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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.

Anonymous Referee #1

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The authors present cirrus statistics from a ground based Raman lidar. Retrievals are performed using only the elastic signal of the lidar. The authors use previous studies to speculate on physical reasons for seasonal and diurnal difference they report. Overall, the paper is well written, organized and easy to follow. However, the authors should be less aggressive when speculating on their statistics and reading into differences that are not statistically significant.

Much of the paper is spent discussing season and diurnal differences in the cirrus statistics. However, little attenuation is paid to whether these differences are actually statistically significant. Some figures/tables give the standard deviations, but little discussion of them is given in the text leaving the reader to determine significance themselves. In Table 2, it appears that non of the statistics differ significantly: i.e. one cannot say that the cirrus differ at all from season to season. Similarly in Figure 2, the box plot reveals that there is no significant seasonal cycle in frequency of occurrence either. In other figures where histograms are given, a statistical test should be applied to ensure difference among distributions are statistical significant before they are discussed. It is only appropriate/worthwhile to discuss differences that are statically significant.

I would also caution the authors against extrapolating too much from their relatively limited data. An example of this is using the lidar ratio to infer the ice crystal habit. The lidar ratio alone cannot be used to identify the ice crystal habit since it also depends on the particle orientation relative to the laser beam. In addition, theoretical studies of ice crystal phase functions vary wildly so there is no real consensus on what the lidar ratio even is for different ice crystals.

The authors note that this ground-based site is unique compared to others reported in previous work, yet rely heavily on previous work to explain their results. The paper would be greatly enhanced by making a more quantitative effort the explain their data. For example, instead of speculating on the sources of moisture for the cirrus in different seasons, a more convincing approach would be to run back trajectories to show the reader where the air came from.

Is there reason the authors don't use the nitrogen signal to retrieve extinction? Not doing so doesn't completely discount the data presented, but it does devalue it somewhat since this paper is just another in a long-line of elastic lidar cirrus studies. In addition, the transmission method is really only accurate for mid-range optical depths. Too thin and there isn't enough transmission signal to get a reliable optical depth. Too thick and there isn't enough molecular signal above the cloud. I encourage the authors to go beyond just checking the SNR above/below the cloud when doing the optical depth retrieval and to fully derive the uncertainty in the optical depth values they report. Figures 5 and 6 show optical depths down to 0.001, which I expect to be extremely uncertain when using the transmission method to retrieval optical depth.

The treatment and discussion of multiple scattering could be improved. Although, not explicitly stated, I'm guessing the authors use Eq. (10) from Chen et al. (2002) where eta depends on the optical depth of the cloud layer. I'd would encourage against using this equation. Chen et al. provide no physical justification for this equation and the values for larger optical depths quickly approach the wide angle scattering limit of eta=0.5 which is unrealistic for the geometry of a ground based lidar. In addition, for optical depth greater than about 1.2, eta<0.5 which is unphysical. The authors should also keep in mind that the shorter wavelength of 355nm (compared to 532nm as is used in Chen et al. 2002 and many other studies) means much stronger forward scattering and therefore larger amounts of multiple scattering. Typical extinction biases could range from 5-30% and sometimes even larger (see Thorsen and Fu, JTECH 2015 Fig. 13). I would suggest the authors make clear to the reader that their optical depth may contain significant biases due to multiple scattering unless some type of explicit treatment of multiple scattering is performed.

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