Interactive comments on "Scattering and absorption properties of near-surface aerosol over Gangetic–Himalayan region: the role of boundary layer dynamics and long-range transport" by U. C. Dumka et al.

Responses to comments by Anonymous Referee #1

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We would like to express our sincere gratitude to the anonymous referee for his/her insightful and constructive comments on this study. All the comments and suggestions are highly valuable for us to improve the quality of the manuscript. A point by point response is included below. The comments are indicated in black and our responses are in blue.

Review of "Scattering and absorption properties of near-surface aerosol over Gangetic-Himalayan region: the role of boundary layer dynamics and long-range transport" by Dumka et al. Manuscript number: acp-2014-521.

The manuscript presents the results from approximately 1 year of aerosol measurements performed during the GVAX campaign at Nainital. Measurements include aerosol scattering and absorption for particles below 1 um and 10 um in diameter.

The paper needs major revisions before it can be published in ACP.

The main issue with this manuscript is that some of the results were already presented in a recent paper published by Dumka and Kaskaoutis (2014).

In the Abstract the authors write that "The present study examines the temporal (monthly, seasonal) evolution of scattering and absorption coefficients, their wavelength dependence. . ." This was already presented in Dumka and Kaskaoutis (2014) together with the analysis of the scattering Angstrom exponent.

Thus, given the length of the manuscript, I suggest resuming in the Introduction the previously published results (obtained using the same database), shortening the present manuscript and avoiding repetitions. For example, the important differences between D_1 and D_{10} optical properties at Nainital, as well as the effect of meteorology (monsoon vs. post-monsoon seasons) on aerosol extensive optical properties, were already presented in Dumka and Kaskaoutis (2014). Once the main findings from published articles are presented, the authors can better organize the manuscript presenting new results (which include analysis of AAE, backscatter, submicron scattering and absorption fractions) and avoiding repetitions.

Authors: The previous published article (Dumka and Kaskaoutis, 2014) emphasized on the temporal evolution of single scattering albedo (SSA) and aerosol radiative forcing efficiency (ARFE), using the scattering and absorption coefficient measurements. In order to support that analysis, the monthly variation of both scattering and absorption was presented before the results of SSA and ARFE.The present work does not reiterate those results, but exhibits (for the first time) the diurnal variation of scattering and absorption along with their wavelength dependency. Some references to the monthly variation of scattering and absorption coefficients are logic to exist in the current paper, which are all accompanied by the reference Dumka and Kaskaoutis (2014). In the Introduction section, we summarize the already published results from GVAX campaign (Dumka and Kaskaoutis, 2014; Manoharan et al., 2014). The whole manuscript has been edited and modified according to the reviewer comments. The main findings and highlights of the present work, i.e. the role of the boundary-layer dynamics and long-range transport are underlined at the end of the Introduction section, as well as in the Abstract and Conclusions and are further supported by the inclusion of two new figures (Figures 8 and 10). This supports the innovation of the present work and prevents from any duplication with previous results.

The Introduction should resume the main findings from previous studies. For example (Page 21104, Lines 1-7) the main results from Panwar et al. (2013), Komppula et al. (2009) and Neitola et al. (2011) should be discussed in the Introduction.

Authors: The objectives and main findings from previous studies are briefly discussed in the revised manuscript.

The Abstract should be rewritten and it should present the novelty of this manuscript. As such, the Abstract only presents a list of measurements/analyses performed (some of these already presented in Dumka and Kaskaoutis (2014)).

Authors: We have improved the abstract, which now includes the main findings and highlights of the present work, i.e. the influence of boundary-layer height and dynamics in the diurnal variation of the aerosol properties and the role of long-range transport.

The second issue, which has been not yet addressed by the authors, is related with the comparison of optical properties (both extensive and intensive) measured at Nainital with those measured at other mountain top sites worldwide. This will improve the scientific quality of the manuscript. To my opinion this is very important given the peculiar characteristics of aerosols in the Gangetic-Himalayan region in terms of scattering and absorption.

