Author's response in italics

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 stated on p.10015 we compare five ways of DCC selections, three of them are defined by legacy methods. Our focus is on DCCw0 and DCCw4. Figure 1 shows that the DCC selected with the condition TB11 (equivalent to our bt1231) < 210K are much warmer than the tropopause cold point temperature except near 30N and 30S. Since they are far below the tropopause and contain many anvils and cirrus features, they are not the focus of our paper.

As stated in Section 3.2 on p. 10018, line 8, only 53% of the DCCw0, in contrast to 72% of the DCCw4, are identified as deep convection using the co-located microwave AMSU-HSB data. Since the AIRS and AMSU-HSB footprints were aligned to within 0.2 degree of the 1.1 degree FOV (p.10013, line 19), a 72% matchup should be considered as very good. Thus some of DCCw0 and the majority of DCCw4 are deep convective according to microwave data.

As stated with the discussion of Figure 3 (p.10 line10), 22% of the DCCw0 and 39% of the DCCw4 are matched up with AMSU-HSB overshooting convection.

As discussed with Figure 3 (p.10018, line 10) and noted by the reviewer, there is a high degree of spatial correlation between DCCw0 and DCCw4. This suggests that DCCw4 are imbedded in clusters of DCCw0.

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.

We included DCCt2 only for its legacy value. 39% of the DCCw4 are microwave overshooting convection. In Table 3 we give trends for all five definitions. As noted in the discussion of trends *(section 5.1 p.10021, line 3-4) "Note that while the 10 yr mean frequency of the DCC differ by almost 2 orders of magnitude, the trends) are within a factor of two."*

We propose to change this sentence to clarify why trends in DCCw0 as used in the rest of the paper:

"While the 10 year mean frequency of DCCw0 is four times larger than the frequency of DCCw4 for the entire tropical zone (Table 2), neither set has a significant trend. However, the trends normalized to frequency for day/night ocean/land are highly significant, they have the same sign, but differ by about a factor of 2, with the DCCw4 having consistently faster trends than DCCw0."

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.

The abbreviations are consistent, but the many numerical variables may be hard to follow. DTR =bt900−T_{Trop} and DTW =bt1231−bt1419 are numerical values. About 0.6 % of the area of the tropical zone satisfies the condition DTW <0 (they are thus identified as DCCw0), but for DTW=0 the average DTR is +12 K, i.e. the average DCCw0 cloud tops is below the tropopause.

(2) Are these threshold applied to all 90 view-angles.

Yes.

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.

Yes. This is why we used DCCw0 and DCCw4.

But I'm concerned about a fixed threshold such as DCC210 and DCC200, and a threshold defined based on nadir tropopause climatology.

We agree. This is why the legacy DCC210, DCC200 and DCCt2 are included only for comparison with legacy work.

(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.

The AIRS footprint diameter at nadir is 13.5 km; it is 16.5 km at the Earth's surface when averaged over the full cross-track swath. We define a footprint averaged DCC. There is no question that for any footprint identified as DCCw4 there could be sub-pixels within the footprint in which dw < 0 (most likely due to anvils and cirrus). But these sub-pixels are compensated by subpixels with dw << -4, such that for the average over all sub-pixels dw < -4.

We use cloud frequency of occurrence and fraction of the area covered by DCC as synonymous, although this could be confusing at first glance. Let us clarify the situation. We infer the mean DCCw4-covered area fraction. Every day we search the tropical zone for DCCw4 using N non-

overlapping footprints. The N footprints don't have to cover the entire tropical zone, but they have to be a spatially unbiased sample. If n of the N footprints are identified as DCCw4, then n/N is the fraction of the tropical ocean covered by DCCw4 on that day. This is what is shown as one dot in the top of Figure 4. There is a high variability, but 10 years of data show a stable pattern. If we consider the tropical zone as an area covered with 16.5 km diameter footprints, the probability that any one footprint is a DCCw4 is equaled to the fraction of the area identified as DCC. If this probability increases, then the frequency of occurrence of DCCw4 increases. In this sense the faction of the area of the tropical zone covered by DCC4 and the frequency of DCCw4 in the tropical zone can be treated as synonymous.

We changed the sentence on line 16 10017 to "The ratio of the count of footprints selected by the thresholds divided by the count of all footprints in a given area and multiplied by 100 represents the percent of the area covered by these objects."

(4) omega500 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.

We derive the fraction of the area associated with DCCw0 and DCCw4. Since we know the area of interest in square miles, and we know the fraction of the area covered by DCCw0 or DCCw4, we can compare it with the area covered by omega500.

A changing climate could cause changes in DCC occurring frequency, DC spread area, DCC strength, etc.

