

Interactive comment on “Optical and Geometrical Properties of Cirrus Clouds in Amazonia Derived From 1-year of Ground-based Lidar Measurements” by Diego A. Gouveia et al.

J. Campbell (Referee)

james.campbell@nrlmry.navy.mil

Received and published: 4 August 2016

This paper describes ground based lidar measurements of upper tropospheric clouds over one year collected at a site in Brazil. Physical and optical cloud properties are described for a series of queries that characterize the state of upper tropospheric clouds for an equatorial locale where few datasets have been collected and reported in the literature. The paper is well organized. The narrative is well written. The figures have been well designed, and are clear/legible. The subject matter is wholly appropriate for ACP, as ground-based lidar observations remain critical context for evaluating cloud and aerosol properties from satellite observation. Long-term records of clouds and aerosols from such sites have been reported for years, and remain a critical fixture of

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the peer reviewed literature.

This is the first time that this reviewer has considered this manuscript.

My summary recommendation for the Editor is that this paper should undergo a major revision for both scientific and technical content. I'm attaching my technical notes, which include a series of editing recommendations and minor questions. I will list my major concerns here in this portion of the review.

My primary scientific concern relates to the definition of "cirrus" clouds in the manuscript. I will sign this review, so its important that I reconcile my concern with existing literature. 45 years after the first long term reports of cirrus clouds began appearing in the literature (Platt), the community has reached a point where we should and need to be much more diligent with how we characterize our observations for peer review. Cirrus clouds are a phenomenological classification based on ground-apparent observations of ice-phase clouds in the upper troposphere. In a recent paper that I authored (Campbell et al. 2015), we went to significant length to demonstrate a practical and viable definition for cirrus clouds in autonomous long-term datasets like this one, and in particular for those that lack a polarized backscatter measurement. Whereas I am a primary advocate of papers just like this, and have participated in multiple studies documenting cirrus in a manner consistent with the narrative here, I cannot advocate for a paper that used a simple thermal threshold like -25 C as being a practical delineator for cirrus cloud presence. This absolutely has to be revised. I recognize that this is a serious request, and I raise this point very respectful of the work that has been put into the manuscript, the statistics and the analysis. However, I question every number you have in here, again respectfully, because of such a simple and non-physical definition applied for discriminating these clouds.

I would respect if the authors were to disagree with our conclusions/recommendations in Campbell et al. (2015). But, in response, they'd better come up with a physically-based reason for doing so. Cloud top temperatures of -37 makes physical sense for

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the class of clouds that we call cirrus. Its a practical and defendable threshold. The lidar community, as we argue in our paper, cannot continue to produce datasets with haphazard classifications and expect anyone in the climate community to take our work seriously.

This point must be addressed for clarity and consistency.

MINOR SCIENTIFIC POINTS (in order of the manuscript)

- Its unclear what the authors are saying about the presence of $SNR > 3$ in the upper troposphere with respect to cloud observation. Do they mean clear-sky? Or, do they mean within particulate scattering layers?

- Its unclear how the authors define the tropopause, and thus accommodate the potential for resolving the bottom/top of the tropopause transition layer, in Section 2. This hurts the discussion later on where context is necessary for understanding where the clouds are with respect to this boundary.

- Since the sample size is stated to relative to the ability to measure $SNR > 3$ in the upper troposphere, all of the samples appear to be relative occurrence frequencies and not absolute ones. This is HIGHLY confusing. There is no way that you're resolving an absolute cloud frequency of 67%. In a new paper that we have in Early Online Release in JAMC (Campbell et al. 2016), we show in a year's worth of MPLNET observations at Greenbelt, MD an absolute frequency near 16%, which owes to the attenuation of the beam from low-level clouds and undersampling of the upper troposphere. There are multiple places in the narrative where serious confusion arises and the speculative discussion becomes meaningless because of this confusion.

