Review of "CALIPSO retrieval of instantaneous faint aerosols" by Mao et al.

The authors present a methodology for converting CALIPSO L1 data to a 20 km averaged backscatter coefficient at 532 nm (k_{532}) and compare that product to the L3 CALIPSO extinction coefficient product. The stark difference between the 2 products is the resolution: the L3 product is averaged over a month time period and averaged over 5°x 20°(latitude x longitude). The authors also compare their product to the SAGE III/ISS extinction coefficient at 520 nm (k_{520}) for daytime and nighttime CALIPSO observations. Their k_{532} product agreed better with SAGE when the CALIPSO data were collected during the night and performed substantially worse for daytime observations. Overall, per their Fig. 5, it appears that the agreement between the 2 instruments is good.

While I believe there may be value in this methodology, the authors do not present a convincing case. Overall, the paper is poorly written in both organization and especially detail. In its current state it is challenging to understand what the authors did (and why) and the methodological details are insufficient to reproduce the work. Further, the authors do not make any attempt to explain how this methodology is different from previous work (they even cited these previous studies so it should be straight forward for them to explain how their method is different). They simply state that their product is below the 0.01 km^{-1} LOD threshold for the previously-published CALIPSO L2 products. However, I have to question whether this is a fair comparison (i.e., their L1-based product with the L2 product). I think that, as written, the paper does not present a novel approach that will produce a scientifically interesting product and there are some substantial technical issues at play as well. Finally, the authors list 3 main conclusions from this study. However, for reasons listed below, I find all 3 conclusions to be tenuous (or demonstrably wrong), which leave me questioning the scientific merit of this study. For these reasons, and those enumerated below, I cannot recommend this paper for publication at this time.

1 Major Issues

The following issues should be addressed prior to resubmission.

1. The paper requires major revisions in writing. Many sentences do not make sense, some references do not make sense within the context of the sentence, and some references do not support what the authors claim.

2. The authors do not make it clear how their method is different from previous work, which leaves me questioning the scientific merit of this work.

3. This work is ultimately a statistical analysis and the authors are looking for faint signal that, per their paper, is below the previously published limit of detection (LOD) for CALIPSO. Their retrieval requires an iterative process but do not attempt to propagate uncertainties through this process. Therefore, we have no was of determining whether their end product is significantly different from zero.

4. One has to wonder how valid a comparison between an instantaneous product and a monthly average is (i.e., the authors L1 product and the standard CALIPSO L3 extinction product)

per their Figure 7. Further, the faint aerosol layer they identify is at ≈17.5 km and they showed the L3 product at 15.2 km. The validity of this comparison is tenuous even when the altitude match, and much less so when the altitudes do not match.

5. It seems the authors are unaware of the 2019 Raikoke eruption, which is likely the source of aerosol in their Figs. 3, 4, 7. Instead, they attribute these layers to a single wildfire event in Siberia without providing any supportive evidence. The papers they cite in support of this claim do not, in fact, support their assertion. Ohneiser et al. 2021 claimed to see smoke up to 13 km from 50°N to 80°N. Kostrykin et al. 2021 did a modeling study wherein they claimed to trace smoke up to ≈ 6 km. The authors claim to see smoke from 10°N to 60°N at 15–20 km. This is in stark contrast to the finding of the 2 referenced works. The authors either need to support their assertion that smoke was present, and responsible for the faint aerosol layer they observed, or reconsider the source.

6. Section 3.1: The authors claim these faint aerosol layers are the product of wildfire events (i.e., smoke). Smoke has a very different lidar ratio from 50 sr^{-1}. Did the authors use a lidar ratio of 50 sr^{-1} here? If so, how does this impact the interpretation of the results (e.g., do the faint aerosol layers disappear if the correct lidar ratio is used)?

2 Minor Issues

The following issues should be addressed prior to resubmission.

1. The authors perform their coefficient correlation and RMSE calculations in logarithmic space. Why use logarithmic space when this effectively reduces the comparison to essentially how well the orders of magnitude agree.

2. How is "faint aerosol" defined? What qualifies as a "faint aerosol"?

3. On lines 81–83 the sentence containing "...observed by the rays passing through the atmosphere..." does not make sense and this is not how SAGE quantifies extinction coefficient. I think, as written, this sentence is incorrect. Light passes through the atmosphere and is attenuated by some combination of scattering and absorption of molecules, particles, and clouds. The molecular number densities and extinction coefficients are then derived based on the recorded spectra.

