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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Revised identification of tropical oceanic cumulus congestus as viewed by CloudSat" *by* S. P. F. Casey et al.

S. P. F. Casey et al.

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Before addressing the referee comments in the order in which they were received, the authors wish to highlight one comment from S. Tanelli, which yielded a major change in the manuscript:

it would be good to specify which data release (I suppose R04) was used for this study, in fact the cloud mask algorithm has been revised in the most recent version with a reduction of false detections (but also an increase in missed detections).

The original submission of this manuscript used 2B-GEOPROF-LIDAR data version P1-R04. In October 2010, version P2-R04 was released by the CloudSat science team, after differences in CALIPSO cloud detections were reported by the CALIPSO science

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team. At the time, the authors assumed that, given the “reduction of false detections” in the new CALIPSO data product, it would not affect the results of a study looking at convective clouds. As is shown in the new Appendix A and Table A.1, this assumption was incorrect. As a result, many quantitative and qualitative results have been changed. That being said, the overall message of the paper remains the same; that is, our results still show problems arise with using quantitative reflectivity thresholds to identify congestus clouds.

Anonymous Referee 1

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The revised manuscript has made the needed clarifications and additions. This is a very useful contribution that provides insight into some subtleties of the vertical structure of congestus clouds and the strengths and limitations of the CloudSat/CALIPSO CLDCLASS classification scheme. It will be of interest to future users of the data and provides some valuable perspective on previous published claims that many of the observed congestus are actually deep convective clouds that have not finished their ascent. I recommend that the manuscript be accepted in its current form.

The authors would like to thank the reviewer for this positive comment, and invite the reviewer to read the updated version as well as it contains improvements.

Interactive comment on “Revised identification of tropical oceanic cumulus congestus as viewed by CloudSat” by S. P. F. Casey et al.

S. Tanelli (Referee)

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This paper revisits some of the criteria recently adopted to identify and analyze congests clouds by means of A-Train measurements (primarily CloudSat and CALIPSO, *but GCM reanalysis and MODIS measurements also play a role). The overall conclusion should be of interest to the community however there are 3 areas that in my opinion require further clarification to make this paper suitable for publication.*

I) Scope: this paper mainly revisits the criteria adopted in Luo et al. 2008 and 2009 to identify congestus clouds and estimate their level of maturity. Those methods were explicitly targeting vigorous convection (in fact the main point of those papers was to define a method to assess the occurrence of overshooting convection), and separate vigorous but terminal congestus from transient congestus on its way to become full fledged deep convection. In one of the two papers, the main author indicates that only clouds classified as deep convection by the CLDCLASS product, and in any case containing precipitating cores, were targeted. Some portions of the main text of this paper seem a little overly critical of such methods in that they may lead a reader to see them as methods targeting the overall detection of all congestus clouds, but failing at that goal. A few changes crediting the cited papers for what was their scope, and simply stating and documenting how those methods perform in the more general congestus detection should be included in all fairness. For example the sentence in the abstract "This implies that previous methods used to identify congestus clouds may be biased towards more vigorous convection, . . ." should be changed. Those methods WERE designed to identify vigorous congestus. Similar considerations apply to many other parts of the paper. The merit of this work is not diminished by doing this. In fact, the choice of words adopted by the authors in the conclusions is exactly in line with this comment.

The abstract has been rewritten to remove the phrase in question, and to emphasize that the criteria used may still be effective in identifying transient congestus. It was not the authors' intent to be overly critical of the Luo et al. [2009] paper, simply to highlight that many congestus clouds do not satisfy the criteria used in this study. It should also be noted that in using the latest 2B-GEOPROF-LIDAR version, the authors found a higher percentage of congestus clouds satisfied the three criteria, provided the

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cloud-top was viewed by CALIPSO. As the Luo et al. [2009] uses MODIS brightness temperatures in its method to determine terminal vs. transient congestus, a cloud that isn't viewed by CALIPSO would not have an associated brightness temperature anyway, as only the top-most cloud layer would be viewed by MODIS.

