We thank the reviewers for their thorough reading of the manuscript. We made significant changes to the introduction and the methodology sections. Clarifications were requested in the result section, we added text. We used in part results from a manuscript that has been submitted to JGR by one of the TCAP PI's. Below we reply in detail to the reviewer comments.

Reply to reviewer 1:

However, the paper is too short. Many points are not outlined in sufficient detail. This must be improved.

We added information in the introduction, we added a section about the inversion algorithm, and we added more information regarding our interpretation of the data. With regard to the instrument section and the reviewers' requests, comments are provided below. Changes to the original text are marked in bold letters.

The PI of the HSRL team (C. Hostetler) is of the opinion that it is not the purpose of this paper to provide technical details on HSRL-2. It would water down the real purpose of the paper which is about presenting first results of microphysical particle properties that can be obtained from this system. For this reason the first author of this manuscript added mainly text on why HSRL-2 is needed for the retrieval of microphysical properties, see introduction and new section 2.2.

The PI of HSRL-2 takes the right to publish the instrument details himself. He regrets that he has not found time yet to do so. Particularly with regard to the 355-nm channel he emphasizes that no technical details can be inserted in the contribution. I hope the reviewers understand and accept the motivation for his decision.

The reviewer asked for a historic overview on EARLINET and ESA activities. Information was inserted as much as possible to answer the reviewers' questions.

The HSRL team is of the opinion that a detailed explanation is not suitable for the purpose of this paper. It would water down the aim of the paper. I hope the reviewer understands and accepts this opinion.

Detailed comments:

Introduction:

Page 1062: A better introduction into multiwavelength aerosol lidar is needed. Why do we need this multiwavelength technique? Where is it realized? EARLINET!

Text has been added to the introduction.

Next, a better introduction in the development in multiwavelength HSRL is then need. What is the potential (compared to existing and well established Raman

lidar networks) of such systems, advantage compared to Raman lidars? HSRL can be used on aircrafts, Raman lidar are only useful as ground-based systems.

We added text to the introduction. Restriction with regard to daytime measurement capability, long signal-averaging times are among the caveats of Raman lidar. Details on the development of multiwavelength HSRL will be given by Hostetler et al. in his contribution, as mentioned in my introductory paragraph to this reply. We hope the reviewer understands the motivation for this decision.

What is the basic motivation to develop such a system? Explain, why NASA undertakes these effort to investigate the potential of future, next generation spaceborne lidars.

We provide some explanation in the introduction and we mention ACE in the summary section.

Then one needs to discuss the already on-going activities of ESA, ALADIN and EarthCARE HSRL and the potential to derive aerosol extinction profiles. Give references. There are papers from Ansmann and from Flamant in Appl Opt. and Tellus A.

References and some text have been added.

Methodology:

The history of HSRL needs to be better introduced and discussed. Papers from She et al. from the 1990s, Hair et al., Appl. Opt, 2001, and earlier Eloranta papers (with Grund, with Piirionen) need to be cited and discussed. This should cover hardware developments as well as retrieval techniques. Eloranta (2005) is not an adequate reference, provides just a brief summary or introduction. This is not sufficient.

The HSRL-2 team is of the opinion that this overview fits better in the paper that will be prepared by Hostetler et al.

And the new 355nm channel! To say, the novel channel at 355 nm will be discussed in a separate publication is not acceptable. Please provide at least one paragraph on this.

Unfortunately I cannot provide this text, decision by the PI of HSRL-2.

You do not discuss the unique depolarization measurements. You measure depolarization ratios at three wavelengths. This is a strong point. Why is that not used, not presented? Even if not used, please explain, how that is realized.

We did not use these data as the depolarization did not show that depolarizing particles were present in a significant concentration. Thus there is no need to explain this channel in detail in the context of this paper.

It seems that paper is just a first, quick publication with some easy-to-produce products, and not a well prepared manuscript. This is not good and readers may get a negative impression of the NASA activities.

The initial idea of this paper is to provide the very first measurement example of the airborne multiwavelength HSRL without writing too much about instrument and software development and historic events. We hope the reader has the trust that this is an experienced team of lidar and software developers. We prepare manuscripts that will provide detailed information on HSRL-2 and the software. This paper is by far not a quick publication.

The optical data were analyzed several times since 2012 with improved data analysis software that is used for HSRL-2 signals. The challenge of this work was, as always, the task to receive high quality in-situ data from the aircraft team. It was a challenge to convince the in-situ team to reanalyze their data with regard to relative humidity effects, particle loss of the aircraft inlet, signals averaging, picking the correct size range of the investigated particle size distributions, combining data of different instruments so that "complete" particle size distribution were available. We had to select the correct flight track of HSRL-2 in terms of distance to the spiral flights of the G-1 aircraft in order to obtain the optimum overlap between profile measurement and spiral measurement.