4. Throughout the paper the authors refer to SAGE extinction, but do not specify wavelength. They stated within the text that they used the 521 nm channel, but it would help the reader if the wavelength is included in all subsequent references to SAGE extinction.

5. The authors used the SAGE III/ISS v5.1 product while the v5.2 product is available. Vertical smoothing for the extinction products was turned off for the v5.2 product, which effectively provides data at a higher vertical resolution. The authors are encouraged to use this data set since the higher resolution will make their comparisons more meaningful.

6. Line 95 What is the "vertical moving filtering" that was done? What kind of filter is this and what is it removing?

7. Not all of the variables in equations 1-3 are defined within the text (some are defined within a figure, some within a figure caption, some within the text). Please define all variables within the text.

8. Lines 120–121: Please provide reference for $\beta(0) = 0, T_p(0) = 1$.

9. Line 124: What is meant by "same day"? Is this the same calendar date, or within 24 hours?

10. Line 125: You state the SAGE horizontal resolution is ≈300 km; please provide a reference for this value. This raises a greater point: the viewing geometries and sampling volumes of SAGE and CALIPSO are vastly different and may provide another source of uncertainty.

11. Line 126: The reference to Adams et al. 2013 does not make sense here. Please include why this is relevant or remove the reference.

12. Line 127: What is meant by "...where the CALIPSO orbit exceeds 0.75°latitude..."? What is meant by 0.75°? This is confusing, please clarify.

13. Line 144: The authors refer to a "red dashed box" in Fig. 3 (a), but there is no such box in that figure. Please add the box.

14. Lines 148–150: One has to ask, if these faint aerosol layers are visible within the ASR product, then why go to the trouble of calculating extinction coefficient? It seems that computing extinction coefficient introduces unnecessary uncertainties, so my 2 questions are: 1. why use the extinction coefficient?, 2. are the uncertainties that the extinction calculation introduced acceptable?

15. Line 156: It seems the authors are introducing an additional step in their processing algorithm (i.e., the 3x3 mean filtering window). This should be included in the main body of the text and explain what this is.

16. Line 163: Perhaps the pdf did not build properly, but there is no red dash line in Fig. 4 (a) though there is a pink line.

17. Figure 4: What is the vertical pink dashed line?

18. Line 172 and Figure 5 (a): How were these averages calculated? There are 11 points on this line, but the SAGE data exists over a continuum, so there must have been some binning along the x-axis

19. Line 172 and Figure 5 (a): The authors refer to this as "faint aerosol extinction", but does this really qualify as "faint" when the SAGE extinction coefficients exceed 1E-3 (even 1E-2!)?

20. Line 173: The authors claim the CALIPSO retrieval has a low bias while their line of best fit falls just above the 1:1 line, indicating the CALIPSO retrieval has a high bias. Can the authors explain what they mean by low bias and justify this designation?

21. Line 184: What is meant by "mean values of 5% quantiles"? It seems the authors have introduced another calculation that is not defined/explained within the text and is not readily understood by the reader. This should be explained within the text.

22. Line 193: The authors state "...the CALIPSO retrievals in the troposphere should be more reliable [than SAGE]" While I agree about this statement to some degree, this is a blanket statement that is made without qualifier and is misleading. The authors need to explain what they mean here. Further, there are nuances that are important. In the troposphere you can have highly variable lidar ratios; how does this influence the "reliability"? You have to propagate your errors all the way through the stratosphere before hitting the troposphere; how does this influence reliability? Finally, the troposphere is often loaded with aerosol (as compared to the stratosphere), so how often do you expect to be below the 0.01 km^{-1} LOD (i.e., is you method still necessary at this point)? It seems like the discussion regarding the troposphere is misplaced within this paper.

23. Line 201: What is the purpose of the Kim et al 2017 reference here?

24. Section 3.3: The authors attribute the faint aerosol during this time period to the Siberian wildfires. As discussed above this is more likely the impact of the Raikoke eruption. It is strange that this was not mentioned here. Please see discussion above regarding this topic and address the issues stated therein.

25. Figure 7: It will help the reader if panels (a), (b), (c), and (d) all have the same colorbar scale and limits.

26. Line 214: The Kostrykin reference in not germane to this paper as they showed no indication of the Siberian wildfire smoke leaving the troposphere. They estimated a 5% chance of smoke making it to 6 km.