In doing this, I think a more explicit review of "what is a congestus cloud" with respect to the various methods adopted to identify it in the cited references would be a nice (optional) addition to this paper.

A few sentences have been added in the introduction to compare how Johnson et al. (1999), Rossow et al. (2005) and Jensen and Del Genio (2006) defined congestus clouds.

II) Explanation of methodology: when I first read section 2 I thought I had understood the methodology. However a few sentences and the results shown in Table 1 forced me to doubt my understanding, because, had it been correct, they would not make sense to me. I will illustrate my understanding and the sentences that threw all that overboard. A few possible gaps that could explain will be cited also.

When the three mask level (20, 30 and 40) CTH are defined, my understanding was that only profiles that achieve mask level of 40 somewhere were used. As such, the CTH-XX is the highest range bin where cloud mask has a value of XX or higher. As consequence CTH-40 <= CTH-30 <= CTH-20 and usually CTH-20 <= CTH LIDAR. The GEOPROF-LIDAR product includes multi layer information, but no specific explanation appears in regards to the use of multi-layer information, hence one is prone to think of CTH as the CTH of the highest layer in the profile.

This issue is now clarified in Section 2: "If a field-of-view has a cloud mask of 20 and 30, but not 40, this convective area will be included in the cloud mask 20 30 statistics, but not in the cloud mask 40 statistics. Because of this, more convective fields-of-view will be considered for cloud mask 20 statistics than cloud mask 40."

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The first sentence that made me doubt is in section 3.1: "As expected, the mean CTH difference between CALIPSO and radar cloud mask 20 CTH is higher than between CALIPSO and radar cloud mask 40 CTH." My expectation being quite the opposite (based on the above understanding) I thought at first this was just an editorial mistake. However, Table 1 (referred to in this sentence), corroborates exactly this interpretation. I must therefore question either my understanding of the methodology, or the correctness of the procedure. The authors should clarify or correct.

This was an error in Table 1 and the text, with two causes. One cause was the use of P1-R04 instead of the latest version. A number of fields-of-view were reported as one cloud in P1-R04 but as two or more clouds in P2-R04, substantially decreasing the mean lidar-to-radar CTH. This included cells where a Cloud Mask = 20 was present, but not Cloud Mask = 40, so large lidar-radar CTH differences (on the order of 10 km) were included in calculating Cloud Mask = 20 statistics, but not Cloud Mask = 40. The other main cause is related to the "second obstacle" below, and will be addressed there. "As expected" makes more sense with the latest version of 2B-GEOPROF-LIDAR, as the lidar-20 difference is now much less than the lidar-40 mean difference.

Related to this. . . Table 1 lists CTH and ETH differences for congestus (CTH of 3 to 9 km and radar cloud base below 1km), What if a profile had a CTH-40 of 8.5 km and a CTH-20 of 9.5 km? Would this profile only be counted in the CTH-20 portion of the table? In general, it would be preferable if the criteria adopted to define all these statistic sample populations were laid out more clearly. In general, if a profile only achieved mask level of say 30, and not 40. Was it still included in the statistics?

This has been clarified, similar to what was mentioned above. In the case discussed in this comment, the profile would only be counted in the CTH-40 portion of the table, as the CTH-20 height of 9.5 km would be above the area of interest.

Second obstacle...: Column 4 also lists the percentage of cases with the appropriate cloud mask/classification where CALIPSO identifies a higher cloud top than CloudSat.

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This occurs for about 75 [percent] of cumulus cases and 90 [percent] of deep convective cases. The CALIPSO lidar beam is extinguished at an opti- cal depth of about 3, so the cases where a CALIPSO CTH is not identified concurrent with a CloudSat CTH may be due to the CALIPSO beam being extinguished by a non-connected cloud higher in the atmosphere. The averaging of smaller CALIPSO pixels onto the CloudSat footprint may also contribute to pixels where CALIPSO reports a CTH below that observed by CloudSat.

First: how much of the 'missing' 25[percent] or 10[percent] has CTH lidar 'equal' to CTH radar? I suppose very little, but the way the sentence is phrased leaves room for this doubt.