- Speaking of this issue, nothing is said of the work of Thorsen et al. (2011) and Protat et al. (2014) and undersampling issues relating to ground-based profiling, attenuation, and the relative cloud samples that we have to analyze. This is a serious weakness that leads to three other points of concern.

- It is discussed that the lowest cloud observations occur around solar noon (10-12 LT). This leads me to believe that your instrument is suffering from issues with SNR from the bright background, even at 355 nm. Whereas it is introduced that this is potentially a real artifact, I see no reason to take such a claim at face value. As I cannot evaluate your algorithm or its performance, and with the practical understanding that you are willing to deal with cloud samples in the algorithm at an SNR as low as 3, I cannot help but conclude that you're dealing with sampling issues due to background noise.

- Furthermore, all of the speculation about the transport of clouds vs. near-source convective generation is very weak. The authors are forgetting that if the clouds are being generated at/near or on top of them, the lidar will not be profiling the clouds. You are **always** dealing with transport of some kind, as such. I recognize what they are trying to say, but recommend they be much more circumspect about how they are delineating source/transport with respect to the limited information that they have.

- The distribution of clouds as a function of COD also relates to sample bias and attenuation effects. Yes, there is an exponential distribution of cirrus cloud occurrence with respect to COD (again, see what we have in Campbell et al. 2015). However, the distributions that you have with respect to subvisible, optically-thin and opaque clouds is absolutely not consistent with other studies. There should roughly be a 50-60%/40-50% distribution between translucent and opaque clouds. In Campbell et al. (2016), we see a very similar distribution as yours that we fully attribute to sampling bias. I see no reason to think this sample is not subject to the same effects.

- Although there is a point where the authors show a correlation between COD and cloud base, cloud base is a nearly useless parameter for such vigorous study. As myriad Sassen papers discuss and describe, cloud top is the most important layer because this is where cirrus cloud nucleate, grow and begin falling. Cloud base, as such, is redundant. Its simply the boundary where evaporation/sublimation is complete in falling crystals. So much effort in the narrative is spent on cloud base and drawing physical correlation, where it seems to have no physical meaning. Cloud top should be

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the focal point.

- As such, there is absolutely no physical basis for evaluating lidar ratio versus mid-cloud temperature. It makes absolutely no physical sense. Now, I recognize that the CALIPSO team has done this very thing with their analyses. I don't agree with them either. But, they are dealing with a downward looking dataset, at least, and this offers other challenges that the authors are not dealing with in the zenith. Whereas I would accept if the authors referenced Garnier et al. (2015) and wanted to leave this as is, I still wouldn't think that it made much physical sense. In particular, as with CALIPSO, you're never actually going to know for certain what the mid-cloud temperature is (or unfortunately the cloud top temperature is) because of attenuation. For CALIOP, this is actually a bigger issues, since they can attenuate working downward with clouds that ground-based lidars would likely never reach. But, the comment still remains. I recommend sticking with what you can physically interpret, and particle effective size and habit are likelier in the long run to relate with available water vapor and temperature found at cloud top than somewhere within the cloud.

- No uncertainty analysis is provided for the lidar ratio analysis. This concerns me, again, because of the low SNR environments that you claim to be working with. As such, its unclear to me that you can actually develop meaningful correlative relationships, like Garnier, with a relatively low number of cases that the SNR would be sufficient and uncertainty suppressed. The uncertainty term presented appears to me to be a standard deviation, which again seems misrepresentative in context.

- Please see my note about how you interpreted Chew et al. (2011). Its not correct. 34% of Level 2 AERONET observations were found biased by unscreened cirrus.

I recognize that this is a lot of stuff. I offer this with full respect to what you are trying to do, because its in my direct interest working so many years with MPLNET to see this sort of work get published. I present these thoughts in detail with the sincere hope of helping resolve what I believe to be significant scientific shortcomings in the narrative

as it is. I wish you the best.

J. Campbell Monterey, CA USA

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2016-458/acp-2016-458-RC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-458, 2016.

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