27. Line 224: "...classified as elevated smoke by VFM). This is categorically wrong. The CALIPSO vertical feature mask identifies this plume as "sulfate/other" **not** smoke.

28. Lines 227–228 and Figure 7: It is difficult to compare a monthly average and an "instantaneous" measurement. However, the layer referenced in Fig. 7 (d) is not at the same altitude as what is shown in Fig. 7 (b). Going off Fig. 7 (b) alone, I would expect little (no?) enhancement at 15.2 km for most of the the NH. If we accept Fig. 7 in its current state we cannot draw a decisive conclusion (panels (b) and (d) contradict each other). However, if panels (a, b) were updated to mate the altitude of the layer in (d) this would significantly bolster your case.

29. Line2 242–246, conclusion #1: As stated above, some of your fundamental assumptions will be different within the troposphere. Further, the discussion of tropospheric aerosols does not seem to fit within the context of this paper. Further, the authors failed to demonstrate that this method is novel from previous work. Therefore I find this conclusion to be tenuous.

30. Lines 247–248, conclusion #2: While the authors demonstrated this they were not the first to determine this so I do not view this as a significant finding/conclusion to be drawn from this paper.

31. Lines 249–252, conclusion #3: While the authors demonstrated a faint aerosol layer that extended from 10°N to 60°N (at 20 km) the wrongly classify this as smoke. Further, they claimed the corresponding k_{532} was significantly below the CALIPSO L2 LOD (LOD is 0.01 km^{-1}, their aerosol layer was $\approx$0.003 km^{-1}) and they failed to account for the propagation of error in their extinction coefficient retrieval. Therefore, we do not know if the layer

they identified is significantly different from background. While I suspect this layer is real, it is difficult to interpret the significance of this conclusion without having this thorough statistical support.

3 Wording Issues

The following issues should be addressed prior to resubmission.

1. Line 13: " the susceptibility of clouds to aerosols is more pronounced when the aerosols are faint." This sentence does not make sense. Please clarify meaning.

2. Line 39: What is the "undetected tenuous aerosol layer"? This needs defined.

3. Line 48: The word "tenuous" does not make sense here. Please rephrase.

4. Lines 60–63: This sentence does not make sense. Are the "faint aerosols" composed of both background AND undetected aerosol layers? If so, what does it mean for a particle to be composed of undetected aerosol layers?

5. Lines 83–84: You discuss the extinction coefficient uncertainties of SAGE III/ISS and cite Thomason et al. 2010. The Thomason paper is in reference to SAGE III/METEOR (or SAGE III/M3M). Please indicate that this reference is for SAGE III/M3M and not SAGE III/ISS.

6. Please update all "SAGE III-ISS" to "SAGE III/ISS".

7. I had to read the first paragraph of section 2.3 several times before I understood your methodology. It will help the reader if you change the last sentence of this paragraph to: "To improve the signal-to noise ratio (SNR) of the total attenuated backscatter (TAB) data, we performed the following pre-processing steps prior to running the data through the algorithm presented in Fig. 1:"

8. Line 102: "particle particulate multiple scattering factor" is called "multiple scattering coefficient" at other places in the manuscript. Please make this consistent throughout.

9. Line 124: "diurnal variation of faint aerosols in SAGE III-ISS data" does not make sense. Perhaps you meant "...dirunal variation of faint aerosol within the stratosphere"?

10. Figure 5 caption: Much of this caption is difficult to understand and should be rewritten for clarity. Some information should be removed and put in the text and the authors should organize the caption in such a way as to explain what each panel is in a successive manner. In its current state the caption explains panels (a), (b), (c), then returns to discussion panel (a) without telling the reader what is going on.

11. Line 190: This sentence ("The number of matched points...") does not make sense. Please reword this to better communicate what you did.

12. Lines 194–195: This last sentence does not make sense (what is un-performed?). Please rewrite this to better communicate what you intend.

13. Line 210: Please include the year (e.g., June and August 2019).

14. Lines 229–230: This sentence is difficult to understand. What is a "propagation trajectory" and how can you estimate that based on a single CALIPSO granule? Please rewrite this sentence to better explain what you mean.

15. Line 239: Perhaps the authors meant "...retrieved instantaneous extinction coefficients for faint aerosol layers based on..."?