In addition, the decision with regard to text that can be inserted in this paper is based on the opinions of the team members and the final decision by the HSRL-2 PI after he read the reviewer comments.

How can you achieve uncertainties of 0.1-3% for the optical data (backscatter and extinction coefficients)? The calibration (backscatter coefficient, particle reference value in the free troposphere) and the use of temperature and pressure profiles in the retrieval of the extinction coefficients introduce already errors at least of the order of 5-10%. Backscatter coefficients at 1064 nm can never be obtained with accuracies of less than 10%.

The errors were reduced as much as possible by data averaging in order to provide the best possible quality of the optical data that serve as input to the software. We mention this fact in the paper. Regarding the other points raised by the reviewer, i.e. more details on the data analysis: I regret to say that this part will be covered in the instrument paper.

Page 1063, line 15 to page 1064 line 5: These paragraphs should be moved into the introduction. If HSRL has so large advantages why do we see only a few systems running?

We removed these paragraphs and rephrased the introduction.

In contrast, why do we have so many Raman lidars running, although it is of disadvantage. Please discuss more seriously, along realistic facts. It depends on the application! I would never run a HSRL, if I just want to collect aerosol climatological data which I can best get with robust Raman lidars. But in the case of aircrafts, HSRL is of clear advantage. . ., or better there is no alternative.

We added text in the introduction.

Please provide some references (conference abstracts, ILRC?) for the new HSRL.

One website link to AGU 2012 was added at the beginning of section 2. http://abstractsearch.agu.org/meetings/2012/FM/sections/A/sessions/A13K/abstracts/A13K-0336.html. Most information can be found from progress reports and workshops in terms of power point presentations and posters on the web. We think that it is not useful to refer to this "very grey" literature in this paper. The instrument will be described by Chris Hostetler who is preparing a publication.

Page 1064, lines 4-5: "Details on this automated software and the results of the simulation studies will be given in a future publication00. This statement is not acceptable. Please provide a useful introduction (one or two paragraphs, if possible with references to conference proceedings. . .). Otherwise, this short publication is useless at all (something like a black box: results are shown, but in which way, is not said). This is unsatisfactory.

Text on the automated, unsupervised inversion software was inserted. Details will be presented in two contributions that are in preparation. We mention this fact in this manuscript. These two contributions will summarize 3 years of simulation studies.

Page 1064, lines 10-13. I do not understand, the aerosol in situ observations cover the particle radius range from 50 nm to 5.35 microns, and the HSRL data inversion radius range is from about 50 nm to 6 microns. Isn't it better to consider even large particles with radii up to 10 or 15 microns (you may have mineral dust!),

There were none in a significant amount. The comparably high Angstrom exponents also show this, though there were a few "outliers" that showed lower Angstrom exponents. The linear particle depolarization was below the threshold that would point at the presence of significant amounts of dust. In-situ data did not show dust in significant concentration (regardless of possible particle inlet problems). The data inversion considers particles up to 8-10 micron particle radius. We did not find robust results indicating particles above 5-6 micron radius. There were some layers in which we found particles (effective radius) larger than 0.3 micrometer. These "outliers" might be "real particle conditions" or just reflect uncertainties in the retrieval algorithm and the in-situ measurements. We have insufficient information if these larger effective radii reflect robust results. The variation of the individual in-situ measurements was considerable in some of the height bins chosen for the data inversion (150 m).

and then consider for comparison only HSRL results fore particles with radii of less than 5 microns?

6 micron was the closest retrieval bin in the inversion software compared to the 5.35 radius bin from the in-situ data up to which we could identify a significant number of particles; above that size range there was only noise in the data inversion. Thus, like in the paper by Wandinger at al., JGR, 2002 (LACE 98 results) we computed the parameters from inversion and in-situ for the same size bins. The slight difference of radius bins from the two methods is insignificant and could never explain any significant deviation of the results between inversion and in-situ measurements.

There is no compact overview of errors (optical as well as microphysical properties), here, in this section. Please provide all error information within one or two paragraphs.

We added text in the inversion section. Text regarding errors has been given in the results section. An overview on errors from HSRL-2 will be given by Hostetler et al.

Results:

Did the in situ observation always indicate that there were no large particles, i.e., particles with radii larger 6 microns?

