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Discussions

Interactive comment on "Analyzing cloud base at local and regional scales to understand tropical montane cloud forest vulnerability to climate change" *by* Ashley E. Van Beusekom et al.

Anonymous Referee #3

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This paper describes measurements of cloud base altitude using ground-based remote sensing at a site in close proximity to the Luquillo Mountains, Puerto Rico. The authors stress the importance of cloud immersion to the current ecosystem of the tropical montane cloud forest, while also advising the reader that the various mechanisms by which these ecosystems exchange water with the atmosphere are still not fully understood. The authors suggest that rising cloud base altitude as a result of climate change would stress vulnerable species even if rainfall rates in these regions were to remain high. In addition, they hypothesize that the lack of cloud water deposition and increased evapotranspiration, resulting from elevating cloud base, could affect the watershed dynamics in the mountains. The authors also project that their study site may also be vulnerable

to changes in the wet season, especially during wet season drought periods.

While the manuscript documents an important baseline for assessing future changes at this site, the reporting of the data is quite laborious to follow, and by trying to broaden the scope to assess conditions across the wider region, I think the discussion loses a lot of focus. I am not against the idea of using the other data products to bolster the understanding of the regional context, but I think the authors could make a clearer connection with the main study site. The extensive use of acronyms coupled with quite intricate data reduction methods makes the paper hard to read. My recommendation is that the manuscript needs major revisions to address the substantive points listed below, but I would also encourage the authors to consider ways to improve readability, perhaps by reducing the acronyms and perhaps by focusing the description of the results a bit more to the aspects that they wish to stress during the discussion/conclusions section.

Substantive comments:

1) Study Area section: much of this section reads as an extension of the Introduction, in its detailed description of literature focused on tropical dynamics together with references to previous observational work in the region. However there is no actual description of the local terrain nor mention of other geographical features pertinent to the study. Many readers will not be familiar with Puerto Rico and/or the wider region and so a detailed map or at least a text description documenting the location of the ceilometer and other relevant locations such as the ASOS sites (you give coordinates for the TMCF, but in reality it is not a point. I had to wait for the Methods section to get a brief description of the ceilometer site). If you choose to do a map, you could also indicate the terrain contours in detail and show the proximity to the coast, both of which are very important to the discussion. I would recommend that the current content of this section be worked into the Introduction. 2) Cloud base statistics methods: In Page 3 lines 14-21 there is a thorough description of the method the authors used to generate various statistics for cloud base. While I understand that broken cloud, multiple layers and/or rapidly changing conditions may justify a more detailed algorithm

for identifying cloud base characteristics, I think the current method involving quartiles, tertiles and octiles to produce a set of four cloud base metrics is really confusing. This becomes more confusing when these metrics are then displayed in a histogram type format, because it is hard to tell which of these metrics is most relevant to the ecosystem health. Could the histogram not display the raw 30-second resolution data and that way the frequency counts could be related to a physical diagnostic (i.e. time-in-cloud)? Unless there is an ecosystem relevance to the various statistical quantities, (do they capture/differentiate the intermittency or variability of the cloud within hourly or daily timescales?) the authors should reconsider a more readily interpretable set of metrics. If there are specific reasons, concerning the ecosystem and/or hydrology, for the specific choice of quartiles, tertiles and octiles, then a description of this is certainly warranted. Such a description would be useful for future work, if the same metrics were carried over. 3) CALIPSO: The authors should be careful in their usage of the CALIPSO cloud mask for the purposes they report. Lidar signal attenuates within optically thick clouds and so it is not possible to determine cloud thickness in that case. Winker et al. (2009) report a cloud optical depth of 5 as the threshold below which thickness can be determined and for optically thicker clouds, only the cloud top altitude is possible. Trade cumuli would typically be classified as optically thick using this threshold. On p5 L32, Winker et al. (2009) could be a more appropriate reference than Hunt et al. (2009), since that reference makes no mention of the cloud data products. When using the vertical feature mask, if there is “no signal” data below “cloud” data it is not possible to determine thickness. 4) LCL calculation (described in Appendix A): the authors provide a brief description of calculations, which were done to determine the LCL. The LCL is a parcel property (i.e. given the temperature, pressure and a humidity variable – RH, dew point, mixing ratio. . . - the LCL altitude, LCL pressure and LCL temperature can be uniquely defined). The authors state that surface observations are used, which is an acceptable choice, and they calculate LCL temperature with appropriate citation of the method. However at that point they also have the LCL altitude, by definition. Instead they describe an interpolation of the LCL temperature to determine a corresponding

altitude on a radiosonde sounding. It is not clear to me what that altitude means, but it is not the LCL. This should be addressed before the paper is published.

D.M. Winker, M.A. Vaughan, A. Omar, et al. Overview of the CALIPSO mission and CALIOP data processing algorithms J. Atmos. Ocean. Technol., 26 (11) (2009), pp. 2310-2323

Minor comments:

P1 L30: “Smaller mountains have lower temperatures and steeper adiabatic lapse rates”. Please consider rewording this. It is at odds with Line 27-28, and also do you mean pseudo-adiabatic lapse rates? The word steeper is generally confusing when describing lapse rates because of the conventional way of plotting them.

P3 L16 “more diurnal effect of convection” Suggest rewording to make this statement less vague

P3 L24 “originate with strong winds. . .” is this relevant to your site? Later you provide another reference suggesting that winds are 3-5m/s, which is quite light.

P3 L25 suggest “lifting” instead of “forcing”

P4 L3-4 “Raw ceilometer. . .” what you describe is not really raw data, it is a processed product.

P4 L9 How is temporal and spatial variability separated with a ceilometer? This whole sentence is vague, consider rewording.

P4 L13-14 “. . .more complete picture of the climate of the entire atmosphere above the site.” Consider replacing “atmosphere” with “troposphere” since you measure <8.2 km. Consider also replacing “climate” with something more specific to the measurement like “cloud patterns” or “cloud variability”

P4 L28-31 Ceiba is only a few km from the site. MSLP is certainly going to be very similar. If this is an example to support the claim that “weather immediately around the TMCF was homogeneous in pattern…" it is quite weak. Consider removing it. Also the statement "the weather immediately around the TMCF was homogeneous in pattern, but clearly not in magnitude" is vague but also confusing. Later, you go on to show various heterogeneities (associated with the topography and other features) that appears to be in direct contradiction with this statement. Please clarify what you mean and consider using another term instead of "weather".

P4 L34 "climate oscillations" – I think you should be more specific. Also, I think you should provide a bit more clarity on why you removed all the various segments. Was it because you could not clearly define the break between seasons? If so, do you think there is another way of classifying your seasonal groups rather than calendar months?

P7 L29 "within 100 m the LCL" missing "of"?

P8 L3 Suggest stating which stations you are referring to instead of "Caribbean ASOS stations windward of Luquillo" or else, perhaps substitute "windward" for "east"

P8 L3-4 "stable" do you mean that the marine boundary layer is thermodynamically stable?

P8 L5 "remnants of this weather pattern may be carrying on to land…" this is an awkward statement, consider rewording. Also in reference to the previous point about homogeneous "weather" this statement also seems at odds.

P8 L27-29 Please consider rewording this whole sentence. In its present state the meaning is unclear and the wording may need adjusted (e.g. "…evidence that consistently as low, or lower, clouds exist…")

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