

Interactive comment on “Estimation of background gas concentration from differential absorption lidar measurements” by P. Harris et al.

P. Harris et al.

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Response to Referee #2's comments

on the paper “Estimation of background gas concentration from differential absorption lidar measurements”

by *P. Harris et al.*

We would like to thank the Referee for their positive opinion about our manuscript. Our responses to the comments are listed below. Changes to the manuscript are marked in blue.

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General comment. *As stated by the authors themselves in the final section at page 20 of the manuscript, the presented method and analysis are part of an ongoing work, and a validation exercise using independent measurements is missing thus preventing the reviewer to have in one manuscript all the elements needed to assess the effective value of the presented approach. To my opinion, what stated by the authors in the items 2 and 5 at page 20 (i.e. need to quantify the total uncertainty budget of the investigated measurements and the need for an independent validation of the background concentration using direct measurements) looks a missing opportunity to complete the characterization of the proposed approach. Concluding, I'd suggest the authors to extend the current version of the manuscript in order to make it more complete and appealing for the reader.*

Response. What the Referee suggests as items a.-c. aligns very well with the future research plans of the authors and, as indicated in the section on concluding remarks and future plans, we do intend to pursue these ideas in a separate publication to follow-on from the current paper. While we understand the suggestion of the Referee, and agree that some items including further validation of the approach using experimental data and the evaluation of uncertainties are important to convince readers of the merits of the proposed approach, we believe the paper in its current form is already quite long and the technical content and the presented results are sufficient to stand on their own and to be of interest to readers.

Comment 1. *In a few points of the manuscript, mainly in the introduction, a more generous number of references might help, for example I'd add a reference about the background gas concentration at page 1, line 18, and another, at line 20, generally describing the DIAL technique.*

Response. We have added the reference THE NOAA ANNUAL GREENHOUSE GAS INDEX (AGGI); <http://esrl.noaa.gov/gmd/aggi/aggi.html>; accessed 7/7/16 for background methane concentration. We have already given a general DIAL tech-

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nique reference (Measures, 1984), which we have referenced again at line 20.

Comment 2. *Page 2, lines 18-20, please specify in which way the introduced constants are derived or provide a reference to mention the source of these numbers.*

Response. Equation 1 is adapted from (Milton and Woods, 1987) where we have introduced α and β to represent the systematic offsets.

Martin M J T and Woods P T (1987) Pulse averaging methods for a laser remote monitoring system using absorption backscatter *Applied Optics* **26** 2598–2603.

Comment 3. *Please throughout the text use the same way to define the figures, Fig. or Figures, see for example page 12, lines 5-10.*

Response. We have corrected this, and used “Fig.” or “Figs.” throughout the manuscript.

Comment 4. *Page 12, lines 6-7: there is a sentence in brackets, but it seems that brackets should be removed. Please revise the sentence.*

Response. We have removed the brackets.

Comment 5. *Page 17, line 3, the authors must provide an explanation for the inconsistencies in the values of $\kappa_{2,k}$ but also of $\tau_{1,k}$, at the different considered elevation angles for the GLS approach.*

Response. We are not able to give a technical explanation for the differences in the values of these parameters for different values of the elevation angle. However, the results suggest that for the dataset considered the auto-correlation terms in the statistical model (defined by the parameters $\kappa_{1,k}$ and $\tau_{2,k}$) for the measured data are reproducible for different elevation whereas the cross-correlation terms (defined by the parameters $\kappa_{2,k}$ and $\tau_{1,k}$) are not. We have modified the discussion of these results and included a comment to this effect.

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Comment 6. *Page 17, line 15-20, explain better the concept discussed in these lines and the reason for the data inconsistency affecting a small fraction of the presented dataset.*

Response. Again, we are not able to give a technical explanation, but the results suggest that the statistical model determined using the data after the plume may not apply for the data before the plume. Ideally, we would like to include that data (before the plume) in the characterization of the noise (or to characterize separately the noise in the data before the plume) but that is made difficult because the amount of data is small and there is a gap between the data before and after the plume. We hope that the availability of data for which there is no plume may help to shed some light on this aspect. We have modified the discussion of these results and included a comment to this effect.

Comment 7. *The authors should spend some effort to simplify the mathematical notations; to increase the readability of the manuscript a good idea could be to put most of the mathematics in an appendix.*

Response. While we understand the suggestion of the Referee, we would prefer to keep the current structure of the manuscript introducing necessary mathematical concepts as they appear, to keep the flow of the explanation. We use only a minimum of technical details that are necessary for understanding the paper, and those may become obscure if moved into an auxiliary section.

Comment 8. *Page 20, Line 18: please change the results presented in the presented results.*

Response. We have changed this.

Comment 9. *Conclusion are much more a summary and outlook section than a real conclusion section; please provide your conclusions in a more extensive and clear way.*

Response. We agree with the Referee's comment, and have tried to add some more definitive statements in the conclusions section.

Comment 10. *Figures and tables are clear. Only in the case of Fig. 17, both the caption and the plot legend must report the number of elevation angles. I also ask the authors to increase the size of the plots in Figure 17.*

Response. We have added a legend to figure 17 to identify the results for each elevation angle and have increased the size of the figure.

We hope that the revised manuscript is now suitable for publication in *Atmospheric Measurement Techniques*.

P.Harris, N.Smith, V.Livina, T.Gardiner, R.Robinson, and F.Innocenti

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