Authors: The Table 2 has been enriched with aerosol properties from more mountainous sites over the globe. The comparison shows that Nainital, although it is far from urban/industrialized sites, exhibits higher values of scattering and absorption corresponding to more turbid atmospheres due to the strong influence of transported aerosols from the Ganges Valley and southwest Asia. This is briefly discussed in the revised manuscript, and from the values summarized in Table 2, readers can easily exclude their conclusions.

Another issue is related with in-cloud data. I have understood that "handling such data is outside the scope of the present study", but it would be useful to know how (and if) authors detected and removed the in-cloud data from the database.

Authors: The present analysis did not include or even being involved with in-cloud data. The methodology for performing the Nephelometer and PSAP measurements is fully described in the current manuscript, as well as in the previous publications by Manoharan et al. (2014) and Dumka and Kaskaoutis (2014). The in-cloud data were removed from the analyzed data series by the AMF technical staff.

Moreover, Table 1 shows the set of optical parameters derived during the GVAX campaign. Some of these (i.e.: up-scatter fraction, asymmetry parameter and hygroscopic growth factor) were presented in Table 1 but not discussed in the present manuscript. The backscatter Angstrom exponent is highlighted in Table 1 but not presented in the manuscript.

Authors: Actually, several parameters have been measured or calculated from the initial experiments during the GVAX campaign, which are impossible to be presented and discussed in a single paper. Thus, SSA and ARFE were well documented in Dumka and Kaskaoutis (2014), while CCN concentrations and growth factor are the topics of forthcoming research (on going analysis). For avoiding any misunderstanding, in the revised Table 1 we removed all the parameters that are not analyzed in the present work.

Among the intensive aerosol optical properties available from the GVAX campaign, the SSA and scattering Angstrom exponents were already presented in Dumka and Kaskaoutis (2014), the scattering Angstrom exponent (with a few more details), absorption Angstrom exponent and hemispheric backscatter fraction were presented in this manuscript. The asymmetry parameter and hygroscopic growth factor are two important parameters derived from the GVAX campaign but not presented here. What are the reasons for this exclusion? To my opinion, adding these results will considerably improve the scientific quality of the present work.

Authors: Only a very brief discussion for the Scattering Angstrom Exponent (SAE) was given in Dumka and Kaskaoutis (2014), just for comparison purposes with columnar Angstrom exponent via MFR measurements. This parameter, along with Absorption Angstrom Exponent, etc. is analyzed in the current work. Some preliminary results of the hygroscopic growth factor are already with us after comprehensive analysis of this dataset, but their inclusion in the current work will enhance strength of the manuscript but disorientate the main objectives. In order to improve the scientific quality of the present work and to emphasize more on the significant role of boundary-layer dynamics, long-range transport and source regions, two new figures (as stated above) have been included.

The authors could remove Figures 11 and 12 (which do not add relevant additional information compared to what already discussed in the manuscript) leaving space for additional results.

Authors: We removed these figures (11 and 12), as well as the figures 3 and 6 of the original version, as suggested by the reviewer.

Moreover, it would be nice to know if the aerosol absorption at Nainital shows any trend given that absorption measurements are available since 2004 at this site (See Table 2 of this manuscript).

Authors: We cannot perform such analysis in scattering and/or absorption coefficients near the ground, since the measurements are not continuous. The previous measurements over the site (included in Table 2) correspond to results from previous campaigns performed at the specific time intervals. Overall, there is no continuous database of absorption coefficient measurements at Nainital that allows any trend analysis.

Another issue is related with Paragraph 3.2.2. The SAE measured at ground is higher during Monsoon and lower during post-Monsoon season. The former was related to the removal of aerosol accumulation mode by the rain, the latter was an indication of abundance of aged aerosols at the site. As stated by the authors these results deviate from those obtained using columnar data (Guleria et al., 2011; Dumka et al., 2008; Srivastava et al., 2012) showing lower columnar Angstrom exponent during pre-monsoon and monsoon, due to the influence of dust, and larger columnar SAE during post-Monsoon season, due to the dominance of anthropogenic aerosols and biomass burning.