There were no particles above 6 micron in the cases considered here. We added some text in the results section. The analysis was carried out very carefully, we considered cut-off effects of aircraft inlet, calibration curves from aircraft instruments, collection efficiency, artifacts of the in-situ instruments, and errors that might arrive from the inversion as the algorithm works on the basis of bins (triangle functions), as well as humidity growth factors that need to be considered in the analysis of the in-data.

What happens when you ignore particle with radii larger than 6 microns in the inversion, but they are present?

There were no particles present in a significant amount above the noise level of the in-situ instrumentation. Therefore no such analysis was needed.

Figure 2 is a nice and a convincing figure. Figure 3 is not so nice. The y axis indicates effective radius, but only for the first two plots.

Done

Better put all figures b,c,d,e in another Figure 4.

Done

Reply to reviewer 2:

The only confusion is the fact that the automated software, the instrument itself, the uncertainties and sensitivity studies are not presented nor referenced.

We added text that the papers are in preparation. See also our comments to reviewer #1 on that matter. He raised the same concerns.

Page 1061 line 1- The authors state:". . . measurements acquired by the world's first airborne multiwavelength High Spectral Resolution Lidar (HSRL-2)" I am not sure if in here the word "first" is related to the airborne or HSRL; probably neither since there is also a HSRL-1 ; actually the "first" measurements are presented so the authors should rephrase

We changed the sentence.

Page 1061 line 9 –The authors probably meant "retrieved" instead of "retrieves"

Done

\The introduction is really short. It should include a short discussion on the needs with respect to in situ observations and occurrence , improving and extending what has been done already, as well as need for modelling/understanding interactions and radiative forcing.

We added text with regard to the comments made by reviewer one. We think comments with respect to in-situ measurements should be kept to a separate paper. This manuscript is about the first measurements of HSRL-2, that these data can be used for data inversion with an unsupervised inversion algorithm, and the need to carry out tests if such software can be used for space borne applications. The software's data products in this contribution were compared to in-situ observations. A detailed overview with regard to comparison studies will be given in a publication in which we will summarize the results of 4 airborne campaigns: TCAP, DISCOVER AQ (2013) in California, DISCOVER-AQ in Texas (2013), and DISCOVER AQ in Colorado (Jul/Aug 2014).

It may be appropriate also to include in this section some background on what is done now as retrieval algorithms for microphysical properties from multiwavelength lidar data.

We added references, we modified the introduction, and we added a new section on the inversion software.

Also some references related to the use of the two aircrafts in research flights related to the paper main objectives.

We are not sure what the reviewer suggests. We refer to Berg et al. (2014) in the introduction. There, the reader will find the motivation for TCAP. The main objective of using two aircraft in TCAP was not related to the main purpose of this paper which is about the presentation of the first measurement example of HSRL-2 and the first application of the automated data inversion software. However, TCAP was an excellent opportunity to make use of this measurement configuration (2 aircraft).

\\Page 1062 line 10- you should probably put a reference with a detailed description of the HSRL1

We inserted the reference regarding HSRL-1 which is described in detail there. With regard to a detailed description of HSRL-1 or HSRL-2: we refer to our reply to reviewer 1.

\\So there are few thinks that can be noted after the first reading of the paper-description of the instrument, data analysis algorithm (the prototype software), sensitivity studies, uncertainties studies will be presented in future studies (so not here!), but I think in several places more details should be given as for e.g.:

We added text in section 1, and section 2.2, inversion algorithm.

\\Page 1062 line 21-22 since there is no reference and no demonstration of this statement we have to just believe that uncertainties for optical data are in the order of 0.1–3 % .

We added text in section 2.1 with regard to the small errors. We averaged the optical data so that we could use the data under the best possible circumstances. In our simulations we consider measurement errors up to 20%. We will show the simulation results in a separate publication.

\\page 1062 line 24 I could not find in Ansmann and Müller, 2005 "the requirement set out for trustworthy microphysical retrievals" so please be more specific

We rephrased the sentence.

\\page 1063 line 5- you mention in here the aerosol typing algorithm of Burton et al. 2012 but you haven't use it to assess the aerosols types in your measured data sets which could be a good asset

We will do this in a future stage of software development. We are currently exploring several options. At the moment we want to make use of mathematical constraints in order to improve our inversion results. We inserted another figure which shows the results of aerosol typing, see figure 3.

\\page 1063 line 8- in the paper bu Ansmann and Muller 2005 there are several types of algorithms described. I think it will be helpful to be more specific in here; a bigger paragraph on the algorithm please; description along with other references where the algorithm is specifically described would be nice

We explain in more detail in section 2.2 the inversion software.