We agree. The inverse correlation between cloud top temperatures and rain rate was the basis of the GOES precipitation index (Joyce and Arkin 1996). Based on the larger fraction of DCCw4 footprints associated with microwave overshooting convection than for DCCw0, we can argue that a DCCw4 is "stronger", i.e. likely more heavy rain, then a DCCw0. This is shown in Aumann et al. 2011. We noted (Section 5.5 p.10025) that the less frequent DCC (DCCt2 and DCCw4 types) show in the past 10 years consistently faster decreases in the frequency over ocean and larger increases over land than the more frequency ones (DCCw0 and DCC210 types). A changing climate could cause changes in DCC frequency at a given detection threshold, the mean spacing between DCC, or a change in the ratio of frequency of DCCw0/DCCw4.

We propose to add the following text at the end of Section 5.3: "Since a changing climate could cause changes not only in DCC frequency but also changes DCC strength or changes in the spatial relationships between DCC, there is no reason to expect the same trends in the decrease of area occupied by $\omega_{500} < 0$ *and the DCC occurrence frequency."*

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".

The suggested sentence is inserted. We emphasis DCCw4 since they are associated more with overshooting convection than DCCw0. In terms of anomaly trends DCCw0 and DCCw4 are *highly correlated, but since DCCw4 are less frequent, the anomalies of DCCw4 are also more noisy, which increases the uncertainty in the anomaly trends, as seen in Table 3.*

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

We actually mean the following: 72% of the DCCw4 are associated with microwave deep convection. A subset of these DCC (39% of all DCCw4) are identified using the microwave data as deep convection and overshooting convection. We agree that this needs to be reworded since this amount of detail is not needed in the abstract. We change the sentence on p 10010, line 10 starting with "We find that " to the following sentence: Simultaneous observations of these DCC with the Advanced Microwave Sounding Unit-HSB (AMSU-HSB) using 183 GHz water channels provide a statistical correlation with microwave deep convection and overshooting convection. In the past 10 years….

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.

We agree and remove the misleading term 'global', since we in this paper we only discuss DCC in tropics. Most DCC are located in the tropical zone.

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

Corrected

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

'past events' is replaced with 'past ENSO events'

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

Corrected to ".. each day on average 22000 footprints associated with CCF are found…"

L3pp10017: "Gettelman" -> "Gettleman".

The correct spelling is "Gettelman".

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?

We agree that strictly speaking none of the threshold used in this paper select DCC exclusively. Some of the "DCC" are false alarms, i.e. other upper-troposphere clouds, which are not always associated with local deep convection. Our argument is statistical: Since 72% of the cold clouds identified as DCCw4 with AIRS are also identified using the collocated microwave AMSU-HSB *data as deep convective, the frequency may be overestimated, but it seems unlikely that the observed trend in the frequency anomaly is due to an increase in the fraction of false alarms.*

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?

Unfortunately Figure 9 has the right caption, but in the type setting process Figure 8 was inserted into the space for Figure 9. This causes the observed discrepancy. The correct Figure 9 (reproduced below) shows the anomaly of the daily count (day + night overpasses) of 2.5-degree cells identified with ω_{500}<0.

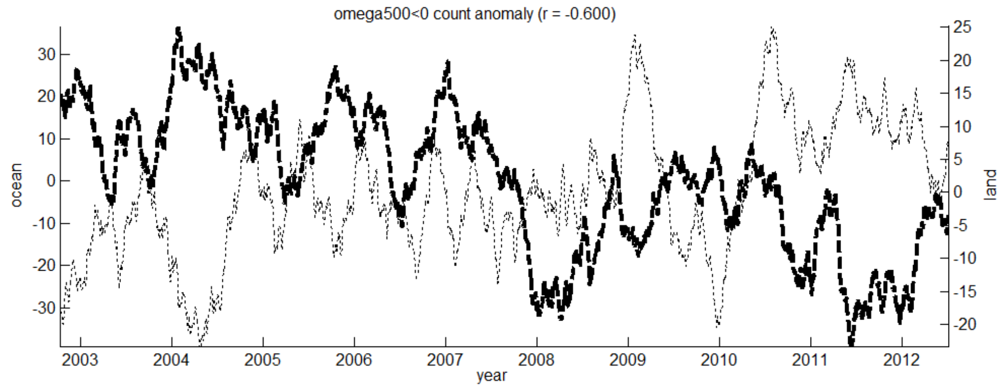

Figure 9. Overlay of anomalies of the daily(day+night overpasses) count of the 2.5 degree cells identified with ω_{500}<0 for ocean (bold) and land, smoothed with a 96-day running average.

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

We mean that both trends are statistically significant. The sentence is corrected.

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.

