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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 #2

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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.

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:

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.

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.

c) L293-294 - this statement is just repeating the previous conclusions of Boutle et al (2014) and Lebsock et al (2013).

d) L335 - Hill et al (2015) also show regime dependence and should be cited here.

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.

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.

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

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!

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.

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.

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.

General comments:

Title - should probably be "Evaluation of ..."

L50 - should say "a significant amount of drizzle is evaporated"

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.

L62 - MG08 is an odd reference here, given it discusses a microphysics parametrization, something which is required in models of all scales

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 collision-coalescence of water droplets.

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.

L77 - using a prime to denote grid-mean quantities is somewhat non-standard - an overbar is the more typical symbol for a mean quantity.

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.

L100 - Boutle et al (2014) use a combination of aircraft, ground-based and satellite measurements.

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.

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