Authors' response to the referees' comments on acp-2013-278

We would like to thanks the reviewers for the helpful comments. We believe that the changes applied to the paper according to these suggestions helped to greatly improve the quality of the manuscript. We individually answered to each comment in the discussion below.

 # Referee 1

General comments:
This study reports a seasonal variability of aerosol types using recent CALIOP observation data set. Parts of the manuscript are confusing (see comments below) and there are doubtless ways in which the identified aerosol types could be refined in future (evaluation using in-situ measurements). Nevertheless, I think the paper is interesting. The characterization of seasonal and spatial variability of aerosol types using CALIPSO make the manuscript potentially suitable for publication. However, the assessment of sources of aerosols during pre-monsoon season is confusing. I have outlined some specific comments below which I feel should be addressed prior to publication.

According to the specific comments we worked on the text to clarify the confusing parts and we added references that help discuss the evaluation of CALIOP data accuracy. Moreover, following the indications of the referee 2 we rewrote the abstract, introduction ad conclusion to better highlight the results in the frame of the current knowledge. Abstract report more explicitly the results and the objective of the work. The introduction was rewritten and some more reference were added to have a more linear structure and to expose in a clearer way the importance of the study presented and which are the new insights that this paper offers. Conclusion were also rewritten in a more concise way to get across the main message more directly.
We have addressed each specific comment in the following discussion.

Specific comments:
1. Page 15272, line 14: Quantify the dust amount.

We added the values of frequency of occurence through the paragraph.

2. Page 15274, line 26: What is CNES?

CNES: Centre National d'Etudes Spatiales. We added the specification in the text.

3. Page 15275, lines 27-29: Sentence is not clear. Rewrite the sentence.

We reformulated the sentence in a more linear way.

4. Uncertainties in trajectory increase with trajectory length. Provide justification for using 10 day trajectories to resolve transport patterns. In general, kinematic trajectories undergo considerably greater vertical displacements. How much uncertain is the estimated transport patterns?

10 days back-trajectories are necessary to investigate events of long-range advection like transport of Saharan origin. Surely the levels of uncertainty may be high. Stohl 1998 shows that ECMWF analysis may lead to an error in the trajectory position of less than 800 km and less than 3 Km in vertical for 10 days. The use of clusters of trajectories, as adviced in the above mentioned paper, helps however to highlight coherent tranport patterns: the synoptic transport in this region, as shown for example in Fig. 6 for April 2010, is a steady feature and we may expect to have a sufficient

accuracy in discriminate among different distant sources. Moreover the statistics are computed on a large time interval (3 months).

5. In this study, the analysis is limited to the transport above the Planetary Boundary Layer (Page 15276). How this will influence the BC local emission transport?

We want to avoid transport in the convective PBL. We assume that, below a PBL representative height, aerosol are effectively mixed, as is often seen from CALIPSO observations (not shown here). We assume therefore that transport of BC is confined around the region of emission. This will induce a possible uncertainty but, given the low precision in reproducing the convective transport inside the PBL, we don't expect to reduce the uncertainty adding the convective dynamics of the PBL in the analysis.

6. Back trajectories analysis is confusing. The starting point area is indicated by a blue shaded region. Clarify the starting point area in the manuscript. Why this starting point area is different in Figure 7?.

The starting point of the back trajectories is the region surrounded by the black box in figure 7. Figure 6 is an explicative image where a smaller region of particles release is used to better show the air masses dynamics around the Plateau. We clarify it in the text.

7. Smoke aerosol types influence is limited to summer and fall seasons (Page 15279). What is the reason for this? It is reported that carbonaceous aerosols are mainly produced from crop-residue, forest and bio-fuel burning sectors during winter and fall seasons over the south Asian regions. Why smoke influence is less during winter season? Whether the polluted dust type is misrepresenting the smoke influence (Fig 1)?

In the text we point out that the smoke inflence is limited to summer and fall season just above the plateau (6-8 km layer) when probably uplift transport is more effective. Near the ground instead (for example 0-2 m layer) is shown that the smoke (and also the polluted dust) is actually found with greater frequency during winter and fall season.

8. Page 15282, line 21: Add reference.

Reference to Stohl 1998 added.

9. Page 15284, line 19: Quantify the small amount?

We added the value of frequency of occurrence.

10. Page 15285, lines 14-16: Sentence is not clear. Rewrite the sentence.

We reformulated the sentence the text.