However, this difference between ground and columnar SAE is not as evident looking at Figure 7a in Dumka and Kaskaoutis (2014) where the seasonal evolution of columnar SAE and ground PM1 SAE during GVAX seems to agree quite well. What’s the reason for this? Is this due to different periods analyzed in these different papers?

Authors: It is true that previous studies have shown lower columnar Angstrom exponent values during monsoon compared to post-monsoon and winter. However, the columnar monsoon measurements were rather rare due to mostly cloudy skies. The present results show higher values of SAE during monsoon, which are in agreement with the measurements at Mukteshwar due to the reasons explained in the manuscript. The coincidence in the monthly variation between the near-ground and columnar SAE values shown in Dumka and Kaskaoutis (2014) may be the result of a specific year only (GVAX campaign). However, note that there is lack of columnar Angstrom exponent values in June and July, while the columnar values in December are the highest, which is not coincident with that observed in surface. In synopsis, for avoiding any misunderstanding and confusion between columnar and near-surface aerosol properties, this sentence has been removed in the revised manuscript.

How many dust episodes were detected during the study period reported in this manuscript?

Authors: We did not analyze any individual case, since this is beyond the scope of the present manuscript. So, we do not have a number of dust episodes (if any) over the site. The dust episodes should be defined by specific criteria, i.e. MODIS imagery, visibility records, peaks in aerosol loading and scattering data series, etc… However, since the main dust period over northern India (April-June) is only partly examined (June measurements), intense dust storms were not detected over the site.

How much the seasonal evolution of the PBL is affecting the intensive aerosol optical properties measured at ground compared to the columnar ones?

Authors: The new Figure 8 clearly reveals that the PBL strongly affects the extensive aerosol properties and only partly the intensive ones. For the respective columnar analysis, we need measurements from spectral absorption and scattering coefficients from satellite sensors (i.e. OMI) and/or Lidar observations for the vertical atmospheric structure. Satellite observations have more uncertainties and these issues cannot be examined in greater accuracy. Analysis of the diurnal evolution of the aerosol scattering (on ground) and AOD (in vertical) as a function of the evolution of the mixing-layer height maybe an interesting topic for further research.

The last issue is related with the effect of LRT and PBL on the measurements presented in this manuscript. LRT and PBL effects are highlighted in the title and abstract. However, only a small section (Paragraph 3.4) is dedicated to these issues. This part should be improved for example adding some cluster analysis using backtrajectory analysis and not only using wind data.

Authors: We spent our main efforts on improving this part of the manuscript. In this respect, two more figures are included in the revised manuscript (Figs. 8 and 10), highlighting the role of boundary-layer height and dynamics as well as long-range transport and source regions (CWT analysis). We chose the CWT analysis instead of trajectory clusters in order to reveal hot-spot areas favoring the dominance of specific aerosol properties over Nainital.

We believe that the new inclusions, modifications and general edits in the manuscript help in its improvement and would be accepted by the reviewer. We are very much grateful to the reviewer's comments.

Interactive comments on "Scattering and absorption properties of near-surface aerosol over Gangetic–Himalayan region: the role of boundary layer dynamics and long-range transport" by U. C. Dumka et al.

Responses to comments by Anonymous Referee #2

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The authors of the manuscript acknowledge the reviewer for carefully reading the original manuscript and providing the constructive comments. A point-by-point response to all the comments follows.

Review of "Scattering and absorption properties of near-surface aerosol over Gangetic-Himalayan region: the role of boundary layer dynamics and long-range transport" by Dumka et al.
Manuscript number: acp-2014-521.

The manuscript deals with a topic appropriate for ACP presenting the analyses of aerosol optical properties gathered during an experimental campaign in the Gangetic-Himalayan region. Although the study period roughly covers a complete annual seasonal cycle, the variety of variables potentially available for the study makes it really worthy. Bellow there are general and particular comments to help the authors in improving their manuscript. According to these comments, I consider that the manuscript requires substantial revision before to be accepted for publication in ACP.