We refer to the model-based work by Allan and Soden (2007). We agree that DCC trends can’t be derived from precipitation trends based on models.

Table 4: What does the last line mean?

It looks like a table creation artifact. The row 4 of Table 4 must be deleted.

Author's response in italics.

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 #2
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Aumann and Ruzmaikin present an analysis of deep convection in the tropics as observed by AIRS. From a climate science point of view, the focus of the paper is on the partitioning of deep convection between land and ocean, and the question whether there is a trend in this partitioning over the period of AIRS observations analyzed here (2002-2012). From a methodological point of view, the paper provides substantial information on what may be classified as "deep convection", or "overshooting convection" in remote sensing data. I welcome the discussion of different metrics, as this is a constant source of confusion in the literature. However, I think it would be possible to somewhat improve the organization of the paper; in its present form, it is occasionally jumping back and forth between science results and methodological aspects which, together with the large number of acronyms, tends to confuse the reader. I leave it to the authors how they want to address this issue; a possible strategy is to focus first on the "scientific question" (namely the partitioning between land and ocean) based on the indicator for deep convection the authors consider the most relevant, and then have an extended discussion how these results depend on the definition of "deep", or "overshooting" convection. Below I list some minor comments. I think some further exploration of the connection between seasonality and trends (details below) would be interesting.

This is a complicated subject. In an earlier draft we had the paper organized as suggested, but it required forward and back referencing which were just as confusing. The major focus was the trend in the tropical DCC, and when none was found we noted the cancelling trends for land and ocean and the correlation with the MEI. To better outline the science goal we add two sentences following the first sentence of the abstract: "Thus it is important to look for changes of DCC in a changing climate. Ten years of data collected by Atmospheric Infrared Sounder (AIRS) allows us to identify decadal trends in frequency of occurrence of DCC over land and ocean."

Minor comments/suggestions

Abstract: The abstract, lines 10-14, illustrates the problem with the various thresholds: While "threshold 2" is defined (line 6), the subsequent statement is unclear; what is meant when you say that "72% of them are identified as deep convective, 39% are overshooting ..." - what's the criterion for "deep convective", "overshooting"? I understand that these definitions are then given in the text, but as it stands, the information in the abstract is ambiguous.

We actually mean the following: 72% of the DCCw4 are associated with microwave deep convection. A subset of these DCC (39% of all DCCw4) are identified with the microwave data as overshooting deep convection. We agree that this needs to be reworded since this amount of detail is not needed in the abstract. We change the sentence on p 10010, line 10 starting with "We find that " to the following sentence: Simultaneous observations of these DCC with the Advanced Microwave Sounding Unit-HSB (AMSU-HSB) using 183 GHz water channels provide a statistical correlation with microwave deep convection and overshooting convection. In the past 10 years….

Abstract/L28: Same problem - the range ("0.06%-0.8%") is a full order of magnitude, we should be given some information on what the criteria are, else the information is not really of much use to others.

We propose to replace the sentence "Depending on the selected threshold, the frequency of DCC in the tropical zone ranges from 0.06% to 0.8% of the area." with the sentence "The area of the tropical zone identified as DCC is typically much less than 1%".

Abstract/last sentence: This statement is not clear from the context of the abstract. I understand that it refers to P10025/L16ff, but in the context of the abstract it's unclear what you mean (I also have some questions concerning the P10025/L16ff section, see below.)

The last sentence in the abstract will be deleted, since with more explanation, as given in Section 5.4., its meaning is unclear.

P10011/L10ff: It is assumed here that the reader already knows exactly what these wavelengths imply; perhaps help the reader with a quick reminder here.

"was first noted in 11µm ***thermal infrared*** *images…"*

P10015/L12: Which tropopause - lapse rate or cold point? Note that it is known that NCEP drifts massively at tropopause levels; overall, I would argue that this particular level is also a fairly arbitrary reference level.

We agree that there are many definitions of the tropopause. We use the tropopause cold point temperature as defined by NCEP, see http://www.esrl.noaa.gov/psd/data/gridded/data.ncep.html.We include the tropopause cold point climatology-based definition of DCC only for its legacy value.

P10016/L15: Explain what "DTR" and "DTW" stand for; having an association makes it easier to keep the overview over all the acronyms.

The acronyms are explained as follows: (P10016, L.15) DTR is the difference between the brightness temperature measured by the 900 cm^{-1} atmospheric window channel, bt900, and the tropopause cold point temperature. (P10014, L19) DTW is the difference between the brightness temperature measured by the 1231 cm^{-1} atmospheric window channel, bt1231, and the brightness temperature in the strong water vapor absorbing channel at 1419 cm^{-1}, bt1419.

