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Atmospheric Measurement Techniques 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.

Anonymous Referee #3

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The authors present in their manuscript an improved algorithm for deriving ozone profiles from DIAL measurements. Using synthetic lidar data enables e 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.

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.

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.

4) page 4280. The authors restrict the simulation of lidar data to cloud free and night-time 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.

5) dead-time correction 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

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present a technical description and they mention that the dead-time is 4-7 ns. Could you explain these different values?

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.

7) figure 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.

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

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or fourth order because it was less likely to pick up extreme noise in the derivative.

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