To answer the question above, very few profiles reported a LayerTop flag of 3 (viewed at that height by both lidar and radar) in 2B-GEOPROF-LIDAR. A perusal of the data showed that even if the CALIPSO and CTH-20 cloud heights are less than 240 m apart (i.e., less than the vertical resolution of CloudSat), the LayerTop flag is set to 2 (Lidar-only) instead of 3. That being said, the rewritten manuscript no longer includes much of the quoted section above (see below).

Second: If the CTH LIDAR is truly the CTH of the highest layer, the lidar extinction explanation does not make sense. In order to accept this interpretation I would have to assume that the authors proceeded by identifying among many layers the one specifically classified as cumulus, or deep convection, and extract the lidar CTH of that layer. If this is so, it should be explained in section 2. And also, since the dataset is there at hand, instead of speculating, it would be nicer if this hypothesis were verified (i.e., how much of that 25[percent] or 10[percent] of cases where CTH LIDAR < CTH RADAR is captured by profiles that have one or more layers above the cumulus or deep convection?). The 'non-uniform' filling explanation is more plausible. Did the authors verify in the GEOPROF-LIDAR ICD or ATBD, or by inquiring with their authors, that this could indeed be an explanation?

In updating the study to include the latest version of 2B-GEOPROF-LIDAR, the lead author discovered a major coding error in the flags he assigned to the data. As such, fields-of-view where CALIPSO reported a lower CTH than CloudSat were given the same flag as fields-of-view where CALIPSO reported no cloud at all (such as times when the CALIPSO beam was not operating). When separated for the latest version (and the recalculated P1-R04 results in Appendix A and Table A.1), the number of cases where CALIPSO CTH was lower than CloudSat dropped to <0.001

There are other instances where my understanding of the methodology clashed with the results and their tentative interpretations, but I believe that the two examples above suffice to let the authors understand my doubts and respond.

III) Conclusion: I understand that the "3 criteria" (and by the way, the criteria were 4 in Luo 2009, did the authors include the 'continuity of echo from CTH to near the ground"?) succeed in classifying as congestus only less than half of the features classified as cumulus or deep-convection by CLDCLASS. But was it verified that CLDCLASS is error free? Did the authors verify that features that failed were indeed congestus?

With regards to the fourth criteria, rather than using the more qualitative "near the ground" of Luo et al. 2009, the authors chose a more quantitative "less than 1 km above the surface" as defined in Section 2. The authors are also unaware of major issues with regards to the 2B-CLDCLASS algorithm that would result in the misclassification of Deep Convective or Cumulus clouds using the Sassen and Wong (2008) method, though no independent investigation of these classifications were addressed by the authors.

Minor comments:

page - line: remark type - text

2 - 6: minor comment - 'twice daily' could suggest a 12 hr global coverage, or that in any case they visit the same cloud system twice daily. This is true only for very few

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and selected spots of each daily orbit pattern. I would suggest to either remove the twice-daily or elaborate just a little further.

"Twice-daily" has been removed from the introduction.

2 - 33: -28 dBZ is the nominal sensitivity at beginning of life, the verified sensitivity at beginning of life was of -30 dBZ. The effective horizontal resolution is 1.4 x 1.7 km. See cited Tanelli et al. reference for details.

These values were clarified in section 2, and the authors thank Mr. Tanelli for pointing this out.

3 - 17 9 - 31: Tanelli et al. is 2008 not 2009.

The reference has been corrected.

4 - 1 to 7: recommended expansion: it would be good to specify which data release (I suppose R04) was used for this study, in fact the cloud mask algorithm has been revised in the most recent version with a reduction of false detections (but also an increase in missed detections). Furthermore, what do the authors mean exactly by 'cumulus-20', 'cumulus-30' etc? Is that the CTH calculated as the highest bin higher than that threshold in the cloud mask and on a profile classified as 'cumulus' by CLD-CLASS? Why did the authors choose to investigate the 20-30-40 cloud mask thresholds? What is their specific meaning in regards to the CLDCLASS and CTH estimations? Could the authors verify if some of the misclassifications were due to a second cloud layer barely detected in the CTH calculation but not used in CLDCLASS algorithm? Also, a sub-1km artifact seems to be present in all classes. Is that due to ground clutter? The presence of that feature in Fig 1 is too evident to be ignored. In general it would be beneficial if this performance assessment were discussed more in depth, at this stage, perhaps with some visual examples. Otherwise the meaning of all the following discussion in terms of statistics becomes muddled, and any insight less clear.