Authors: We are grateful to the reviewer for his/her valuable and to-the-point comments that helped us in improving the scientific quality of the paper. We took the whole comments into consideration. Before our specific responses, we may mention here that the whole manuscript has been edited and revised substantially from the beginning, avoiding some repetitions and removing figures of lower importance (also following the suggestions of Reviewer A), like Figs. 3, 6, 11 and 12 of the original version. This gives us the possibility of extra analysis and presentation of two new figures (Figs. 8 and 10 in the revised version), highlighting the importance of the boundary-layer height and source of long-range aerosol transport as stated in the title.

General comment

The experimental data set is really interesting, but the study will benefit from amore exhaustive analysis of some aspects like the PBL study or the use of additional data described in table 1 but excluded from the present study, like the hygroscopic growth information. Furthermore, as reviewer #1 states the authors must clearly distinguish the outreach of the present manuscript from the previous one Dumka and Kaskaoutis (2014) where some analyses of the same data set were presented.

Authors: As stated above, we included two new figures in the revised manuscript for the PBL height and long-range transport (CWT analysis). The current manuscript is considerably long and does not allow us to include comprehensive analysis of the hygroscopic growth factor, etc. The hygroscopic growth factor is under collaborative analysis with another scientific team, and its results are expected to be released soon. Thus, that analysis cannot be included in the current work. In the Introduction section, we clearly stated the results by Dumka and Kaskaoutis (2014) and clearly separated them from the current analysis. Note that the temporal variation of the scattering and absorption coefficients is very briefly discussed in the current work, since it was covered in our previous paper.

It is worthy to note that along the text there is some confusion on the interpretation of the separation of data measured with PM_{10} and PM_1 cut-off. It seems that the authors consider data measured with these two cut-offs as completely separated categories, while in fact the larger cut-off allows measuring the effects of micron and submicron particles at the same time. An effective characterization of micron and submicron particles could be only got by subtraction of the PM_{10} and PM_1 associated variables.

Authors: Authors agree with the Reviewer. The properties of the $D_{10\mu m}$ aerosols also contain those of $D_{1\mu m}$, but the influence of the super-micron aerosols seems to be larger. This has been brought out in the revised manuscript. However, during the whole discussion, we made a clear separation between the properties of $D_{10\mu m}$ and $D_{1\mu m}$ particles in order to avoid any confusion on the interpretation of the results.

Particular comments

Along the manuscript the authors use the term parameter to describe the variables the measure, compute and analyze. They must revise this misuse considering that they measure different variables that characterize the atmospheric aerosol properties. Some of these variables are used in different models, climate models, as parameter of the models and in this use the values are fixed according to a given choice of aerosol type for example. But in this study they determine the experimental values of these properties, so the term variable is the appropriate.

Authors: We agree with the reviewer and we replaced the term “parameter” “variable”.

The treatment of the PBL information is really superficial in the paragraph in page 21118 in spite of the relevance evidenced in the abstract. Details on the source of this information are required together with a more detailed discussion.

Authors: This part of the manuscript has been substantially revised in view of the inclusion of the new Figure 8. The information of the derived PBL height is also indicated on the figure in the revised manuscript.

Table 1 describes a large number of variables that can be derived from the experimental data set. In this sense, it is clear that the potentiality of the data is not completely exploited by the authors in the manuscript, read my general comment. In my opinion the study will benefit of the analyses of additional variables like the hygroscopic growth factor that will compensate of the limited period of measurements and will contribute to enhance the advances over the previous study on the same experimental campaign (Dumka and Kaskaoutis, 2014). Furthermore, a good number of the variables included in the table are derived from experimental values assuming some empirical relationships that require the inclusion of the appropriate references to track their meaning and relevance.

Authors: It is really impossible to include a comprehensive analysis of all the measured and calculated variables in a single paper. Some of them, like scattering and absorption coefficients, SSA, ARFE have been documented in a previous paper (Dumka and Kaskaoutis, 2014). The analysis of hygroscopic growth factor is in progress and the results will be published in near future. To avoid confusion, we removed from the Table 1 all the parameters that were not analyzed in the current manuscript.

