Atmos. Meas. Tech. Discuss., 8, C2763–C2769, 2015
www.atmos-meas-tech-discuss.net/8/C2763/2015/


Atmospheric Measurement Techniques
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Discussions

# *Interactive comment on* "Optimization of the GSFC TROPOZ DIAL retrieval using synthetic lidar returns and ozonesondes – Part 1: Algorithm validation" *by* J. T. Sullivan et al.

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Received and published: 28 August 2015

Author Response for

Journal: AMT Title: Optimization of the GSFC TROPOZ DIAL Retrieval Using Synthetic Lidar Returns and Ozonesondes - Part 1: Algorithm Validation Author(s): J.T. Sullivan et al. MS No.: amt-2015-32 MS Type: Research Article

——————————————————————————————————- Anonymous Referee #2 Received and published: 31 July 2015

The paper, according to the authors, is the first of a series of papers that deals with



the optimization of the GSFC TROPOZ DIAL. The authors present in this paper an improved algorithm and they validate their improved algorithm using synthetic data. This allows them to quantify the effect of the changes and/or improvements, independent of the accuracy of validation data such as ozonesondes. The paper is well structured and written and is suitable for AMT and thus can be accepted for publication, after considering few remarks listed below.

1. It would good if the authors summarize in a table the main differences between the old and the optimized algorithm.

AR: Table 1 has been created and placed in the final discussion section of the manuscript. This describes the main differences between the optimized algorithm and the previous algorithm (found in Sullivan et al., 2014).

2. The authors should emphasize more that the paper mainly aims to validate changes in an algorithm and not to validate real DIAL data, processed with a new algorithm. In some cases the reader is confused what to expect.

AR: This has been cleared up in several sentences.

3. Section 3. The authors should provide more details how (e.g. provide some equations) they compute the synthetic data. They mostly mention what are the input data they are using rather than explaining how the computations are made. They just provide a reference to an empirical model for the estimation of the atmospheric state. It would be better to describe some basic steps of the calculations in the manuscript since the use of synthetic data is a key issue in their paper.

AR: We have added in a discussion and several of the equations used in simulating the raw signals. It has also become apparent after adding several equations that the flow of the paper is improved if the section 2 and 3 are reversed. Now the signals are mathematically described (Sec 2) and then the DIAL equations (Sec 3) are introduced using consistent definitions of lidar parameters and variables.

4. Section 3.2 The authors should eventually also examine, or at least discuss, the use of the different data sets concerning the ozone cross sections that were considered in ACSO study coordinated by WMO (see WMO GAW report 218).

AR: A discussion of this study has been added into Section 3.2.

5. Section 3.3 It is not clear what is new in this section. Did the authors consider improved new Rayleigh extinction cross sections or they just quantify the effect of using them in the correction form?

AR: This section was aimed at confirming the retrieval was properly accounting for the Rayleigh extinction in the DIAL retrieval. This was not new per se, but was an important step in systematically quantifying any underlying biases in the retrieval. This can be removed if necessary, however it is only 2 paragraphs and helps to advance the methodology of the manuscript.

Anonymous Referee #3 Received and published: 1 August 2015

The authors present in their manuscript an improved algorithm for deriving ozone profiles from DIAL measurements. Using synthetic lidar data enables a separation of various influences on the data as well as estimation of the effect of algorithm modifications. The authors are aiming on a series of papers. In one of the future papers they are intending to publish a systematic error analysis. It seems strange to me to validate an algorithm without systematic error analysis. The work is quite important for the network TOLNET. However it is questionable whether an improvement or optimization of an algorithm represents a substantial contribution to scientific progress and justifies a publication as a separate article. Nevertheless the analysis the algorithm and the described individual effects (incomplete see below) are well done and well written. Some detailed comments

1) An overview effects taken into account by both (old and new) algorithm together with highlighting improvements of the new algorithm is missing.



AR: The original algorithm was published in Sullivan et al., 2014 (amt-7-3529-2014) and Table 1 has been added to highlight the improvements over the original algorithm. This is stated more clearly in the text.

2) Although the authors mention the influence of aerosol particles in the retrieval of ozone (see eq. 3), they were neglecting this effect for validating the optimized tropoz algorithm. It remains unclear whether the new algorithm still performs better than the old algorithm in the presence of aerosol particles. As the authors used synthetic data for their validation, it would be easy to simulate the effect of aerosols. The argument by the authors (page 4279) that "would yield little information about the retrieval's ability to correct for aerosols during actual observations" is not convincing given the aim of validating an optimized algorithm.

AR: There is no simulation of aerosol backscatter or extinction implemented at this time. However, we understand the need to address aerosols in the real atmosphere and the aerosol correction from Sullivan et al., 2014 is utilized in Section 6 when computing ozone profiles from actual lidar signals and comparing to ozonesondes. This has been stated more clearly in the text. Because a full aerosol simulation and analysis would require careful considerations (e.g., number and type of particles), it is beyond the scope of the present paper. The current plans are to have a specific section in the following manuscript addressed to aerosols and aerosol uncertainty.

