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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Combined retrievals of boreal forest fire aerosol properties with a polarimeter and lidar" *by* K. Knobelspiesse et al.

Anonymous Referee #2

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Title: "Combined retrievals of boreal forest fire aerosol properties with a polarimeter and lidar"

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This paper discuses the merger use of polarimeter and lidar data for absorbing aerosol properties retrievals. The inversion algorithm is based on the modified Optimal Estimations approach. The authors performed retrieved data analysis and compared the obtained results with the data received independently from the additional instruments

(AATS, HiGEAR) used during the campaign for reference and validation purposes only. The subject of this paper inversion is appropriate for publication in journal Atmospheric Chemistry and Physics and interesting for the aerosol remote sensing community. The text of the paper is clear and well written. In my opinion the paper can be published in "Atmospheric Chemistry and Physics". However, I suggest some revisions of the text of the paper before its publication.

Comments:

1. The optimal estimation method (by Rodgers C. D.) relies on the application of a priori covariance matrix. If such matrix is not used, then the inverse technique cannot actually be called "optimal estimation method" in the sense as defined by Rodgers. However, in these regards, I did not see of a priory estimates and their covariance matrix use in section 2.3.1 devoted to the optimal estimation. The a priori covariance matrix appears later in the section 3 in equation 18 in context of the Shannon information content. From my point of view this matrix should be explained in some details. The authors should clearly state if they use any a priori terms or not. If not, then they should explain how equation 18 defined for optimal estimation method could be used in their case.

2. In general, the foundation of used inversion scheme and scheme by itself is not clear.

- First, it seems that the authors try to follow (in many aspects) the retrieval strategy suggested by Waquet et al. (2009). However, the inversion scheme was changed. It would be very useful if the authors could state why they concluded to make those changes, and what kind of improvements they expect.

- Second, the authors seem to use rather basic scheme of numerical non-linear fitting based in well-known Levenberg-Marquardt technique. I am not sure that it is appropriate to include into this paper as many technical details as the authors done. Those details should be only included if the authors used some original modifications. Then modifications should be clearly explained and shown.

- The text of the paper implies that using Levenberg-Marquardt technique resolves the possible issues with insufficient information content. However, it is not correct, because Levenberg-Marquardt method is mostly used to achieve monotonic convergence in non-linear case. However, it does not address the fundamental issue of solution non-uniqueness. As a result the authors admit the appearance of local minima. The authors should have strategy to make solution unique.

3. The paper is very technical and does not provide enough physical inputs and interpretation. For example, the authors do not discuss at all the assumptions taken for their aerosol models (bi-modal log-normal size distribution). There is no discussion if there any limitations in using such assumption. At the same time, the authors note by themselves that HiGEAR Size Distribution data measured during the campaign is not bi-modal log-normal. They suggest that this could explain some discrepancy of retrieval. In my opinion it would be very appropriate to investigate this aspect more seriously. For example, in order to evaluate the possible relevant uncertainty in the inversion method's performance I would suggest completing a sensitivity study using at least HiGEAR Size Distribution data measured during the campaign to compute the reflected radiances. This sensitivity study may immensely improve understanding of proposed algorithm's efficiency.

4. The conclusions suggest that the authors observed quite significant limitations in the retrieval accuracy of aerosol properties by APS. If that is the case, it would be very interesting if the authors could comment how their results agree with generally very high expectations from APS and previous RSP results as those published by Waquet. Can actually APS retrieve aerosol properties as accurately as it used to be expected or the authors identified some previously unknown limitations.

Minor comments:

1. The notation of the equation 6 p.7920 seems to be unclear and needs to be more explained. The authors probably used quite specific mathematical notations that for

ACO readers are not evident (in my opinion). In particular it is not clear for me whether the colon (:) sign in the equation stays for division or has another meaning.

2. Line 17, page 7918: the authors referenced the term CF, which does not appear in any equation of the article. Thus the beginning of the sentence "Waquet et al. (2009) uses a fourth term" remains unclear.

3. The term Y introduced in equation (2) as measurement vector is used later in equations 9 and 10 as scalar having obviously meaning of Lagrange multiplier.

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