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Interactive comment on "The Cumulus And Stratocumulus CloudSat-CALIPSO Dataset (CASCCAD)" *by* Grégory Cesana et al.

Anonymous Referee #1

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Summary:

This paper describes a simple algorithm (CASCCAD) to classify low level clouds into either stratocumulus or cumulus based on cloud top height, horizontal cloud fraction, and vertical cloud fraction as measured by active space borne sensors (either lidar or lidar+radar). This algorithm is then compared against an existing radar/lidar algorithm (cldclass). The new dataset (CASCCAD) identifies far more cumulus than (cldclass), which agrees better with our a-priori assumption about the geographic distribution of these cloud types. An extremely simple classification based on the cloud optical thickness and cloud top pressure is also examined and found not to perform favorable compared to the CASCCAD dataset.

The paper is well written. It represents a novel contribution to the field. The paper

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Discussion paper

should be published following some major revisions. My main complaint with the paper is the comparisons with COT-CTP classification methodologies. For quite some time now people have been using more sophisticated k-means approaches that use a 2d joint histogram of COT-CTP to classify imager data into 'weather states'. If the authors want to compare their algorithm to the imager algorithms they should use the more recent approaches. I don't believe that the current comparison represents a fair assessment of how well imager data can be used to classify cloud regimes. Below is a limited list of papers that use MODIS based k-means regimes.

10.1002/2013JD021409, 10.1002/2016jd025193, 10.1002/2015jd024502, 10.1002/2016jd026120, 10.1007/s00382-016-3064-0, 10.1007/s00382-017-3806-7

I believe that the CASCCAD should be compared against the k-means approach ideally from MODIS but possibly also from ISCCP. The MODIS based regimes do have a clear advantage over CASCCAD; there is believable way to compare model output to those regimes. The author need to seriously elaborate on how CASCCAD can be used to evaluate models or drop the statements about model evaluation. I suggest the latter as I don't think it is possible to do a meaningful comparison. Other than these two points, I only have minor comments which are listed below.

Comments:

-Page 3, line 23: should be 1.4x1.7 km (tanelli et al., 2008). 1.1 is the distance between adjacent pixels but they overlap somewhat.

-page 4, line 31. Same as above

-page 6, line 18: The RL-GeoProf release 5 (newer version) is now available past the 2011 anomaly.

-page 7, line 18: why are attenuate profiles excluded from HCF? They are certainly cloudy right?

-page 7, section 3.4: can you explain here what influence multiple scattering has on the signal? It seems to me that when you see several bins of lidar return you are probably looking at a multiply scattered signal.

-Section 4.2.1: This COT-CTP method is quite simple. The k-means methods of Rossow et al. (2005) applied to ISCCP or more recently Oreopolous (https://doi.org/10.1002/2013JD021409) applied to MODIS probably do a much better job because they consider the joint histograms of COT-CTP over a large area. You should at least comment on this. There is a 1x1 degree daily gridded dataset that has maps of the regimes. If I were you I would get this data and plot his regimes next to yours. The aqua MODIS regimes are even coincident in time and space with yours.

-Page 10, line 29: I would remove this statement unless you want to elaborate. I can't see how you could use this data to evaluate a GCM in anything but a qualitative way. There is generally nothing in the model called Sc and Cu to compare against. Also, there is no way to apply an instrument simulator to recreate the data using the model output because the categories depend on spatial continuity.

-Figure 15: I'm concerned about the cloud fraction PDF's (left column). What is the resolution over which the cloud fraction PDF's are calculated? Is the resolution different for the different products? The PDF depends on the resolution so if the different products have different resolution the PDF's will be different just based on that fact. For this reason, the nadir-only

-sampling of the CALIPSO-CloudSat data will inherently be different than a 2D imager. I think you should remove these panels because of these concerns and I also don't think they add much to the paper.

-Page 123, line 20: Again, I don't know how you use this to evaluate a model. Just because the model has some arbitrary distinction between a boundary layer parameterization and a shallow convection parameterization doesn't mean that this is the same as stratocumulus and cumulus as you have defined them from the observations.

Furthermore, newer parameterization are beginning to ‘unify’ these distinct regimes (e.g. CLUBB, EDMF). I think the dataset is interesting enough in its own right without having to sell it as a model evaluation tool.

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