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Atmospheric Measurement Techniques Discussions



# *Interactive comment on* "Above-Cloud Aerosol Radiative Effects based on ORACLES 2016 and ORACLES 2017 Aircraft Experiments" *by* Sabrina P. Cochrane et al.

**Anonymous Referee #1**

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General comments

The authors focus on the calculation of the direct aerosol radiative effect over bright clouds using airplane campaign measurements. They propose a new methodology to derive the vertically resolved aerosol properties, minimizing horizontal cloud inhomogeneity. They identify a critical cloud albedo value and compare their findings with past studies. I enjoyed reading their work, because the subject is scientifically important, the presentation very clear and the scientific methods seem robust. It is a very good manuscript, which I find publishable with only minor changes. They are only secondary scientific points, whose resolution will not alter the findings of the study. Moreover, there are only a few technical corrections.

Specific comments

p. 2, ll. 10-11. I would write this as "... radiative effect occurs at an albedo value (critical albedo) just above 0.2 ..."

p. 3, l. 22. A couple of more recent works that might be inserted are: Oikawa, E., Nakajima, T., Winker, D., 2018. An evaluation of the shortwave direct aerosol radiative forcing using CALIOP and MODIS observations. J. Geophys. Res. Atmos. 123 (2), 1211–1233. Korras-Carraca, M. B., Pappas, V., Hatzianastassiou, N., Matsoukas, C., 2019. Global vertically resolved aerosol direct radiation effect from three years of CALIOP data using the FORTH radiation transfer model, Atmospher. Res., 224, 138-156

p. 4, l. 4. The reader gets the erroneous impression the the Kim et al. correction to CALIOP is unofficial. I would use the phrase "... until the development of a new method in version 4 to derive AOD ..."

p. 8, l. 7. "The nadir light collector is not actively leveled". Just for clarity reasons, please state if the upwelling flux is sensitive or not to the pitch and roll angles.

p. 10, ll. 21-24. Why not use a circular pattern with smaller, within the ALP limits, pitch and roll angles? Would the area covered be too large then?

p. 11, ll. 3-5. Surely the spatial variability is smaller with the spiral descent. However, only one albedo value is reported in Table 1. Shouldn't there be a range of albedos from all the upwelling-downwelling pairs?

Figure 2. I assume that all points in 2a,c are altitude-filtered, since the altitude filter is not mentioned in the color scheme. In that case, the caption of a) and c) could be "The latitude vs. altitude of altitude-filtered ..."

Figure 2. Where are in a) the purple dots between latitudes -16.65 and -16.70 as seen

in b)? Similarly, where are in c) the -8.9 latitude purple dots shown in d)? I cannot detect the correspondence between the points of a and b and between c and d. It would be better if in all figures, the start and end of the spiral were marked clearly.

Figure 4a. Unless I missed it in the text, the 470, 530, 660 nm data points are never explained. They probably belong to the 2016 case, but I am not sure. Also, which wavelength corresponds to the blue and red points?

p. 15, l. 5. If I understand it correctly, for the 2016 case H_infinity is less than 1.12 %, not less than 0.2 %. Please clarify.

p. 15, l. 20. $H\lambda$ is not defined rigorously, so we are not sure if $A\lambda=V\lambda+H\lambda$ or $A\lambda=V\lambda-H\lambda$. It is mentioned in the Appendix, however.

p. 15, ll. 25-26. Because of the non-rigorous definition of $H\lambda$, we just have to trust the authors here.

p. 17, l. 18. In the beginning I was confused by how different the profiles of AOD and extinction coefficient were in Figure 5. I then realized that of course AOD is the 4STAR column-integrated AOD down to that height, while the extinction is local. So I suggest that this line be changed to "... so that the column-integrated AOD profile decreases ...", just to remind the reader.

p. 17, l. 23. If I understand correctly, the extinction coefficient is derived from the 4STAR AOD data. Is it meaningful to compare the extinction coefficient with measurements from the HSRL-2 instrument? Such measurements exist for the 2016 case, don't they? Under the same light, where have the HSRL-2 data been used? Could the HRSL-2 be removed from the description altogether?

p. 18, l. 23. "... retrieval at 501 nm ...". In Fig. 6b the title is "380 nm".

p. 24, l. 12. Here as also in l. 8 of the previous page, the albedos given do not match the albedos of Table 2. Are we referring to the TOL sweep?

p. 48., l. 6. These derivatives come from Equations 13A and 8A, so it would be clearer if 13A were presented first.

Technical corrections

p. 22, l. 2. "... in Figure 8..." I think the authors mean Figure 7.

Figure A1. Equation 7 is mentioned, but it is irrelevant

p. 48, l. 1. "Figures 8a and 8b ..." probably should be Figures 7a and 7b

p. 48, l. 19. There is no Equation 16A. Generally, the A2 part of the Appendix should be reviewed and polished.

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