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Cumulus-20 refers to the highest vertical bin with a cloud mask value of 20 or higher for a cloud identified as cumulus, etc. The authors chose to investigate the 20-30-40 cloud mask thresholds to determine whether the choice of cloud mask certainty affected the statistics of Tables 1 and 2 in any way, and if so by what amount. These two clarifications were added to the text. The sub-1km peak was an artifact, and has been removed from Figure 1. At this time the authors feel that the corrections to the paper in its latest manuscript suffice in the addition of detail, and as such we have chosen not to introduce any other figures.

Comments on "Revised identification of tropical oceanic cumulus congestus as viewed by CloudSat" by S. P. F. Casey et al.

This article re-evaluated the criteria adopted by Luo et al. (2009) to identify cumulus congestus using CloudSat data and suggested that some revisions are needed. It's a short but focused study and should be useful to the cloud community, especially those researchers who use CloudSat data. However, there remain a couple of major issues in the current form:

1) As pointed out by another reviewer (S. Tanelli), this manuscript misrepresented the previous study which it revisited – Luo et al. (2009). Luo et al. (2009) clearly stated that "these radar ETH conditions are the characteristics of active convective cores". In other words, the previous study didn't miss cumulus congestus because of negligence or carelessness, but it was intended to be exclusive, in order to single out convective cores inside larger cumulus congestus features. It's these convective cores that can be further analyzed using simple parcel theory, as did in a follow-up study (Luo, Z. J. , G. Y. Liu, and G. L. Stephens, 2010: Use of A-Train data to estimate convective buoyancy and entrainment rate, Geophys. Res. Letts. 37, L09804, doi 10.1029/2010GL042904). So, I echo Tanelli's viewpoint, that is, the authors should do justice to the previous paper and present it in a correct way giving it the credit it deserves. In particular in the abstract, the authors should avoid using strong words such as "bias" or "misrepresenting".

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As mentioned in the response to S. Tanelli above, the authors did not intend to misrepresent Luo et al. 2009, and every effort has been made in the revised manuscript to emphasize that Luo et al. 2009 may be useful for identifying transient congestus, though it may not be representative of all CloudSat-observed congestus.

2) Some results contradict our common understanding of CloudSat /CALIPSO observations. The explanations presented by the authors are unsatisfactory. Some extra efforts are needed to make this a solid study.

We hope that the responses to all of the reviewers address the stated concerns and clarify any ambiguities.

(Lines 21-23 on p14888) states, "As expected, the mean CTH difference between CALIPSO and radar cloud mask 20 CTH is higher than between CALIPSO and radar cloud mask 40 CTH". If we assume that CALIPSO CTH is higher than that of CloudSat (most of the time), this means CTH(mask 40) > CTH(mask 20). This is clearly contradictory to what we know about CloudSat cloud masks. Cloud mask 40 means high confidence in cloud occurrence and should be observed at a lower level than cloud mask 20.

As mentioned above, this would be the case if the same fields-of-view were analyzed in both the cloud mask 20 and cloud mask 40 rows of Table 1. The presence of convective cells of lower degrees of certainty (i.e., without a cloud mask 40 area identified) could lead to a mean lidar-20 difference being greater than the lidar-40 mean difference (in which case, the authors agree that the words "as expected" would be incorrectly placed in the manuscript). That being said, the use of P2-R04 in the revised manuscript changes the results so that the mean lidar-20 difference is less than the lidar-40 difference.

