

## Response to Reviewers

We thank both reviewers for their comments. The paper is changed in significant ways to address these comments and we believe the paper has been improved in the process. Below we address the reviewers comments and summarize the changes that we have made. We include the text of the reviewers comments with our responses embedded.

### Reviewer 1:

An interesting paper. I have a only mainly editorial concerns.

1. Line 13: What is meant by “reduced optical data”? It is likely not clear to the general reader and thus, a sentence explaining this should be inserted in the abstract. It is also unclear what is meant by “yield”. I guess the authors are referring to the fraction of data volume that yields a final inversion product subject to some accuracy requirement.
  1. We added text to the abstract to clarify that the full set of measurements is 3 backscatter and 2 extinction measurements and that the reduced dataset consists of 3 backscatter and 1 extinction.
2. Simulation approach: The part of the simulation chain going from the simulated HSRL measurements to the retrieved extinction and backscatter profiles seems, to me, to be missing. Perhaps, this aspect is well-covered by one or more of the references. However, even if this is the case, it needs to be explicitly called out and even a quick overview would improve the presentation.
  1. We added a reference for the lidar equation and a statement that the model carries physical units through the entire simulation process. For a more complete explanation of the inter-workings of the model, the reader would need to consult the references as we feel this goes beyond the scope of the current paper.
3. Line 135: In the LE inversions were formulations using both volume and area based kernels used? Some studies have suggested that this can improve the accuracy of the volume and area retrievals respectively.
  1. We did such tests but didn't find improvement in S and V retrieval by using the corresponding kernels. The results depended on the specific size distribution of the case under study. We found that, in general, volume kernels worked better for all retrieved parameters, so all retrievals done in the paper used volume kernels.
4. Also were the full number of principle components, in general, used to generate the linear estimators?
  1. We used all principle components because we didn't find significant improvement by reducing the number of components.
5. Line 141: Why remove the uv and not the green? An, arbitrary decision or somethingelse?
  1. We now clarify that the 355 measurements are more technologically challenging and inherently more risky.
6. Line 158: Change "profile" to "profiles"
  1. done
7. Line 188: What is the uv extinction the noisiest? Rayleigh backscatter and extinction is about 5 times higher in the uv and the green so I guess that the total (Rayleigh+aerosol) extinction can be more accurately determined than in the green. However, the contrast between the Rayleigh and aerosol extinction is much greater at 532 nm and this leads to the final SNR being higher in the green than in the uv. Is this correct?

1. We now clarify that the laser output power is lower at 355 and the atmospheric attenuation is considerably higher than at 532.
8. Figure 1: Why is the first data point at 600-700 meters? Is this a Topography effect or a retrieval issue?
  1. This is due to a combination of the resolution of the retrievals (450 m) and the fact that we require that the elevation of the lowest bin used in the evaluation of the extinction be one that is equal to or higher than the lowest bin generated by GEOS-5 (GEOS-5 elevation resolution is different than that of the model). That way, no extrapolation is used by the model – only interpolations are done between data points provided by GEOS-5. We did not include an explanation of this in the paper since we feel it gets into detail that the reader would find distracting.

Reviewer 2:

#### General Statement

The reviewer offers many interesting comments and poses questions that would be of interest to study in the context of multi-wavelength inversions of spacebased lidar measurements. The breadth of the reviewer's questions is a good indication of the wide variety of future studies that could be performed and would, no doubt, result in increased understanding of the topic. However, in the Discussion and Conclusions section of our paper, we stated “The work done here is hardly an exhaustive study of the ability to invert aerosol microphysics from spaceborne lidar data, however. Rather it should be considered an initial study that can help to point toward needed refinements...In this way, the tables point toward other inversion sensitivity studies that would be useful for determining optimized constraints.” We included this statement exactly because of the many questions that were not addressed by this paper. Our goal was to show the basic measurement and retrieval capability of a feasible space-based multi-wavelength lidar and to put it into the context of the documented measurement goals of the ACE mission. We feel that some of the questions asked by the reviewer, while interesting and of potential value, go too far beyond the scope of the current paper for us to be able to address them here.