A relevant question concernsthe statistical information. In this sense, by one hand the information included in Box and whisker charts must be explained, see figure 1 and 2, the meanings of the elements in this figure are similar? Why they are described for figure 2 for the first time? On the other hand, along the text when the authors present average values, for example in their description of the meteorology it is necessary additional statistical information offering the reader a complete picture of the variability of each variable.

Authors: The box and whisker charts in Figs. 1 and 2 are similar. Figure 1 has been modified and this information is now given in the Figure 1 caption.

Concerning quantitative aspects the authors present a chaotic treatment of the significant figures of the measured variables. When they combine the information on average values and associated standard deviations they sometimes presents different number of decimals or significant figures for then (for example Line 17 page 21112, 1.5+-0.09). In other cases they express the results with an excessive number of significant figures (Line20, page 21110, 75.2+-41.7). They must carefully revise these expressions avoiding the use of more than two significant figures for the standard deviation, as a measure of the spreading of the data. In fact, they must reduce the number of significant figures to only one if the most significant figure is larger than 2. And finally, there must be an agreement between the significant figures used for the average and standard deviation. So the previous example must read 80+-40 (clearly evidencing the large spreading of data around the average value).

Authors: The presentation of the results (values +/- stdev) has been revised in order to attain coincidence between the decimal digits.

Furthermore, the authors must be careful with the uncertainties of the experimental values and those of the derived variables. In this sense for example the description of the Angström exponent for the scattering or absorption coefficients with more than one decimal figure is inappropriate, especially for the absorption coefficient, considering the large uncertainty of the experimental values used in their computations. So in this sense for the example on figure 21112, see about, 1.5+-0.1 would be more appropriate than 1.50+-0.09.

Authors: This issue was also incorporated in the revised version.

Another point that must be carefully revised is the use of duplicated notation for the same variable, for example he authors must revise the notation for SAE and AAE that is not coherent along the text, figures and tables.

Authors: In the whole manuscript the terms SAE and AAE are used for the scattering and absorbing Angstrom exponents, respectively.

Concerning the figures, by one hand the authors must apply the previous comment on significant figures, but also must revise carefully the inclusion of information on the units used for the different variables, see for example figures 9, 10 and 11.

Authors: Some figures have been revised as suggested.

The last paragraph previous to the conclusions section (Pages 21121 and 21122) is a little bit confusing. In fact the comment is on figure 13 that for me is an example of figure that can be excluded from the analyses. The spread of the data in this figure hardly allows deriving any dependence between the analyzed variables. In this sense, the above mentioned paragraph must be carefully revised or excluded from the analyses.

Authors: Finally, we decided to maintain this figure and to remove the previous 2 (Figs. 11 and 12). Figure 13 shows a rather well-mixing of aerosols over the observational site and this is an important finding that warrants discussion at the end of the manuscript. Overall, this paragraph has been edited and modified in the revised manuscript.

The author must track the typos along the text. An example is the term “isentropic” in line 19, page 21114, they must correct the word otherwise they need to explain the meaning of “isentropic scattering”.

Authors: The term isentropic scattering is explained with parentheses as (homogeneous scattering at all directions).

Interactive comments on "Scattering and absorption properties of near-surface aerosol over Gangetic–Himalayan region: the role of boundary layer dynamics and long-range transport" by U. C. Dumka et al.

Responses to comments by Anonymous Referee #3

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There are many critical errors with use of the data. It doesn't seem as though the authors used the most recent edited, corrected data from the ARM archive. The data during this campaign were compromised and require substantial corrections. Even with these corrections I have reservations about the data quality. A few comments on the paper

In the "Measurements and Techniques" section there are several errors which led me to believe that the authors didn't use corrected data from the ARM data archive.

Authors: We thank the anonymous referee for his/her comments and suggestions that helped us to improve the quality of the manuscript and to remove some errors made by disregard in the "Measurements and Techniques" section. A point-by-point response to all the comments follows.