(Lines 24-27 on p14888) CTH- CALIPSO < CTH- CloudSat contradicts my understanding of how radar and lidar view cloud tops. Cloud top should consist of some very tiny particles so lidar will see them before radar does. The authors offered the possible

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explanation that some thicker clouds (tau > 3) hang above cumulus congestus so lidar signal gets attenuated (not seeing the underlying congestus). First of all, this hypothesis can be easily tested using CloudSat data alone since CloudSat will surely pick up those thick overlying clouds (tau > 3). It will be only a single morning's programming work to sort this out. Second, I don't believe this explanation as offered by the authors (i.e., CALIPSO being attenuated) will hold because even under total attenuation, CALIPSO will still be able to identifying the very top of this overlying cloud and will consequently assign a much higher CTH value. Something is fishy here.

As mentioned above, the vast majority of what were identified in the previous manuscript as CTH-CALIPSO < CTH-CloudSat were incorrectly flagged, and should have been considered areas where CALIPSO saw no cloud at all (most likely due to instrument issues). There remain a small number of fields-of-view (about 8 out of 245,740) where the cloud layer top reported in 2B-GEOPROF-LIDAR is flagged as being viewed by CALIPSO, yet the layer top is slightly below the cloud mask 20 height of CloudSat. This is most likely a statistical aberration. The authors regret the misrepresentation of these errors in the previous manuscript.

Minor points:

1. (Lines 2-3, p14888) Why do some deep convective clouds have CTH of 2-3 km? Is this a CLDCLASS misclassification?

Sassen and Wang (2008) use a number of metrics in their cloud classification algorithm to determine cloud type. While cloud top plays a role in the separation of deep convective clouds from cumulus clouds, other measured values could lead to a lower cell being classified as deep convective. In addition, while Figure 1 shows CTH for fields-of-view, clouds are classified as contiguous features in the 2B-CLDCLASS algorithm, so many fields-of-view with low CTH but classified as deep convective are connected to areas with higher CTH. At this time the authors choose not to include this clarification in the paper as this analysis is beyond the scope of this article.

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2. (Lines 8-17, p14888) At first, I was quite confused why the authors are so "obsessed" with cumulus and deep convection. Later, I started to realize that they serve as the candidate pool from which cumulus congestus clouds (3 km< CTH < 9 km) are selected. I think this point should be made clear earlier on.

Cumulus and deep convective are identified as convective cloud types in the 2B-CLDCLASS algorithm, in Sassen and Wang 2008, and in Section 2 of this paper.

3. (Line 8, p14890) Fig. 3 shows the global distribution of cumulus congestus. I don't think Yanai et al. (1973) is a relevant reference. Yanai never showed any "global" view of convective clouds. In those days, people built up their understanding of clouds based on data collected from radiosondes, which are very sparse over the oceans.

The reviewer is correct that this is the wrong reference. Yanai et al. (1973) has been removed from this and the references, and Sassen and Wang (2008) has been placed as the reference instead.

4. (Line 27, p14891) Although this is considered a major "limiting factor" by the authors, presence of 10 dBZ is also an important factor for identifying convective core. One man's noise is another man's signal. I think the authors should clarify this, that is, putting this "limiting factor" in a proper context.

The following has been added to the manuscript for clarification: "(While a 10 dBZ echo may be important for determining active convective cores, or convective areas producing precipitation, this study focuses on all convective congestus clouds, and as such a 10 dBZ echo would not be expected in all convective features.)"

5. (Figure 3) This figures shows the total number of congestus features, regardless of size. In other words, a congestus feature of 100 km wide will be counted with the same weight as one that is10 km wide (one person, one vote, whether it's Bill Gates or a homeless – fair enough). But it may be a little misleading if people want to know how frequently cumulus congestus occurs over tropical oceans. In that case, size does

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matter. I personally prefer using occurrence frequency, instead of feature counts. After all, there might be some systematic differences in congestus size from one region to another (e.g., west Pacific Vs east Pacific).

The following has been added to the manuscript for clarification: “No difference was noted when plotting in terms of occurrence frequency (not shown) instead of total counts, suggesting no regional differences in congestus convective feature size.”

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