3) The description of the computation of synthetic lidar data is very vague although the synthetic data are very important for this paper. The authors mainly mention the effects which they took into account. For instance on page 4280 it is mentioned FoV, filter bandwidths. Numbers are not provided. A reference to a system description where numbers can be found is also not provided. Here some more information would be useful.

AR: A reference to the Sullivan et al., 2014 paper has been added for these values. Additionally, as requested by another reviewer, several equations and descriptions of the methodology have been added to describe the simulation of the signals.

4) The authors restrict the simulation of lidar data to cloud free and nighttime data. Why is that? what about day-time data? In their AMT paper (Sullivan et al., http://www.atmos-meas-tech.net/7/3529/2014/amt-7-3529-2014.pdf) the authors present day-time measurements.

AR: Nighttime data is used as the basis for the simulation because of the low level of background noise. This is the simulation for the closest to "only signal" regime. Additional effects are added in to represent daytime conditions in Sec 5. Nighttime conditions (low simulated noise) allow a larger vertical range of validation. Daytime simulations can also be made, with the only difference that the background noise is much higher and limits altitude range. After the algorithm is optimized, Section 6 presents several comparisons with actual daytime lidar data and ozonesondes.

5) The authors describe the dead-time correction in section 3.4. In section 6 they mention that the true dead-time is slightly larger than the theoretical one with 4-5 ns. Again details of the PMT are missing. In their AMT paper (Sullivan et al., http://www.atmos-meas-tech.net/7/3529/2014/amt-7-3529-2014.pdf) the authors present a technical description and they mention that the dead-time is 4-7 ns. Could you explain these different values?

AR: The dead time correction is based on the inverse of theoretical counting rate of the data acquisition system. The current system is 250 Mhz (or a dead time of 4.0 ns), which will be explicitly stated/updated in the text. Because this is the theoretical counting rate, it also acts as an upper limit to the counting rate, or conversely a lower limit for the dead time correction value and typical values needed to correct for this effect are between 4-5 ns.

We have since the Sullivan et al., 2014 paper been able to determine several sources of electronic noise in the system that were largely affecting the lowest altitude range of the detectors and 4 -5 ns is currently an adequate range of dead times for the retrieval.



6) Section 4 vertical resolution scheme it seems to me that the variable vertical resolution scheme is a key improvement of the new retrieval algorithm. i am missing a discussion or remarks on the final retrieved EFFECTIVE vertical resolution. I guess that the simulations are done with the vertical resolution of the lidar system. After smoothing the data within the same smoothing window are not anymore independent of each other and the resolution is decreasing which results in a decreased ability to resolve fine-scale structures.

AR: The native resolution of the lidar system and the subsequent signal simulations are 15 m, which are based on the limits of the data acquisition system. The vertical resolution scheme presented in Fig. 7 is the effective vertical resolution of the final retrieved ozone product. Therefore, Fig 7 shows an optimized vertical resolution scheme between the hardware of the lidar system and the physical atmosphere (i.e. vertical processes/gradients occur more frequently within the PBL than in the free troposphere). A discussion on this and how the effective vertical resolution is calculated has been added.

7) I am not understanding figure 7. the vertical resolution (window size) is well correlated with the relative statistical uncertainty. could you explain why is that? I would rather expect an anticorrelation.

AR: The vertical resolution is a measure of the amount of signal filtering applied to the signal and as a result of this filtering the statistical noise is reduced. The step at 3 km should be expected because the amount of filtering changes with the pair of channels used (from low SNR to high SNR). Regardless of vertical resolution schemes, the SNR in the upper altitude range is largely decreasing as the laser signal (or simulated laser signal) is becoming less resolvable as compared to the ambient background noise. Because of this inherent vertical relationship, the statistical uncertainty increases in the upper altitudes of profile. The vertical resolution scheme presented here is intended to optimize the statistical uncertainty to maintain values below 10% throughout the first 10 km of the atmosphere.

8) The authors should re-read their paper and provide references (self-citations) in cases they copied and pasted from their own articles. One example can be found on page 4282 of this manuscript which is identical with the text in the AMT 2014 paper (Sullivan et al., http://www.atmos-meas-tech.net/7/3529/2014/amt-7-3529- 2014.pdf) on page 3537. For easier comparison i copy here some sentences: this manuscript:

The finite impulse response (FIR) Savitzky Golay (SG) differentiation filter (Savitzky and Golay, 1964) is used for the numerical derivative and acts as a smoothing filter by neglecting large noise spikes. The SG filter is a generalized running average with coefficients determined by an unweighted linear least-squares regression and a 2nd degree polynomial model applied to the derivative. The second degree is chosen, instead of a third or fourth, because it is less likely to pick up extreme noise. the Sullivan et al. 2014 paper: The finite impulse response (FIR) Savitzky–Golay (SG) differentiation filter has been used to produce the required first-order derivative. The SG filter (Savitzky and Golay, 1964) is a generalized running average with filter coefficients determined by an unweighted linear least-squares regression and a second-order polynomial model applied to the derivative. The second order was chosen instead of a third or fourth order because it was less likely to pick up extreme noise in the derivative.

AR: Additional references to the Sullivan et al., 2014 paper have been implemented and redundancy has been reduced.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C2763/2015/amtd-8-C2763-2015-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., 8, 4273, 2015.