\\page 1063line 15-please insert references to prove the higher SNR

We refer to our reply to reviewer 1

\\No information is given on the range of the measurements. I would guess 0 to 4 km from the curtains representation but it should be mentioned in the text

We do not understand? Do you mean height above ground, or distance flown? We modified the curtain plots. They now show the distance flown during the measurements shown in figure 2 and figure 4.

\\Related to Results and discussions It will really help the reader to understand your results if you present a map with the aircrafts tracks

We inserted new figure 1.

and highlight the measurements you considered for your inversion algorithm

Time and location are given in the figure caption of figure 4 and 5. The curtain plots provide measurement time on the top x-axis.

\\Page 1066 lines -51 Please provide backward trajectories and fire map to convince the reader your assumption (regarding the type of aerosols)is correct

We refer in several spots to a paper that is currently under review: Berg et al. (2014). The paper contains all information requested by the reviewer. We refer in our paper to the plots and conclusions made by Berg et al.

\\Page 1066 lines 6-13 Only 7 lines regarding the automated algorithm; very difficult to understand exactly how this automated actually works.

We are preparing two publications on this algorithm in which we show simulation results. We tried to answers some questions (see also comments made by reviewer one) in our new section 2.2 which is about the inversion algorithm.

You divide in 150m arbitrary layers? Are you sure there is aerosol in these layers?

We refer to the curtain plots shown in Figure 2 and Figure 4. Aerosols were found up to 3.5 - 5 km in variable concentration. We do not use arbitrary layers. We used profiles smoothed with 150 m window length and we carry out the inversion for this set of data. The window length was small and aerosols were found throughout the aerosol layer. We did not have to go through the comparably time-consuming manual identification of aerosol layers that needs to be done for ground-based multiwavelength Raman lidar. This was possible because of the high signal-to-noise ratio and the favorable aerosol conditions.

Do you set any thresholds for SNR in each of these layers that I understand they become ”data sets” input in the automated software?

No threshold is used except that we use data with errors less than 15% which was easily fulfilled in the present case. We will investigate this SNR challenge in more detail in the context of our analysis of data we collected during other campaigns: DISCOVER AQ California (Jan/Feb 2013), DISCOVER AQ Texas (Sep 2013), and DISCOVER AQ Colorado (Jul/Aug 2014).

This is the first time I see that this algorithm is used for arbitrary layers. Can you point out any references where this has been done before? If not than the results of the simulations should be at least mentioned here.

It (arbitrary layers) has not been done before. We use this kind of “arbitrary layer selection” in the context of a different prototype of our inversion software in which we use profile segments for data inversion. However, this prototype is not yet part of our automated software that is developed for the airborne instrument, and we think it is out of the scope of this paper if we refer to these references (Kolgotin and Müller, Applied Optics, 2008 and Müller et al., Applied Optics 2011).

HSRL-2 is the first MWL lidar that provides us with a data quality that might allow us to avoid layer identification to some extent in future. The challenge with regard to Raman lidar is the comparably low signal-to-noise ratio. The noisy data, among other factors, still make it too difficult to use this concept of choosing “arbitrary layers”; these challenges with regard to Raman lidar might be overcome in future. Thus, layer identification is needed for Raman lidar in order to provide trustworthy inversion results.

Nevertheless we will also develop tools for layer identification in the case of HSRL-2 data. An idea on how this could be done is based on the aerosol typing concept of Burton et al. (2012). More tools will be developed for the space-borne applications which are the goal of this software development. We will need the layer identification as we plan to develop the inversion software such that it can be used for combination of lidar (vertically resolved profiles) with radiometer (column integrated data products). In contrast to the currently used layer identification for Raman lidar in EARLINET we will make the layer identification unsupervised and automated.

\\Page 1068 line 25 you haven’t provide any proof(references) in the paper to support your statement” Ångström exponents are in agreement with literature values of urban haze and/or biomass-burning smoke”. Also the paper by Muller et al. 2007 presents the effective radius for biomass burning events and combined with your observed “ lidar ratio at 355 nm was similar or slightly lower to the one at 532 nm” could lead to decision that your measurements are related to biomass burning event. Why not?

We rephrased our sentences. Chemical analysis points at urban haze and biomass burning. We inserted the conclusions drawn by Berg et al. (2014) on that matter.

\\Page 1069 line 4 –In this paper “the curtains” presented are not related to the microphysical parameters as you state (see Figure1)-please clarify this

Figure 2 shows a section of the flight leg outbound over the ocean. We inserted another curtain plot that represents the profiles used for the data inversion, see figure 4.

\\Page 1069 lines 7-10 you should provide references for this statement

It is unclear what the reviewer means? That the data are already being used for the studies mentioned in this sentence, or the fact that the data allow for such studies? We rephrased the sentence.