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

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Author response to referees for:

Combined retrievals of boreal forest fires aerosol properties with a polarimeter and lidar

Response to anonymous referee 1)

I would like to thank this referee for his or her thorough and helpful review of this paper. I have attempted to make all the suggested changes. I address them each individually below.

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Specific Comments: *Page 7911, lines 4–6: Add (1974) to Hansen and Hovenier on line 5, and delete this repeated reference on line 6.* OK, thank you

Page 7911, line 20: Are there 240 viewing angles or 240 combinations or viewing angle and spectral channels? There are 240 viewing angles in each channel, I modified the text to hopefully make this more clear.

Page 7913, lines 13-15: The scan angle range and increment given here suggests ~140 angles, not the 240 angles mentioned on page 7911. The Aerosol Polarimetry Sensor (APS), which was planned for Glory, was to have about 240 angles. Its aircraft prototype, the Research Scanning Polarimeter (RSP), which I describe in this paragraph, has a lower angular sampling and fewer viewing angles.

Page 7914, line 13: The 150 view angles given here is inconsistent with prior statements. Please be accurate and consistent throughout the paper. Please see above

Page 7919, line 5-9: Can you say how you selected the 0.5 degree heading and pitch error estimates, are they empirically determined by data variance? The error estimates are indeed empirically determined. I put a comment to this regard in the paper.

Page 7919, line 27: I assume you mean lack of surface heterogeneity over ocean? Yes.

Page 7924, lines 3-5: You should elaborate that surface reflectance is nominal in the blue for soil and vegetated surfaces, but not for snow and ice surfaces (see Hsu et al. 2004). Yes, of course. I changed the text and added the Hsu reference.

Page 7927, lines 1-2: I suggest using BOLD in Table 1 to make it more obvious, since I cannot distinguish the italics from the standard font. Ok, done.

Page 7927, lines 7-13: I suggest that Lewis et al. (2008) would be good reference to add in this discussion of BC and OC in smoke aerosols. Great suggestion, thank you

Page 7929, lines 19-20: What wavelengths were used in the computation of the

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Angstrom Exponent, and were only 2 wavelengths used or multiple wavelengths with linear regression? Then Angstrom exponent was calculated with a linear regression in log space. This comment is added to the text.

Page 7931, lines 17-19: Why do you use an exponential function, do you mean a 2nd order fit of \ln AOD versus \ln WL as shown by Eck et al. (1999) to be the best fit of AOD spectra? I do indeed mean a 2nd order polynomial, and indeed this is what the AATS instrument people do . . . saying exponential function was apparently a typo. I've corrected the text.

Page 7933, lines 9-16: Why do you only compute the single scattering albedo at one wavelength and not interpolate to the 3 measurement wavelengths (which are relatively close for both the nephelometer and PSAP)? I feel that interpolating the scattering and absorption coefficients first, and then computing only the SSA at a single wavelength is the best way to minimize the potential spectral differences between. I see no added value of computing the SSA for three wavelengths, since the purpose is to compare to RSP results.

Page 7935, lines 1: You say 'each plot' but there is imager y only in the top panel of Figure 3. I changed this to 'panel' from 'plot'.

Page 7936, lines 1: You say the optical depths from HSRL and AATS are 'quite similar'. Please quantify the comparison here with the mean absolute difference and standard deviation of the differences. The median values of HSRL and AATS at 532nm differ by 0.033. I put this more specific note in the text.

Page 7936, lines 14-15: The comparison of the data sets in Fig. 3 is one of the most important Figures in the manuscript, however in the size presented it is relatively small and hard to read, including the y-axis font size and the color legends at the bottom. I suggest you consider a full page or even 2-page format. I would like this to be a full page figure as well, and will speak with the editors to ensure this is the case.

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Page 7937, lines 24-28: *Related to Figure 4, please give the Angstrom Exponent computed from both the AATS and retrieved AOD in the 350 nm to 1000 nm wavelength range. It would also be useful to include the spectral variation of Angstrom or include the α' parameter (Eck et al., 1999) to characterize the magnitude of the non-linearity of In AOD versus In WL.* OK, I'll compute these values and add them to figure 4.

