

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

Anonymous Referee #2

Received and published: 20 May 2016

GENERAL COMMENTS

The manuscript by Harris et al. compares the use of least square models traditionally used for the estimation of background gas concentration from differential absorption lidar (DIAL) measurements with other two approaches with the final aim to reduce the uncertainty affecting the retrieval of the background gas concentration. The authors introduce an autoregressive model for the characterization of noise of DIAL measurements which looks to be able to reduce the residual deviation of a generalized least square (GLS) fitting approach for both the off- and on-resonant signals. The manuscript is written very well and looks in good shape. Nevertheless, 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

[Printer-friendly version](#)

[Discussion paper](#)



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 quantity 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. At minimum, I ask to include:

- a. in absence of any independent measurement to validate the performance of the proposed approach in retrieving the background gas concentration, an assessment of total uncertainty budget of DIAL measurement is needed to show how much large is the impact of using the new GLS approach (i.e. is the uncertainty reduction discussed in the manuscript the main contribution to the uncertainty budget thus improving the total uncertainty budget affecting the estimate of the background concentration).
- b. the performance of the approach when there isn't any knowledge about the location of the plume should be also studied and discussed.
- c. optionally, an assessment of the approach based on a joint analysis of the measurement for all the elevation angles considered in the manuscript represents another interesting additional content for the manuscript; if the authors will also deal with this part, I'd also suggest to use a simple dispersion model to simulate the background concentrations at the various angles as an ancillary information in the data interpretation.

DETAILED COMMENTS

Below I also provide some comments to help improving the quality of this manuscript.

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

[Printer-friendly version](#)[Discussion paper](#)

DIAL technique.

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.

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

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

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

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.

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.

Page 20, Line 18: please change “the results presented” in “the presented results”.

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.

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.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-73, 2016.

Printer-friendly version

Discussion paper

