q_c and N_c are close to each other, we will focus on analyzing the results from q_c only for simplicity and clarity. The effect of q_c and N_c covariance, as stated in Section 4.1, is not presented in this study due to the intrinsic correlation in the retrieval (Dong et al., 2014a and 2014b and Appendix A of this study).".

3) L207, 340 - simply using a constant wind speed is quite crude - most previous studies with ground based equipment (eg. Boutle et al 2014) have either used actual wind speeds or model derived reanalysis wind speeds to construct spatial scales from time averages. At the very least this simplification needs to be noted and possible errors due to this discussed.

Thank you for the comment and suggestion.
We agree that use actual wind speeds or model derived wind speeds can reduce the sampling uncertainty. In the revised manuscript, we use the averaged wind speed within cloud layer to mimic different model grid sizes.

The following paragraph is added to lines 229-236 in the methodology part: "To evaluate the dependence of autoconversion and accretion rates on sub-grid variabilities for different model spatial resolutions, an averaged wind speed within cloud layer was extracted from merged sounding and used in sampling observations over certain periods to mimic different grid sizes. For example, two hours of observations corresponds to a 72-km grid box if mean in-cloud wind speed is 10 $m\ s^{-1}$ horizontal wind and if the wind speed is 5 $m\ s^{-1}$, four hours of observations is needed to mimic the same grid. We used six grid sizes (30-, 60-, 90-, 120-, 150-, and 180-km) and mainly show the results from 60-km and 180-km grids in Section 4."

4) L220 onwards, L281, elsewhere - the analysis appears to be presented in terms of LWP and RWP, i.e. column integrals of quantities. This is very different to the LWC and RWC, i.e. grid-box mean quantities which are used in parametrizations. Most previous studies have used LWC and RWC to calculate the variability, and so the results are directly applicable to parametrizations. It's not clear to me that results presented in LWP and RWP are so directly applicable. The authors need to investigate how applicable their results using column-integral quantities are to previous studies and parametrizations - it appears from the text that you do have direct observations of LWC and RWC, so it should not be too difficult to make this comparison, or re-do the analysis using the LWC and RWC data.

Thanks for the comment.
We agree that the use of CLWP and RLWP ignores the heterogeneity of collision-coalescence process in the cloud layer. In the revised manuscript, q_c and q_r are jointly retrieved and applied to the calculation.

We add the following sentences to methodology part lines 211-220 in the revised manuscript: "The autoconversion and accretion parameterizations partitioned from collision-coalescence process dominate at different levels in a cloud layer. Autoconversion dominates around cloud top where cloud droplets reach maximum by condensation and accretion is dominant at middle and lower parts of the cloud where drizzle drops sediment and continue to grow by collecting cloud droplets. Complying with the physical processes, we estimate autoconversion and accretion rates at different levels of a cloud layer in this study. The averaged q_c within the top five range gates (~215 m thick) are used to calculate E_{auto}. To calculate E_{accr}, we use averaged q_c and q_r within five range gates around the maximum radar reflectivity. If the maximum radar reflectivity appears at the cloud base, then five range gates above the cloud base are used."

General comments:

Title - should probably be "Evaluation of ..."
Thanks. Title has been changed.

L50 - should say "a significant amount of drizzle is evaporated"
Thanks. This has been changed.

L56 - I'm not entirely sure I agree with this statement - change in albedo (i.e. the first indirect effect) is the most significant indirect effect. There is also an extensive literature on buffering of the 2nd indirect effect and mechanisms through which aerosol could even enhance convective precipitation. At the very least this statement needs to be more accurate in the context it is being used - increases in aerosol are mainly thought to suppress precipitation in MBL clouds.
Thanks for the comment and sorry for the confusion.
This sentence means that the aerosol indirect effect associated with MBL cloud constitutes the major part in global aerosol indirect effect.
To avoid confusion, this sentence is deleted in the revised manuscript.

L62 - MG08 is an odd reference here, given it discusses a microphysics parametrization, something which is required in models of all scales
Thank you for the comment. This reference is deleted in the revised manuscript.

L63 - the "process" of autoconversion and accretion only exist because modellers have partitioned the liquid water into "cloud" and "rain" categories - please rephrase this sentence, they are not real processes, all that happens in the real atmosphere is collisioncoalescence of water droplets.
Thanks for the comment.
This sentence is rephrased to "For Example, warm rain parameterizations in most GCMs treat the condensed water as either cloud or rain in the process of collision-coalescence, which is partitioned into autoconversion and accretion sub-processes in model parameterizations"

L64, 72, 73, 122, 129 - the references to MG08 and LG13 are odd here, given they do not propose autoconversion or accretion parametrizations of their own, they use the scheme of KK00 which is already referenced.
Thank you for the comment. The odd references have been deleted.

L77 - using a prime to denote grid-mean quantities is somewhat non-standard – an overbar is the more typical symbol for a mean quantity.
Thank you for the suggestion. The symbols have been changed in the equations and text.

L79 - I'm not sure I follow why positive skewness is important - can you elaborate? It is only really the non-linear form of the equations that mean rates depend strongly on the sub-grid variability.

Thanks for the comment.
The skewness determines the degree of error by using mean value to represent entire domain. If LWP is normally distributed then mean value is equal to mode value, meaning that mean value can represent the value that most frequently occurs in the field. Whereas in skewed distributions,

e.g., Gamma distribution where mean value is greater than mode, then the mean value only represents a relatively small portion of the samples. And using mean to represent entire field results in larger errors than that in normal distribution.

For clarification, we rephrased this sentence to "MBL cloud liquid water path (CLWP) distributions are often positive skewed (Wood and Hartmann, 2006; Dong et al. 2014a and 2014b), that is, the mean value is greater than mode value. Thus, the mean value only represents a relatively small portion of samples. Also, due to the nonlinear nature of the relationships, the two processes depend significantly on the sub-grid variability and co-variability of cloud and precipitation microphysical properties" in lines 77-81 in the revised manuscript

L100 - Boutle et al (2014) use a combination of aircraft, ground-based and satellite measurements.
Thanks for the comment. The citation to this reference has been changed to "Boutle et al. (2014) used aircraft in situ measurements and remote sensing techniques to develop a parameterization for cloud and rain, in which not only consider the sub-grid variabilities under different grid scales, but also consider the variation of cloud and rain fractions. The parameterization was found to reduce precipitation estimation bias significantly." In lines 99-103 in the revised manuscript.

L312 - using flash flooding as an example when discussing drizzling marine stratocumulus is a bit of a leap, I suggest removing this statement unless you have any evidence that extreme rainfall rates are affected.
Thank you for the comment.
The flash flooding is not a suitable example in the context of marine stratocumulus. We have rephrased this sentence to: "providing limited information in estimating rain water evaporation and air-sea energy exchange" in lines 342-343 in the revised manuscript.

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Thank you for providing the references, we appreciate your time.

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