Page 7938, lines 25-27: *It would be useful to also reference Dubovik et al. (2002), 0.94 and Eck et al. (2009) Alaska smoke of 0.96, especially since the agreement with your data is excellent.* I will, thank you for the suggestion.

Page 7939, lines 9-11: *Please quantify the difference is single scattering albedo here rather than just 'slightly higher'.* OK, I will put the values from the table in the text.

Page 7939, lines 13-14: *Similarly, please quantify the difference is single scattering albedo here rather than 'drastically lower'.* See above

Page 7939, lines 16-18: *Also, quantify the differences in effective radii and variances in addition to using the relative phrases of 'slightly larger', 'much larger', etc.* See above

Page 7945, lines 20-23: *DeÔÁÍÁne quantitatively what you consider an acceptable uncertainty range for aerosol single scattering albedo.* I will change the text to be more specific.

Response to anonymous referee 2

I am grateful to the reviewer for his or her thoughtful comments with regards to this paper. Before I delve into responses to detailed comments, I want to describe our optimization method and philosophy. For starters, we use the terms "Optimization" or "Optimal Estimation" in a broad mathematical sense, meaning that a strategy is employed to select a set of parameters that maximize (or minimize) a function. Although the important work of Rodgers is used in some parts of the text (most specifically when computing the information content), the optimization algorithm we used is described in the referenced papers by Markwardt and More and implemented in the MINPACK-1

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and MPFIT software packages. Although there is much overlap, we are not specifically using the methods of Rodgers. Indeed Rodgers is not referenced in the paper until the discussion of the results. We hope that readers will understand, based upon the cost function described in equation 2 and subsequent description, that this method is slightly different than the common approaches taken by Rodgers. Most specifically, we do not have a side constraint on the distance from an a priori defined set of parameters in the cost function. Rather, an initial set of parameters were chosen as starting points in the optimization, but were allowed to freely vary as specified by the Levenberg-Marquardt technique.

As stated in the paper and noted by the referee, much of the aerosol retrieval approach we used has its heritage in the work described in Waquet et al. The radiative transfer model was identical, and much of the retrieval approach was the same. The software implementation of the optimization, however, differed for reasons that are both practical and theoretical. Rather than use custom built optimization software, we decided to switch to a publicly available, peer reviewed and tested, optimization software package. While Waquet's software was entirely capable, the use of MPFIT meant that retrievals could be performed faster and in a more flexible manner. Furthermore, we could be confident that various numerical and computer sciences related issues are handled by software created by people who properly implemented the Levenberg-Marquardt method. We note that this algorithm is often introduced purely as a constraint on step size, which does not include the insight provided by Marquardt regarding the use of the diagonal of the Hessian matrix as the appropriate measure with which to constrain step size. From a theoretical standpoint, it was also an advantage to forgo the a priori value side constraint and we note that an a priori covariance matrix is also not used in analysis of data of this kind by Dubovik et al. (2011, end of section 4.1.1), or Hasekamp et al. (2011) since the problem is not ill-posed and they also describe their methods as being a statistical optimization. The purpose of this work is to investigate the information contained within RSP/APS style retrievals, so that results are less sensitive to both the choice of the starting set of parameters and strength of the a priori distance

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constraint.

Considering the potential for confusion with regard to these issues, we felt it was important to describe the mathematics of the optimization algorithm in detail. While we recognize that this is a long manuscript, we hope that the interested reader will find the details useful when differentiating with other techniques.

This paper discusses 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 priori 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.

As noted above, this method does not use the specific optimal estimation method described in Rodgers. The first reference to Rodgers' work is in the Results section,

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where the information content has been calculated in order to compare the results of retrievals that used, and did not use, external data from the HSRL. This information content metric requires that an a priori matrix is specified, containing the parameter uncertainty prior to observations. We filled our a priori matrix with uncertainties representing the range of possible values for the retrieval parameters. This was done the same way for both types of retrievals, so we think that the exact values of the a priori matrix used for computation of the information content are neither important nor interesting for the reader.