#### Detailed response

1. I encourage the authors to carry out a more thorough analysis of their simulation study (with additional simulations where necessary) and particularly a thorough analysis of the implications of their study.
  1. To discuss the implications of this study would inevitably get into areas of politics regarding what capability of lidar should be deployed as a part of a space mission and we chose to avoid such areas.
2. The results presented here are not robust enough in order to allow for an informed assessment of the quality of the inversion results in view of the chosen lidar parameters.
  1. It is difficult to evaluate this comment from the reviewer without more detail. We have provided what we believe to be a statistically robust set of simulated lidar profiles and inversions based on those profiles that permit useful conclusions to be drawn about the potential performance of the candidate lidar system. Many weeks of computer time were needed to perform the inversions from the 8640 simulated profiles that were studied and these cases cover oceanic, continental and various other regimes. We believe our study to be quite robust statistically.
3. The model (GEOS-5) that is used for the simulations is not well described in the paper. It remains a black box, comments are too general, and thus it is hard to judge how representative the aerosol optical data (generated for the study) are. Are the aerosol scenarios (aerosol load, absorption properties, particle size) considered in this study representative? Particularly, the authors mention the aerosol model. I would like to see a critical description and assessment of this aerosol model. In how far is this aerosol model suitable for this study? What is the impact on the optical data generated in this study if this

aerosol model (over)simplifies? The authors consider fine mode particles and coarse mode particles. Can coarse mode particles and their optical data be modelled in a realistic way? Is dust considered in this study? If yes, are these optical data trustworthy in view of the fact that the modelling of optical data of dust (the authors mention Mie simulations) is questionable. There is plenty of literature on this topic. If this aerosol model has simplifications and constraints: these factors transfer to the input data set that are used for data inversion. Thus the inversion results may be biased or may not reflect the true situation in a reasonable way.

1. We appreciate these comments from the reviewer. In response, we provide a new, extensive description of the GEOS-5 model, its interworkings and the validation work that has been done to address these comments of the reviewer. Please see the lines 85 – 129 and references therein. The paper has been strengthened by these additions.
4. What are the assumptions made in the simulations (with GEOS-5) of lidar profiles in general, aside from the aerosol model? Such information must be provided in a paper that wants to inform the reader on the potential benefit of a multi wavelength high spectral resolution lidar for aerosol studies in the 21st century.
  1. In response to reviewer 1 above, we expanded the description of the model. Beyond that, the answers to the questions that this reviewer poses are found in the references cited in reference to the lidar simulator.
5. Further it is unclear what the quality of the microphysical data really is. The authors show a few tables which are compressing the information on the retrieval quality. It is impossible to decide how the optical data quality influences the retrieval results.
  1. For the inversions based on lidar data, we show the discrepancies between the inverted results and the reference microphysical data within GEOS-5 as a function of lidar measurement uncertainty. There is a large volume of information there so we used color coding to aid the interpretation using the ACE document guidelines for establishing the color scheme. We believe that this addresses the reviewer's comments about how data quality influences retrieval results.
6. Figures 3 and 4 show distribution functions, but these plots are too general and provide little insight on the real challenges one is faced with when experimental data are analyzed in a future ACE mission.
  1. Our intent with these figures was to give more insight into the distribution of values that enter into the deviation metrics defined in equations (1) and (2) and presented in the table. Discussing the challenges of analyzing lidar data within the context of a future ACE mission goes well beyond the intent of this paper.
7. To my opinion the second main part of this paper is the generation of the optical data based on an atmospheric simulator. Thus it is equally important to provide sufficient information on the input data (simulation of lidar measurement and generation of optical data from GEOS-5).
  1. Please see 5 above.
8. Why was one specific scenario chosen for this study? Is it representative? Could there be an unwanted bias with regard to the choice of this model. The choice of this example certainly influences the reader's opinion on the benefit of a lidar system with the parameters listed in Table 1.
  1. It is not clear what specific scenario the reviewer is referring to here. There were more than 8000 lidar profiles simulated as a part of this study all with one configuration of lidar hardware. If the reviewer refers to the lidar configuration, please see next response.
9. The authors mention that the lidar parameters were chosen on the basis of feedback from manufacturers and lidar specialists (parameters are technically feasible and a reasonable representation): the tables with the results on the microphysical retrievals in part are not encouraging.
  1. We took the approach of considering a technologically feasible spaceborne lidar and studying the measurements and inversions from the the measurements of such a system. As an initial study this adds considerable insight into how well such a system would address the ACE goals. The results, indeed, do motivate other questions. As we point out, future work should focus on techniques for refining the constraints used in the inversions and in joint inversions that couple polarimeter and lidar data. Such work will likely lead to improvements in inversion capability as we mention in the Discussion and Conclusion section.

