

Interactive comment on “Seasonal and spatial variations in aerosol vertical distribution and optical properties over China from long-term satellite and groundbased remote sensing” by Pengfei Tian et al.

Anonymous Referee #2

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This paper describes aerosol climatology over China using CALIPSO/CALIOP. The method used in this paper is rather simple using CALIPO version 3 level 2 data, but the results are interesting and merit publishing in ACP. The paper is generally well-written. However, some of descriptions are not correct or not reasonable. Especially, previous works are not properly reviewed and some of the references are not original and not suitable.

Specific comments P 2 First paragraph: In my opinion, It is not appropriate to cite too many papers with a single simple statement. In the first sentence, citing Boucher et al.,

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is reasonable, but it is not clear at all why He et al. and Peng et al. are cited here. The same thing for the following sentences.

P9 I.9 The sentence “The retrieved aerosol extinction coefficients suffer from large uncertainties” is miss leading. What about CALIOP level 2 data? Similar layer type classification and retrieval method using variable lidar ratio can be used for ground-based lidars. It is fine that the AOD-constrained retrieval method is used in this paper. But that is not clearly mentioned. That should be mentioned also in the caption of Fig. 2.

P9 I.12: It is not Huang et al. who first introduced the AOD-constrained Fernald method. The method was used already in 1994, for example, in Takamura et al, Appl. Opt. 33 (30) 7132-7140 (1994). If the AOD-constrained method was employed, it would be useful to present a histogram of the derived lidar ratio value.

P12, I.19: The volume depolarization ratio includes molecular scattering contribution. The discussion is consequently not very quantitative (though it is still useful). The definition in Eq. (4) is fine, and the contribution in the lower height is dominant. So the contribution of molecular scattering is probably not significant. The situation should be mentioned.

Figure 8: Definition of height should be provided. The profile with a large secondary peak in PRD MAM seems unusual as a climatological profile. What is the number of profiles averaged in this profile? The number of the data used and the error bars of the profiles should be presented. If the secondary peak is real, the source of aerosols in the secondary peak must be discussed. The descriptions in p. 18, l. 2-3 do not explain the cause of the secondary peak. Fan et al. paper is on the meteorological condition on October, not MAM. As to the vertical profile in Guangzhou, the following paper should be cited. It describes non-dust aerosol climatology in Beijing and Guangzhou using ground-based lidars and CALIOP. Hara et al., (2011) “Seasonal Characteristics of Spherical Aerosol Distribution in Eastern Asia: Integrated

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Analysis Using Ground/Space-Based Lidars and a Chemical Transport Model” Scientific Online Letter on the Atmosphere, Vol. 7, 121–124, doi:10.2151/sola.2011-031 (https://www.jstage.jst.go.jp/article/sola/7/0/7_0_121/_article/)

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-749, 2016.

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