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

We have changed some of the text in the opening of section 2.3 to hopefully make this more 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.

Again, please see above and in the body of the text, which we modified.

- 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.

While we did not make original modifications to the technique in the papers by Marquardt and by More, we feel that we are justified to describe these details because there are a variety of different implementations of the Levenberg-Marquardt technique. Indeed, the referee's confusion about our methodology implies that the technique we

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used warrants discussion (and requires further clarification).

- 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.

The intent of this paper is not to imply that the LM technique resolves issues with insufficient information content. Rather, the large quantity of information gathered by the RSP/APS will lessen the possibility that a non-unique solution is found (although we did, indeed find one in this case). In any case, if information do not exist in the data, neither the LM technique, nor any other technique, can select the proper solution without making additional assumptions about the aerosol properties. Recall that both solutions we found consisted of physically plausible aerosols, and that both had similar residual errors. Without the luxury of external data, it is impossible to select one and ‘solve’ non-uniqueness. This is the primary value the use of the HSRL data in this context, as it helps the APS optimization converge to the appropriate minima (as verified by in situ data).

We also want to clarify that we did not converge to a local minima. The terms “non-unique” and “local minima” are not synonymous, as the referee’s text implies. Rather, a local minima has a greater squared error than the true solution. Non unique solutions, on the other hand, have equal squared errors, and this was indeed the situation we found.

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

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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.

The use of bimodal size distributions to describe aerosols is neither new nor exotic – in fact they are used as far back as Hansen and Travis (1974). Size distributions such as these are of course approximations of reality, but necessary if we are to limit the quantity of retrieved parameters and avoid ill-posed retrievals. The sensitivity study you suggest does sound interesting. However, considering the length of this paper and the difficulty of implementing it in a radiative transfer model that specifies aerosol quantity in terms of size distributions, it is clearly beyond the scope of this work. In any case, the aerosol size distribution, both retrieved by RSP and observed by HiGEAR, are very clearly dominated by the fine size mode. This size mode is well described as a lognormal distribution, and has peak number concentrations at least four orders of magnitude larger than any peak in the coarse mode. If considered in terms of the more optically relevant cross sectional area, the fine mode is still at least three orders of magnitude larger.

4. The conclusions suggest that the authors observed quite significant limitations in the retrieval accuracy of aerosol proper ties 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 proper ties as accurately as it used to be expected or the authors identiÔÁÁed some previously unknown limitations.

As stated in the first sentences of the conclusion (and elsewhere in the paper), this

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scene represents a rather non-typical aerosol condition. Recall that the aerosol optical depth at 532nm is nearly 0.7! This is a very large optical depth (nearly double the median value for boreal forest fire smoke observed by AERONET in Dubovik et al 2002), which renders the surface nearly invisible at blue wavelengths. Furthermore, the aerosols are vertically distributed in a manner that is uncommon for Tropospheric aerosols. Essentially, this scenario was chosen in an attempt to find the limits of RSP and APS retrieval capability. The results here are not applicable to other retrievals in more common conditions. Moreover, the retrieved size distributions are in good agreement with the in situ observations. Aerosol optical depth retrievals are also within 0.078 of the AATS-14 and 0.045 of the HSRL observations. A high bias such as this may be correct given that the AATS is on a plane flying at 627 m and the HSRL is heavily attenuated. The retrieved single scattering albedo is also within ± 0.04 of the in situ value, which would usually be regarded as reasonable. Finally it is important to recognize that the primary source of measurement uncertainty for this set of measurements is aircraft attitude, in part because the aircraft is flying at the altitude of the jet stream, which causes significant yaw and pitch effects.

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.

The colon means ‘such that’. So the expression in equation 6 means to find the minimum of the norm on the left hand side of the colon, such that the inequality on the right hand side of the norm is still satisfied. We added more description of this equation in the text.

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 four th term” remains unclear.

We changed the text to make this more clear.

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.

Equations 9 and 10 actually use a non boldface Upsilon that we hoped would be different from the boldface Y in equation 2. Unfortunately in the ACPD font, they appear similar, although we hope the bold/not bold is sufficient to differentiate the two.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 7909, 2011.

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