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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Frequency of deep convective clouds in the tropical zone from ten years of AIRS data" *by* H. H. Aumann and A. Ruzmaikin

Anonymous Referee #1

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Review of "frequency of deep convective clouds in the tropical zone from ten years of AIRS data" by Aumann and Ruzmaikin

This paper investigated tropical deep convective cloud (DCC) occurring frequencies (CF) during the past 10 years using AIRS L1 radiance data. With different definitions of DCC using different thresholds, consistent significant increasing (decreasing) trend is found for tropical land (ocean). The authors further correlated the trends with ENSO index and vertical velocity @ 500 hPa, and claimed that the trends reflected the decadal variability that shifted the distribution of DCC in the tropics.

The entire paper is written in fluent, clear language that is easy to follow. The logic and methodology are in general deliberative. Besides some minor issues that I'll list below, there are at least three major (general) aspects that may be improved in the revised version: (1) there are at least 5 definitions of DCC in this paper, some of which find overshootings while the rest correspond to cold cloud features (CCF). I think thresholds such like DCC210 and DCCw0 actually select many anvils and cirrus features, as shown in Fig. 3. As the rest of the paper use DCCw0 to study the trend and interannual variabilities, I doubt if DCCw0 is suitable to represent "deep convective clouds" only. The authors should first spend at least a paragraph in section 3 to reemphasize the threshold sensitivity to different clouds (cloud clusters), and secondly, use another strong threshold, such as DCCw4 or DCCt2 in the trend and decadal variability studies, unless otherwise the authors prefer to alter the title. Besides, the authors need to make sure the abbreviations being consistent throughout the context. For example, DCCw0 & DCCw4 correspond to DTW in line 15 of pp10016, while DCCt2 is equivalent to DTR in the same line.

(2) Are these threshold applied to all 90 view-angles. Do you consider the lift of weighting functions at side-views would result more sensibility of overshooting clouds? DCCw0 & DCCw4 might suffer the least at the side-views as they are defined by channel differences. But I'm concerned about a fixed threshold such as DCC210 and DCC200, and a threshold defined based on nadir tropopause climatology.

(3) The authors mixed together the concept of cloud "frequency" and "area" throughout the paper. As the footprint size of AIRS vary with scan-angles and clouds are inhomogeneous within a single footprint, one cannot directly infer the cloud-covered area from dividing the DCC number by the total pixels.

(4) ω500 can be interpreted as a proxy for large-scale convective cloudiness. However, the quantity derived from AIRS data is convective cloud occurring frequency, not the area. A changing climate could cause changes in DCC occurring frequency, DC spread area, DCC strength, etc.

Minor issues (L123pp10010 means Line 123 at page 10010). Abstract: L10-14pp10010: As the three definitions yield different % of CF, it is not clear to me why the authors chose to emphasize DCCw4 in the abstract, while the paper used DCCw0 the most. I suggest to alter the sentence as: "We find that DCC occur 0.06% - 0.8% of the time according to different definitions and thresholds".

Same lines: 72%+39%=101% (?) For AIRS data, DCC includes overshooting, while here the authors treat them as different concepts.

L18pp10010: why do you associate tropical DCC with "global" precipitation? The regime shift and decadal variability you talked here seem to only associate with ENSO and Walker circulation, both of which are tropical phenomena.

L21pp10010: "This" -> "The consistent trends of DCC & precip".

L25: "past events": what events in particular? ENSO?

L10pp10014: "22000 CCF" -> "22000 pixels of CCF". They are not individual events.

L3pp10017: "Gettelman" -> "Gettleman".

L18pp10017: "percent of the area" -> "percent of the occurrence frequency".

general question about section 3.2: this part of work demonstrates that none of the thresholds here select DCC only. All of them include other upper-troposphere clouds, which are not always originated from local convections. For example, the characteristic and formation mechanisms of cirrus are completely different from DCC. How do you know the trend and CF are from DCC instead of from other UT clouds?

L13pp10018: "200000 DCC210" -> "200000 DCC210 pixels".

L20pp10019: why do you show night data instead of day+night averaged data?

Section 5.3: how did you select "day" and "night" scenes from daily 4-times NCEP data?

L2pp10024: "equally significant": what do you mean by that? same p-value?

L26pp10024: you mentioned the trend in precipitation several times in this paper (e.g., abstract, here, summary). But no references have been given. More importantly, the increase in precipitation frequency? strength? duration? Increase in DCC occurring frequency may result more frequent but less vigorous precipitation, or more vigorous but less frequent precipitation. It's hard to prove the DCC trends through precip trend. Table 4: What does the last line mean?

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C4455/2013/acpd-13-C4455-2013-supplement.pdf

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