Response to Reviewer 3.

Thank you for your careful reading and valuable comments. We appreciate the time and effort you put into reading and improving our paper. Responses to specific comments appear below in blue.

Reviewer 3 writes:

> Review of "Aerosol extinction and backscatter optimal estimation retrieval for high spectral resolution lidar" by S. P. Burton, J. W. Hair, C. A. Hostetler, M. A. Fenn, J. A. Smith, and R. A. Ferrare, proposed for publication in Atmospheric Measurement Techniques.

In this article, the authors present a method based on optimal estimation to retrieve lidar-based optical parameters (particulate backscatter, extinction, lidar ratio, depolarization ratio) in aerosols based on HSRL measurements in 3 channels (cross-polarized, and co-polarized Mie-dominated and Rayleigh-dominated). They compare results from that approach to those obtained using the more widely used analytical approach. They consider a theoretical case study, and actual measurements from the NASA Langley airborne HSRL instrument in a scene that mixes different aerosol layers. They discuss the advantages of the optimal estimation approach for each retrieval parameter, and introduce some of its peculiarities, like the effective vertical resolution.

The subject matter is new, interesting and valuable for lidar experts, especially now that HSRL measurements from space are widely available. The writing is clear and effective. The methodology is well-supported by appropriate references. I appreciate that the authors took the time to remind readers of the basics, and enlightening comments can be found throughout the manuscript. Figures are well-designed, most convey a clear and useful message with evidence that moves the discussion forward. The results show that the method proposed by the authors brings significant improvements to retrievals of aerosol extinction compared to the analytical approach. Even though the paper could very well be published as-is, I have minor comments that mainly hope to make the paper slightly more approachable to non-specialists.

Minor Comments

1. Figs. 1, 2, 3, 6, 7, 8, 12, 14, 15, 16, 17 : please add units for parameters when relevant (backscatter, lidar ratio, extinction, altitude)
Units have been added to Figures 1-3, 6, 12, and 14-15 for the revision. Figures 7,8, 16, and 17 have been removed, as suggested below and by another reviewer.

1. p. 6, L.15: "the backscatter and depolarization are each found from the ratio of channels" - how is the backscatter obtained from the ratio of channels? I could not find how this aligns with eq 4 in Hair 2008 or any equation in Burton 2018. As I understand it the backscatter is directly proportional either to the Pm (for molecular backscatter) or Pp (for particulate backscatter) channels, no ratio here.

Yes, aerosol backscatter is proportional to P_p and molecular backscatter is proportional to P_m, but in both cases, there is the unknown molecular attenuation

T^2. In the HSRL method, the two channels are accurately calibrated relative to each other (i.e. the ratio g_m/g_p is known), so taking the ratio of the two channels allows the T^2 factor to drop out, leaving the ratio of aerosol to molecular backscatter (plus one). The molecular backscatter is furthermore known (with small uncertainty) from an externally provided profile of molecular number density, so the aerosol backscatter can be solved from there simply by subtracting one from the ratio and multiplying that by the molecular backscatter. Equation 3 in Burton et al 2018 shows the simplest form of this. Equation 4 in Hair et al 2008 appears a little bit more complicated because all three channels are considered (including the perpendicular channel that allows for solving for depolarization), but the idea is the same.

2. Section 3 is huge (17 pages out of 28, not counting the references). Splitting 3.1 and 3.2 into their own sections could help make things more balanced.

Good suggestion and easy to implement. This has been done in the revision.

3. The authors introduce in Section 3.1.3 the "effective resolution", which is different from the grid resolution and is given by the inverse of the degree of freedom. From the text I understand that having a different effective resolution for each data point along the vertical profile is a consequence of the optimal estimation approach considering entire profiles of all parameters at once (compared to the analytical solution which considers each altitude point independently). I also understand the effective resolution gets coarser where the signal is weaker and the optimal estimation gives precedence to the prior compared to the measurement. I'm not sure, however, of how to interpret a profile of effective vertical resolution as in Figures 9 or 19. In Figure 19, the effective resolution reaches > 1 km near 500m ASL, but is much finer (< 500 m) 165m above or 165m below. Is the effective resolution only to be interpreted qualitatively as an indicator of the relative importances of the signal and the prior at that particular height, or do the values themselves (500m, 1 km) mean something? If so, could you expand a bit of how to interpret them? If not, would it make sense to divide the minimum possible value (165m) by the effective resolution and provide the result as a unitless qualitative indicator of the importance of the signal for the retrieval at each height?