We have noticed that there are several versions of the data from the Aerosol Observing System (AOS) in the ARM archive and we have double checked the datasets before its use in the current manuscript by discarding the contaminated data and by applying correction factors. The data have 1-min time resolution and mentor-QC applied already. These datasets are included in the final netcdf files of the GVAX campaign and are considered of acceptable accuracy for atmospheric research community. Of course, certain uncertainties in the experimental procedure cannot be avoided and they are clearly stated in the manuscript along with the appropriate references from similar measurements performed worldwide. It should be noted that the corrections that were made in the text do not affect the measurements and analysis in the whole paper.

1. The inlet pipe was not "Stainless steel" but powder-coated aluminum pipe with an 8.0 inch or 20.32 cm ID. This is standard sewer pipe for the US. I don't know where they obtained the other descriptions.

Authors: According to the AOS handbook (Jefferson, 2011), in Page 10, section 7.2 "System Configuration and Measurements Methods" it is noted "*The sample air flows through a 2" diameter stainless steel pipe in the center of this larger flow*." The same (stainless steel) is justified by Titos et al. (2014) for measurements in Cape Cod, where the AOS system was transferred after the GVAX campaign. We changed the diameter of the pipe to 20.3 cm, sorry for our previous typographical mistake.

2. There was no metal screen on the inlet as stated by the authors for this deployment.

Authors: We are extremely sorry, again, for this mistake by oversight. This sentence has been deleted in the revised version.

3. The data alternate ever y 30 minutes between sub10 and sub 1 um aerosol and not every 5 minutes as stated in the paper. This leads me to believe that they didn't correctly parse the data along the aerosol cut size. As there was a substantial difference in the signal in these two size modes, I question the accuracy of the data.

Authors: We are grateful to the reviewer for this notice. The sentence has been rewritten according to the AOS handbook, with 30-min alternate between D_1 and D_{10} particles. The datasets that were obtained are in 1-min time interval clearly differentiated for D_1 and D_{10} particle groups, i.e. in the netcdf file when a measurement is given for the D_1 there is -9999 for the D_{10} and vice versa. This shows that the dataset for the two groups has been clearly separated and we used it correctly. In the current analysis, the hourly-averaged data for D_1 and D_{10} were used.

4. The PSAP was operated at a flow rate of 0.7 to 0.8 lpm and not 1.5 lpm as stated in the paper. The flow rate is in the PSAP data file which makes me question if the data was accurately flow-corrected.

Authors: We corrected the flow rate in the revised manuscript as per the comment. Thanks. However, we have used the flow-corrected data as given in the netcdf file of the campaign.

5. The authors didn't use the Bond et al. correction to subtract the effect of aerosol scattering in the filter medium. They state that this is an "additional bias". This is substantial bias and will affect calculation of the wavelength dependence or absorption angstrom, AAE.

Authors: We agree with the reviewer that this is a substantial bias. However, the Bond et al.'s correction has been applied, as stated in the very next sentence "The total uncertainty of the PSAP measurements after the transmission and scattering correction is ~20-30% (Bond et al., 1999)". Maybe the sentence started with "Additional biases" confused the issue, so now, we modified it.

6. The CO_2 provided at the site for the Nephelometer calibration was either mixed with another gas or was of too low quality to provide a good calibration. As access to the data was denied during the field deployment this error wasn't discovered until the end of the field campaign and the data had to be corrected. This is problem is reported in a data quality report that accompanies data downloaded from the ARM archive. Data that doesn't use this correction has a 10-15 percent error.

Authors: We are grateful to the reviewer for this observation. As stated in the manuscript, the instrument's calibration with CO_2 has been applied, maybe without the expected accuracy. We noted this in the revised paper and the overall uncertainty in the Nephelometer measurements increases to 10-15%, which is within the acceptable error for ground-based aerosol measurements.

7. In the "Extensive Properties" section the authors report on "absorption efficiency" for the aerosol. This parameter requires measurement of the aerosol black carbon. Such measurements were not available during that field campaign

Authors: In that sentence we cite the results of Manoharan et al. (2014). The term "efficiency" was now replaced with "coefficient". Thanks for the comment.