Figure 2 illustrates the relationship between the $DTR = bt900-T_{Trop}$ and DTW = bt1231-bt1419 in a scatter diagram for all CCF collected in September 2002 with DTR<20K.

This sentence is the new caption for Figure 2.

P10018/L21: 20S-20N?

Yes, 20S-20N is what TRMM uses. There is no uniformity in the discussion of DCC. We use 30S-30N to contain the seasonal motion of the ITCZ.

P10019/L20: Is "overlaid" the best word here? Perhaps "calculated separately"?

We will replace the word "overlaid" with "calculated separately" as suggested. *The white traces in Figure 2 represent the mean, the 16% and 84%-tiles of the distribution of the data within each one degree K wide DTW bin.*

P10019/L21: Although I think I understand what you want to say, the sentence as it stands makes little sense: How can ocean/land partitioning be "consistent" with the "diurnal cycle ..."?

The sentence will be corrected to

“This observation is consistent with rain radar measurements of precipitation in the tropics (Liu and Zipser 2005)”

P10019/24/Figure 4: Can you inform the reader where this seasonality comes from? (I.e. the tropical mean hydrological cycle has very little seasonality. It would be helpful to know whether the observed seasonality arises, e.g., from seasonality in the thermal structure in the TTL, from seasonality in land/ocean partitioning, or whether there is a genuine increase in deepest convection over land and ocean independent of TTL temperatures.)

The 30S-30N tropical zone seasonal cycle in DCC is small. *As shown in the daily DCCw0 record shown in Figure 4 (and the similar DDCw4, not shown) the seasonal variability is overwhelmed by the random nature of clouds and the deep convection process. With a 3 month smoothing filter the peak seasonal signal varies by less than 10% of the mean, with clear minima for the NH winters, not so clear maxima in the NH summer. This points to the seasonal signal as being related to the solar input. Superimposed on the seasonal variability is a bigger ENSO signal.*

The question of the seasonality is also highly relevant for your subsequent analysis of anomalies thereof. That is - you find little trend in the tropical average (Section 5.1) because of compensation in changes over land and ocean, yet this seems to not work on a seasonal basis. If the seasonal variations would arise from differences in land/ocean convection, then a shift in land-ocean partitioning should also give a trend in the total. Hence - what does your result imply?

Figure 4 shows that there not much seasonality. We interpret the absence of a trend for the tropical zone, compared to the clear trends of opposite sign for land and ocean as a part of a multiannual oscillation, where latent heat is conserved, and its redistribution and release as rain oscillates between land and ocean.

P10021L2: Clarify - if I understand correctly, the sentence should state that "the absolute DCC frequencies *using different criteria to define DCC* differ by ... the trends are very similar".

We plan to change this sentence to be more specific: “While the 10 year mean frequency of DCCw0 is four times larger than the frequency of DCCw4 (Table 2), for the entire tropical zone neither set has a significant trend, while the trend normalized to frequency for day/night ocean/land are highly significant and differs by less than a factor of 2, with the DCCw4 having consistently faster trends than DCCw0.

P10024/L9: No, mass conservation cannot be invoked; the number of deep storms is not a conserved quantity (which is also nicely illustrated by the seasonality). The mass associated with each DCC can vary, and the radiative cooling can vary (i.e. more ascent can be balanced by more descent - the question is whether the surface energy budget allows this to happen).

You are correct. Mass is not conserved. The available latent heat is conserved, but the its redistribution and release as rain shows what may be a multi-decade oscillation between land and ocean, superimpose on ENSO effects. The sentence will corrected to: “…can not be caused by mass conservation, i.e. an increasing (decreasing) ascent balanced with increasing (decreasing) descent., due to the complexity …”

P10025/L16ff: I think I understand what that statement refers to, but please write it out explicitly. As of now, it’s too vague. If I understand you correctly, you think that the fact that the rates of change differ somehow implies that more extreme events respond stronger. However, I am not sure whether I would believe that; but I don’t know whether you really mean this since the text is too vague.

From Table 3 we note that the less frequent DCC (DCCt2 and DCCw4 types) show consistently larger decreases in frequency for ocean and larger increases over land than the less frequent ones (DCCw0 and DCC210 types). This very consistent pattern there is worth noting, but not enough to write a paper on. This is why we left it somewhat vague. Since the frequency of occurrence of DCCw4 is much less than 1%, DCCw4 can be considered as extreme events. S*low change in mean climate variables can lead to much stronger change in extremes as the values in the tail of probability distribution functions. We will change this sentence to:*

*"This finding **may fit** into a framework of how weather extremes **are expected to** respond to climate change (c.f. Allen and Ingram 2002; Emori and Brown 2005), based on the distribution functions of non-gaussian distributed events."*