Yes, you are correct that the DOF of the signal is directly related to the relative importance of the signal and the prior at a given height. The inverse of that can be interpreted quantitatively as an estimate of the distance over which the state vector elements are independent and uncorrelated, at that particular point of the profile. For instance, in a region of the profile with high signal-to-noise ratio, it may be possible to retrieve the same or nearly the same number of independent state vector elements as measurements, whereas in regions with lower signal, more neighboring measurements must be considered to produce a single independent estimator of a state vector element. (To help clarify in the revised manuscript, the two sentences above have been inserted into the revision in the section "Degrees of freedom and Effective Resolution"). For the particular case of Figure 19, there are two aerosol layers with large signal-to-noise ratio separated by a very narrow clear area with low signal. If we imagine that the clear area was larger (deeper in altitude), then about 1000 m worth of data at that signal-to-noise level would need to be averaged to produce one independent estimator of aerosol extinction. Since there isn't 1000 m of clear space in the profile, the effective resolution at that particular point in the profile is a little more abstract than what would be involved in simple averaging, but it's still a useful indicator of the distance over which retrieved results are independent. The OE technique avoids having to average (or otherwise smooth) all the data to a relatively coarse resolution over the whole profile to achieve a desired resolution at a particular point. Yet, because the varying importance of the prior means the amount of effective smoothing varies as well, this metric is an important indicator of the consequence of that variability.

4. Fig. 12: I'm not sure what is the point of using diamonds, squares and circles -- at their size the symbols are almost undistinguishable anyways (even in the legend).

The symbols were meant to amplify the usability for viewers with color vision deficiencies, to add another way of distinguishing the datatypes besides just color. Your point that they were too small to distinguish is fair, so in the revision the symbols are made larger.

5. Even though the figures are well-designed and extremely clear, 19 figures is a lot, and many have a lot of subplots. Maybe some of the figures could be omitted. I'll admit that the point of the correlation plots (6, 7, 8, 15, 16, 17) was lost on me. Maybe the authors could sum up textually whatever conclusions they drew from these figures (eg about where the uncertainties are mainly systematic) and move the figures to an appendix?

Two reviewers made similar suggestions. In response, Figures 7 and 8 are replaced with this paragraph:

> *Figure 6 shows correlation between profile quantities, but the covariance matrix also includes rows that indicate correlation between the uncertainty in the profile quantities and each the scalar quantities, K′ and χ. There is significant negative correlation (not shown) between the uncertainty in the overall scaling factor, K′, and the backscatter uncertainty profile, as expected. Correlations of the overall scaling factor uncertainty with lidar ratio and aerosol depolarization ratio uncertainties are near zero. Likewise, there is predictably significant correlation between the uncertainty in the depolarization cross-talk parameter, χ, and the aerosol depolarization ratio uncertainties, whereas correlations between uncertainties in this parameter and the aerosol backscatter and lidar ratio are near zero. All of these patterns are expected and reflect that errors (although small) in the backscatter profile are partially systematic and correlated with the calibration constant and likewise that the errors in the depolarization ratio profile are partially systematic and correlated with the depolarization cross-talk parameter.*

Figures 16 and 17 are replaced with similar but shorter summary text. Figures 6 and 15 have been kept. Even if the consideration of correlations in the uncertainties is a bit more esoteric than other parts of the retrieval, I believe it is important to show them as a part of due diligence.

6. Have the authors planned to make their data analysis package (that does the optimal estimation based on lidar measurements as input) available online, for instance as a python package or something equivalent? Since the improvements are so significant compared to the more widely-used analytical approach, doing so would clearly benefit the whole lidar-based community.

We agree that that would be useful, but unfortunately no funding exists at this time to develop or maintain the code as a publicly available package in an open source language. However, a package of code is available in the IDL programming language "as is" on request from the corresponding co-author Johnathan.W.Hair@NASA.gov.