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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.

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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.

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.

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

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CTH as the CTH of the highest layer in the profile.

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.

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?

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. This occurs for about 75 % of cumulus cases and 90 % 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% or 10% has CTH lidar 'equal' to CTH radar? I suppose very little, but the way the sentence is phrased leaves room for this doubt. 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

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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% or 10% 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?

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?

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

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.

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

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 CLDCLASS? 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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