Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-212

Anonymous Referee #2

Referee comment on "Optimization of Aeolus' aerosol optical properties by maximum-likelihood estimation" by Frithjof Ehlers et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-212-RC2, 2021

The authors propose an alternative extinction coefficient retrieval methodology for the ESA's Aeolus ALADIN instrument that performs better compared to the current Standard Correct Algorithm (SCA). The authors formulated the retrieval problem as a maximum likelihood estimation (MLE) problem in which the ALADIN HSRL forward models are incorporated. The benefit of the MLE methodology is that it allows for constraining the physical values of the parameters that are being estimated (e.g. backscatter coefficient and lidar ratio).

The reviewer is disappointed that the authors did not expand on the work of Xiao et al. 2020 and Marais et al. 2016 who both introduced MLE methods that employ the total variation (TV) image smoothness constraint to improve the inference of the extinction coefficient for Poisson noisy observations. The publication would have been much better if the authors at least applied the MLE TV method on spaceborne lidar data. There are readily available software implementations of the MLE TV method, one of which is called SPIRAL and it is available at http://drz.ac/code; the authors will be required to make modification to the SPIRAL code, but that should be within the expertise of the authors.

The reviewer, however, is recommending a minor revision since the authors are improving the state of the art specific to the Aeolus ALADIN instrument measurements.

Major comments:

Lines 81 to 83: In the references that are cited, the work were specific to developing inversion methods for ground based lidar instrument. The authors are proposing a spaceborne inversion HSRL method. This distinction matters because for ground based lidar instruments inaccuracies in the geometric overlap function calibration parameter can introduce biases in the extinction coefficient. Hence, the backscatter coefficient is inferred as a separate first step to isolate any geometric overlap function induced biases to the extinction coefficient. With spaceborne HSRL measurements and depending on the instrument, there is not necessarily a geometric overlap function which are confirmed by equations 2 and 3 in this paper. Thus, a spaceborne HSRL allows for the simultaneous inference of the backscatter and extinction coefficients that are not biased to due to the geometric overlap function.

For the sake of understanding how this paper's method relates to the references that are cited, the reviewer suggests that the authors mention that the geometric overlap function calibration parameter is a limiting factor for ground-based HSRL instrument when trying to simultaneously infer both the backscatter and extinction coefficients.

Line 198: It is unclear where in (Flamant et al 2017) equations 9 and 10 are defined. Since equations 9 and 10 are esoteric, it would be helpful for the reader to know where in (Flamant et al 2017) equations 9 and 10 are defined since (Flamant et al 2017) has 124 pages.

Lines 199-200: Equations 9 and 10 do not make sense. For example, consider equation 9. Since $L_{p,i} > 0$, we have that $-2L_{p,i} < 0$. Hence, $X_{i} < 0$, since the rest of the terms in equation 9 are positive. However, this a contradiction, since X(r) in equation 5 is strictly positive.

Line 224: Should J be J_{obs}? Should S_y not be the inverse of the measurement error covariance matrix? Refer to equations 2.21 and 5.3 of (Rodgers, 2000).

Line 287: The reviewer does not agree with the statement "MLE estimates usually suffer from overfitting and noise amplification.", because it is the forward model along with the MLE that determines whether there is overfitting or "noise amplification". E.g., the averaging operator (or just averaging) can be derived via MLE and averaging operator with a sufficient number of samples does not overfit. In other words, whether a MLE overfits or does "noise amplification" depends on the MLE's parameterization with the forward model.

Line 230 to 232: Could the authors provide a reference to support the claim that the averaged signal's noise can be approximated via a Gaussian distribution? For example, are there sufficient photon counts during the nighttime at 30km altitude that are accumulated and normalized in order for the average signal's noise (at 30km) to be accurately approximated by a Gaussian distribution?

Lines 259 to 261: Poisson noise is not additive and therefore it is confusing to read $\hat{sigma}_{s} = \sqrt{s + \epsilon_{s}}$. For Poisson noisy observations the noise standard deviation is $\sqrt{s + b_s + b_d}$ where b_s is the solar background radiation and b_d is the dark counts.

Lines 291 to 292: The statement "the estimate should fit as close as possible to the signal data and only solve the physical contradictions" is contradictory compared to the previous sentences in this paragraph. If the estimate should fit as close as possible to the observations (signal data), then why not let the L-BFGS-B converge to a solution and why bother introduce an early stopping criteria of 40 000 iterations? In other words, in this sentence the authors are suggesting that the estimator should "overfit".

Lines 431 to 433: Please add a reference that shows the range of lidar ratio values for desert dust at wavelength 355nm.

Lines 457 to 460: Are the authors reporting co-polarized lidar ratios or BERs? If BERs are reported, how were the co-polarized lidar ratios converted to BERs?

Sections 4.1 and 4.2: The authors should validate the uncertainty quantification as described between lines 300 and 305, since it is unclear how accurate the proposed uncertainty quantification is for observational data.