10. What should be the technical parameters of such a lidar in order to obtain results on microphysics that meet the ACE requirements?
  1. This is an interesting question but is a different one than we address here. We hope that others may be motivated by our study to pursue this question but it goes beyond the scope of this paper.
11. Or is that more a problem of the lack of information content in the lidar data and not so much the quality of the optical data provided by such a lidar. Table 5 shows that the retrieval quality does not really change between input optical data with 0-15% error and 40-50% error (coarse mode, case C). I find little change in the retrieval quality for fine mode particles (case A and B) up to 40% random uncertainty. This result is of concern.
  1. These results indicate that the problem is highly constrained yielding the relative lack of sensitivity to input random uncertainty for certain parameters (but certain parameters only). This also points out the need to have quality constraints in the inversion process.
12. The simulated lidar profiles shown in Figure 1 provide little insight on the general situation. A more thorough overview on the lidar profiles including comments on the uncertainties (under different conditions along the flight track that was simulated) should be given. The authors show one(??) profile simulated for the HSRL lidar (specifications are given in table 1) and compare it to lidar profiles of the CALIPSO lidar (in terms of how much better the HSRL lidar could be). I think such a comparison should be made more thorough as it also touches the topic of the benefit of this new lidar compared to the CALIPSO lidar.
  1. It was not our intent to perform a detailed intercomparison of the lidar simulated here and CALIPSO. As stated in the manuscript, the analog nature of the measurements of CALIPSO made for difficulties in assessing the random uncertainties of the actual CALIPSO data so we chose to just give a brief comparison of the two instruments and to use these comparisons to estimate the relative signal strength under day and night conditions. To perform a detailed assessment of the relative performance of the two instruments goes well beyond the intent of this paper.
13. In addition we need to keep in mind that data from CALIPSO cannot be used for the inversion algorithm that is used by the authors. The authors mention vertical resolution of 150 m for the backscatter and 450 m for the extinction profiles. Did they use the same vertical resolution (backscatter and extinction profiles) for generating the optical input data that are used in the inversion?
  1. The inversion were done at 450 m vertical resolution. So the backscatter data were summed to 450 m vertical resolution to correspond to the resolution of the extinction measurements. We have added a statement to this effect in the manuscript.
14. Tables 2 - 4 are helpful as they allow for a first insight on the potential yield of data sets that can (from the total set of data points collected during a track) be used for data inversion; please check Table 6 (color coding information for surface concentration is missing).
  1. Thank you for catching this mistake. We have corrected this in the table.
15. This yield, i.e. the number of data points suitable for inversion are based on the constraint of extinction coefficient at 532 nm ( $0.02 \text{ km}^{-1}$ ). Do other factors play a role in that yield? How big (number of data points suitable for inversion) would the yield be if extinction is set to  $0.01 \text{ km}^{-1}$ ? What about the quality of backscatter coefficients? That does not play a role?
  1. The  $0.02$  extinction threshold was chosen as it is used in the definition of desired lidar performance within the referenced ACE document. Other factors that play a role are the uncertainty of the measured backscatter and extinction signals. We consider this influence by studying the inversion quality as a function of random uncertainty in the data. Yield is assessed by requiring that all measurements meet the goal of 15% random uncertainty or less as stated in the ACE document. A lower extinction threshold would provide more potential points for inversion but they would be more difficult to measure by the lidar due to their tenuous nature.
16. The authors decide to remove extinction at 355 nm and run another set of simulations. This is a good idea as it allows for some insight if a fully blown 3 backscatter and 2 extinction lidar is not used. Why not do the same study by removing extinction at 532 nm? What would be the yield of data points if you prescribe a constraint on the extinction coefficient at 355 nm? What would be a suitable threshold value?
  1. We chose to remove the 355 extinction measurement since it is technologically more challenging and thus more risky for a space-based mission. It is also the one that possesses the highest random

uncertainty. Those considerations made it the most likely measurement to be deleted if such a decision were necessary. We include additional material clarifying these points in the manuscript.