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Atmospheric Chemistry and Physics Discussions

Interactive comment on "EARLINET evaluation of the CATS L2 aerosol backscatter coefficient product" *by* Emmanouil Proestakis et al.

Anonymous Referee #1

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General Comments: In this study the authors are presenting the coordinated effort of the European Aerosol Research Lidar Network (EARLINET), to evaluate the Level 2 aerosol backscatter coefficient product derived by the space borne backscatter lidar namely Cloud-Aerosol Transport System (CATS). The manuscript is well written and has a scientific merit. Therefore, in my opinion it worth being published under the special issue "EARLINET aerosol profiling: contributions to atmospheric and climate research" of the Atmospheric Chemistry and Physics journal. However, in order to help improving the manuscript, I would kindly suggest the authors to take into account the following specific comments. Specific Comments: 1. Abstract: Page 2, line 1: "Independently of daytime/nighttime conditions.". Please consider revising this statement. At the end of this paragraph the authors are mentioning an underestimation of 22.3%

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during day and 6.1% during night time. So there is a significant difference in the comparison based on the sky light conditions something that has to be mentioned clearly in the abstract. Where you can attribute this difference? e.g. SNR issue, significance of your day-night statistical sample? 2. Introduction: The Introduction is well written however I am missing the scientific question that this manuscript envisages to answer. Please try to make this clear in this section and consider mentioning the achievements and progress of the scientific community so far towards this topic. Are there any similar activities for CATS? The results presented here are having great difference with similar studies for other space borne lidars? The reader has to reach section 2.1 in order to find some answers on the aforementioned concerns. 3. Section 2.3.1: I think it would be beneficial for the manuscript to include a flowchart showing the methodology of the comparison followed by the authors. The entire process can be summarized there along with the methodology requirements followed by the authors. e.g. the spatial - temporal constraints, cloud screening requirements, etc. The information exists in the manuscript but I feel like it is scattered among the sections. 4. Page 7, lines 18-19: "... is less than 30%, ... requirements of EARLINET". The authors are kindly requested to provide a reference for this statement. 5. Page 8, line 8: "scattering respectively"-> "backscattering respectively" 6. Section 2.3.3: This section is important for following up the manuscript and has to be highlighted. Therefore, I would kindly suggest to the authors to list it as 2.4. 7. Page 9, line 6: "The discussed constraints...": How much these constrains affect the final dataset (in terms of number of measurements and overall evaluation)? 8. Page 9, line 18: "here considered"-> "considered here" 9. Page 9, lines 32-33: I cannot understand this conclusive statement. How "the absence of significant biases, both daytime and nighttime" is obvious from figure 3c. 10. Page 10, lines 9-10: "due to the different SNR...": I think that indeed this is the case. But this contradicts to the author statement of no significant bias between day and night conditions stated earlier (page 9, lines 32-33). 11. Page 10, lines 24-29: I have the feeling that this information should be moved to section 2.2 where the description of CATS data level product is already given. At that section, the authors can present a

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detailed description of their methodology followed for could screening. 12. Page 11, line 23: “end of 2018. . .” -> Maybe “end of 2019” ? 13. Section 3.2: I wonder why the authors constrained their study only to the comparison of aerosol backscatter and they did not proceed with comparison of other aerosol related properties as well (e.g. physical and not properties such as integrated backscatter, AOD, lidar ratio, layer center of mass-thickness). I have the feeling that by taking into account more properties in their comparison will improve the manuscript and will enhance the arguments (i.e. argument of tenuous layer, argument of lidar ratio assumption) for the discrepancies shown here. In addition to that the information provided by each station individually is lost in the analysis demonstrated here. For example, a figure showing the differences between CATS-EARLINET for day and night time conditions per station along with the mean value may explain some of the discrepancies shown in this section (e.g. the argument of topography) or it may reveal other discrepancy patterns if any (i.e. latitudinal). 14. The pair of observation "i" refer to the vertical height of each case study or to each case study individually? This a general comment related to the comparison methodology followed by the authors: I speculate that the initial vertical resolution of the two profiles is not the same. For example, the L1 data products obtained by CATS are within 60 m vertical resolution (Yorks et al., 2011). On the other hand, the data products obtained by EARLINET (especially the Raman retrievals) are processed (application of low-pass filter on the signal) leading to range-resolution loss. A concept of effective resolution is already discussed in the literature (e.g. Iarlori et al., 2015). Therefore, it is not so clear to the reader how the authors managed to compare values obtained from different atmospheric heights? Did they interpolate their values or they used mean values in specific vertical height windows? In any case the authors are kindly suggested to comment their approach on this. (Iarlori, M., Madonna, F., Rizi, V., Trickl, T., Amodeo, A., Effective resolution concepts for lidar observations, Atmos. Meas. Tech., 8, 5157–5176, 2015 www.atmos-meas-tech.net/8/5157/2015/ doi:10.5194/amt-8-5157-2015) 15. Page 13, line 30: “CALIOP” -> Maybe “CATS” instead of CALIOP? 16. Page 15, line 18, lines 24-25: “slight underestimations of the

total AOD in climatic studies." "results in large AOD biases and unrealistic AOD values." I agree with these statements. However, in the current state of the manuscript there is no straight forward comparison of AOD but only backscatter coefficient. See also my previous specific comment No. 14. 17. Page 29, line 13: "The white circle" -> "The white dot denotes the location". The white circle refers to points at various distances from the lidar station as stated by the authors in Figure 2. Please consider correcting this minor typo in figures 3, 4, and 5. 18. Figure 7: For the night time mean profiles the discrepancies are negligible but for the day time and specifically for the height region from 1-2 km large differences are observed. What is the main reason behind this? The significant influence of the topography? In that case why this difference is not shown also in the nigh-time profiles, considering this as a bias from one or more stations. The low daytime CATS SNR? In that case I would expect to see higher discrepancies than sown inside the PBL (longer atmospheric path), compared to 1-2 km. The calibration region of CATS? In any case, I think that a solid and quantitative explanation on this is missing.

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