Section 4 & conclusion: The authors do not thoroughly explain why SCA methods produce biased results compared to the MLE method, and it will be insightful know why SCA methods produce biased results. Could it be that the X and Y (equations 7 and 8) are modeled by SCA (equations 9 and 10) could introduces biases in the estimates of the backscatter and extinction coefficients relative to the MLE method, since the MLE method employs equations 7 and 8?

Comment about the methods: It will also be useful know what is the performance of a method that directly algebraically solve for the lidar ratio and backscatter coefficient via equations 7 and 8. In other words, what is the performance of the MLE method without constraints? The reason why this comparison will be useful, is to gain an understanding of the low performance of the SCA methods. Specifically, is the loss of performance due to the formulation in equations 9 and 10, or because of the lack of physical constraints?

Minor comments:

Line 2: In the first paragraph of the manuscript introduction all the letters of Aladin is capitalized. To be consistent, Aladin should be all in capital letters.

Line 5: "Being and HSRL" should be "Being an HSRL".

Line 6: Backscatter coefficients and lidar ratios are normally reported without polarization.

Line 6: It will be helpful to the reader to know that ALADIN does not make cross-polariation measurements (lines 107 & 108) and therefore ALADIN is not able to make direct lidar ratio measurements.

Line 8: Do the authors mean that the inversion problem is statistically ill-posed? If the inversion problem is ill-posed, then it would not be possible to infer the extinction coefficients from ALADIN measurements without using lidar ratios.

Line 33: It is unclear what the authors mean by "because high uncertainties in climate change modelling regard the indirect effect of aerosols on clouds and anthropogenic radiative forcing (Illingworth et al., 2015).". Are the authors saying there are high uncertainties of indirect effects of aerosols on clouds in climate change modelling?

Line 41-42: Lower SNRs of what? This part of the sentence is vague.

Line 47: Do the authors mean that the inversion problem is statistically ill-posed? If the inversion problem is ill-posed, then it would not be possible to infer the extinction coefficients from ALADIN measurements without using lidar ratios.

Line 69: Fine resolution of what? Image resolution?

Line 75: "reformulated as a " would read better than "rephrased into a".

Line 133: Do the authors mean detector random errors instead of wind random errors?

Line 135: Is it unclear what is meant by Basic Repeat Cycle (BRC) or Observation. Are these terminologies that are used in the field of wind lidar? If so, would it be helpful to add a reference? Does BRC mean that every 30 consecutive vertical profiles are accumulated?

Line 148: "is used as input to the optical" should be "is used as input by the optical".

Line 162: The reviewer presume that the measurement geometry is the so called geometric overlap function. It is unclear why the authors define O(r) which it is not used in equations 2 and 3?

Line 163: The range of T(r) is defined but not for \beta(r). To be consistent it will be helpful to also define the range for \beta(r) e.g. \beta(r) > 0.

Line 168: If the ALADIN instrument is measuring the co-polarized backscattered energy, shouldn't the molecular backscatter coefficient also be labeled as co-polarized?

Line 180: Using the word excessive sounds negative; it would be best not to use this word.

Line 190: "When" should be "where".

Line 203: What is 'this' referring to? Is 'this' referring to L_{p,sat}?

Line 205: Did the authors mean to say "or" instead of "/"?

Line 221: It is unclear what the "It" refers tp. Does the "It" refer to the second term in equation 12?

Line 222: Should J be J_{obs}?

Line 225: In regards to S_y, refer to comment of line 224.

Line 226: Should "Lidar" be "lidar"?

Line 234: In regards to S_y, refer to comment of line 224.

Lines 236 to 245: The paragraph is superfluous since 1) the authors are not employing a constraint or penalty term and 2) the paragraph does not add value to the current text. If the authors at a later stage employ a constraint or penalty term and report results in a next publication, the next publication can include this paragraph. Furthermore, the conclusion does discuss employing a regularization term.

The reviewer suggests replacing this paragraph with one line saying that equation 14 is applied on each averaged vertical profile of measurements.

Line 252: "would show off diagonal" should be "would have off diagonal".

Line 256: The last sentence on this line is unclear. To what does "following" refer to?

Lines 263 to 264: Are the 2.9km and 87km numbers referring to different horizontal resolutions?

Line 268: The citation style of Wandinger at at. 2015 is not consitent with the citation style of Illingworth et al. 2015.

Line 288: It is unclear why new sentence starts with "But".

Lines 306 to 313: Large portions of sub-section 3.3 are repeated in the first paragraph of section 4. Therefore this sub-section can be removed.

Line 362: It is unclear what the authors mean by "wither".

Line 382: It is unclear what the authors mean by "wither".
Figures 2 and 3: For the curtain plots; what is the horizontal axis? Profle number? Seconds?

Line 427: Can the authors elobrate on how the cross-polarized lidar ratio is transformed into BER (the actual lidar ratio)?

Line 434: More robust compared to what?

Line 473 to 475: See the comments of lines 81 to 83.