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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics of CALIOP attenuated backscatter noise: implication for cloud/aerosol detection" *by* D. L. Wu et al.

Anonymous Referee #1

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General comment:

The paper focuses on the CALIPSO lidar noise, calibration and sensitivity for discussing cloud/aerosol detection. It shows that different sources of noise from day and night as well as land and ocean have to be well characterized before geophysical interpretations of the data (eg. Thin cirrus cloud frequency day versus night, trend). Based on this and after a statistical analysis of the data, the author shows that a method based on standard deviation dependence should be implemented to improve the detection. Interesting solutions (first part) for improving the detection method are developed. They could be alternative solutions for fixing calibration problems but are unfortunately not used after. The comparisons between the detection algorithm from this analysis and the CALIPSO L2 should be performed on a comparable base (same

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averaging). Overall, I found this paper interesting but the points described below should be corrected before its publication.

Science issues :

1. α

The alpha coefficient used in this study to best-fit the molecular backscatter from GMAO to the CALIPSO backscatter profiles is an interesting way to show possible problems of calibration especially during daytime and at 1064 nm. However, the study should also focus on the 532 nm channel in the tropics (region of interest for cirrus clouds in the TTL), where the calibration of the nighttime channel can be impacted by the presence of stratospheric aerosols between 30-34 km. I suggest to add a plot which shows the variation with the latitude of alpha for a given period. I suggest also to keep α to the value determined by the algorithm instead of artificially forcing it to 1. A modification of the algorithm is also required to take in account the attenuation of CALIPSO backscatter profiles below clouds. This possibly could lead to underestimate α. After making those corrections, I would suggest to continue the analysis with α.

2. Comparisons with L2 CALIPSO product

Comparisons with the L2 CALIPSO product are done through the paper but by comparing different types of horizontal averaging. I suggest to make use of the flags available in the L2 CALIPSO product which indicates the horizontal average performed to detect a layer. Therefore, only 5 km averaging should be compared with this analysis.

3. Figures and captions

I found generally the figures interesting but the author should improve the captions to make them easiest to read and understand.

Details comments :

P17265 :

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L25 : However : ‘ The CLOUDSAT radar and CALIPSO lidar sensitivity are really different (eg. CALIPSO is sensitive to very thin cirrus clouds and not CLOUDSAT, CLOUDSAT can penetrate the insight of deep convective system but not CALIPSO). We have then different view of the same cloud, but complementary. Besides, the limb measurements from MLS compared to the nadir observations of CALIPSO and CLOUDSAT make those dataset even more difficult to compare since there are not observing the same features.

P17266 :

L14-L25 : This part is used as a motivation of this work, it should be explain better. Since the scheme for detecting clouds from the Level 2 CALIPSO data is based on different averaging (5 km, 20 km, 80 km), the author says that if a statistic is performed out of this, it will mixed spatial resolution and will be probably bias the results. However, the way that the CALIPSO level 2 (L2) 5 km cloud/aerosol product is done allow you to compute the frequence of occurrence of clouds at 5 km, since a flag is used to indicate with which horizontal averaging the detection were performed. If the author compute his statistics out of only the detection made over 5 km, the comparison between his product and the official product will be more consistent.

See : (http://eosweb.larc.nasa.gov/PRODOCS/calipso/Quality_Summaries/CALIOP_L2VFMProducts_2.01.html)

P17267 :

Enhancements of noise (ex. Daytime over land due to surface reflectance, or SAA) as well as calibration issue (eg. stratospheric aerosols in the calibration zone) will probably lead to underestimate the frequency of occurrence of clouds/aerosols. Even it is announced in p17267, L16, the problem of the calibration are enough explain in this paper.

P17268 :

L5. (‘version 2.0’. . .and version 2.02)

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P17269 :

L3 : At this time, a better explanation of the calibration is required (Powell et al., 2009). In fact, the calibrations of all the channels are based on the calibration of the night-time channel at 532 nm, assuming no aerosols between 30- 34 km. But even if this assumption is true at midlatitude, the presence of aerosols in the tropics can represent between 5 to 12 % of error on the calibration coefficient (Vernier et al, 2009).

The atmospheric background model does not exclude only the attenuation effect (not a calibration issue) of stratospheric aerosols which is most of the time less than 1-2% (Vernier et.,2009) but rather the attenuation effect of all ice clouds which make difficult the retrieval of clouds below. The citation in l8 is not appropriate. In Vaughan et al., 2009 a method is proposed to correct the attenuation below clouds that should be consider in this study.

BUT I found the iterative method interesting since it has the potential to detect bias due to calibration through the alpha coefficient. It should be performed by implanting a correction of the attenuation due to clouds. In fact the standard deviation approach will surely help you to detect and subtract clouds but probably not take out the attenuation effect below clouds. It would lead to underestimate the alpha coefficient, since low value due to attenuation will remain in the profile after subtracting all the features.

P17271 :

L 20 : The term 'best' in not appropriate here. Fig3. I would suggest to have look at the latitude cross section of alpha for January 2008 since the same period is used later on. It could be a good indicator of the problems of the nighttime calibration in the tropics. L19-21 : This is not exactly true : - CALIOP V 2.01 : - June 2006- August 2008 process with GEOS 5.01 - CALIOP V 2.02 : - September 2008 process with GEOS 5.01 - October 2008- February 2009 process with GEOS 5.20

This could explain maybe the two abrupt changes observed.

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Interactive Comment

P 17272 :

L 20-24 : I don't understand why alpha is put to 1. Since it seems to be a very good indicator of problems of calibration, then I recommend keeping this coefficient throughout the study and evaluating the difference between the L2 CALIOP detection features and the detection method developed here.

Figures :

Fig 1. The abbreviation 'N' for night and 'D' for day should appear bigger at the middle of the figure and not in the x absciss. A legend should be add to say what represent '+' the data and the black line (GMAO model) . The comment : 'the backscatter is noiser. . .' should be put in the text.

Fig.2 A legend is required here (-black:day, grey :night) , the x absciss caption should be put in black when it corresponds to the nighttime orbit and grey for daytime. It would really improve and make easy the lecture of the plot.

Fig.3 Legend also. The sentence:'there is a little variability. . .' should be put in the text. I suggest here to make for January 2008 (month used later on) a latitudinal plot of alpha for studying the problem of the nighttime calibration channel in the tropics that could impact the detection of very thin cirrus clouds or aerosols at the tropopause. The evolution of alpha between 10N-10S could also be plotted.

Fig.4. Letter D for day and N for night should be bigger

Fig.5. Same remark

Fig.6-8. Those figures are interesting but required more explanations in the text to be understood. The legend can be improved by writing what represents the lines. Fig.9. From here, the α coefficient should be kept and not artificially put equal to 1. I suggest also plotting here the cloud/aerosols detected by the L2 CALIPSO data only with 5 km horizontal average, since a flag is available to distinguish between 5, 20 or 80 km. It will make more sense to compare it with your method of detection based on the same

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horizontal average.

Fig.10. What do you mean by 'the overlapping portions in the L2 data are removed'. I suggest here also to use only the 5 km average from CALIPSO L2. Captions on colorbars should be included.

Fig.11. same remark than fig.10

Fig.12. The legend should include the grey and black continuous line (color is maybe required ?). The description of the figure (increase) shoud be included in the text.

Fig.13. ok

Fig.14. Legend (grey: L2 data, black : this analysis).

Fig.25. One figure should be enough to characterize and explain why the sub-graph start after the noisy daytime part of the pdf. For the other graphs, I suggest to keep only the relative difference of the pdf (day-night), since it is discussed in the text but the sub-figures are too small.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17263, 2010.