11. It would be useful if the authors can provide the comparison of total aerosol extinction/ optical depth with the AERONET observed aerosol extinction during 2007-2010. This will also address the issue of general accuracy of CALIOP aerosol extinction data (Koffi et al., 2012).
12. Several limitations in the CALIOP aerosol data were reported in recent literature's, including uncertainties due to finite detection sensitivity of the CALIOP instrument, uncertainties associated with selection of lidar ratios, and misclassifications of features (Koffi et al .,

2012; Winker et al., 2013). This discussion should be included in the manuscript.
13. The following papers should be cited in the manuscript:
Omar, A. H., D. M. Winker, J. L. Tackett, D. M. Giles, J. Kar, Z. Liu, M. A. Vaughan, K. A. Powell, and C. R. Trepte (2013), CALIOP and AERONET aerosol optical depth comparisons: One size fits none, J. Geophys. Res. Atmos., 118, 4748–4766, doi:10.1002/jgrd.50330.
Koffi, B. et al. (2012), Application of the CALIOP layer product to evaluate the vertical distribution of aerosols estimated by global models: AeroCom phase I results, J. Geophys. Res., 117, doi:10.1029/2011JD016858, 2012.
Winker, D. M. et al. (2013), The global 3-D distribution of tropospheric aerosols, ACP, doi:10.5194/acp-13-3345-2013

The discussion about the accuracy of the CALIOP data was inserted in the text with reference also to the paper cited and to Schuster et al. 2012 that presents the comparison of aerosol optical depth between CALIOP and AERONET measurements and lidar ratio for dust.

Technical errors:

1. Check the spelling of anthropogenic throughout the manuscript. (See Page 15273, line 2 ; Page 15276, line 17 & 23).

The mispellings were fixed.

2. In Fig. 1, What is white shade indicate?

White indicates absence of data. The specification was added in the caption.

3. In Figure 6, grey shades (BC emissions are uniformly distributed) are confusing.

Fig. 6 simply reports the area labelled as source regions of dust or black carbon. We don't give information on the heterogeneous distribution of the emissions, as specified in the text.

Reference*:*

Stohl 1998, Computation, accuracy and applications of trajectories – a review and bibliography. Atmospheric Environment Vol. 32, No. 6, pp. 947-966, 1998

Schuster, G.L.,Vaughan M., MacDonnell D., Su W., Winker D., Dubovik O., Lapyonok T. and Trepte C., Comparison of CALIPSO aerosol optical depth retrievals to AERONET measurements, and a climatology for the lidar ratio of dust, Atmos. Chem. Phys., 12, 7431–7452, 2012

 # Reviewer 2
General comments:
The presented paper investigates the seasonal variability of different types of aerosol focusing on dust, smoke and polluted dust in the Himalayan region. Due to the importance of the region the investigations are important and the paper focuses on an important topic, the aerosol influence on climate. However, from my side, there are still significant limitations in the presented paper.
From my point of view, the topic of the paper is interesting and worth to publish. However before publishing the paper needs significant improvements as stated above and especially in setting the paper in a broader scientific context. A clear message is miss-

ing.

The paper was restructured in all the parts mentioned in the comments to give emphasis to the most relevant concepts and results of the work. Abstract report more explicitly the results and the objective of the work. The introduction was rewritten and synthesized and some more reference were added to have a more linear structure and to expose in a clearer way the importance of the study presented and which are the new insights that this paper offers. Conclusion were also rewritten in a more concise way to get across the main message more directly. Moreover, accordingly to comment of referee 1, we also added a discussion on the evaluation of CALIOP data accuracy

Specific comments:

1) In both the Abstract and the Introduction the authors should more emphasis, why their study is so important and what is the exact gap of knowledge that will be filled with the paper. The paper should be better placed into the wider scientific content.
2) The introduction needs to be much more structured. I do not see a clear structure either in the literature summary on page 15273 and on page 15274 (lines 2-12).
3) In the introduction clearly the questions,that will be answered with this paper, should be stated.

Comments 1-3 were discussed in the general comment.

4) The section No.3 could also be more structured in a way, that makes it easier for the reader to follow the main results: Additionally, the authors jump between the notation of the seasons "summer","monsoon" and "JJA" (and accordingly to the other seasons). It would be helpful to the reader to have a persistent notation.

The notation was changed to a coherent notation throughout the section. Some modifications were added to make the text more clear.

5) The Figures could be significantly improved. In Figure 5 especially the variabilty for the upper atmospheric layers are really hard to see.

The figure was modified to be read easier.

6) The conclusion is more a repetition of the sections before and does not really conclude and does not draw the real conclusion, that gives further information for the climatic influences of the aerosol in this important region.

The conclusion was reformulated to be more concise and to highlight the main results, as discusses in the general comment.

7) P. 15275 line 23-25: How many profiles were removed?

Generally, over a mean of 65.000 profiles each month, less than 25 % of profile were removed because of the presence of opaque clouds. Some exceptions are present around the months of July and August when the percentage or cloudy profiles reach maximums of around the 30% of the total profiles. This was added also in the text.

8) P. 15276 line 1: 10 days seem quite long. Are the results still reliable?

As previously answered to question 4 of reviewer 1, Stohl 1998 (cited in the paper) provided an

assessment of the possible error deriving from a trajectory analysis based on the ECMWF meteorology output. Furthermore the use of clusters of trajectories and the large time interval (3 months) on which the statistics are computed help to reduce the uncertainty on the analysis. This was discussed also in the text.

9) P. 15277 line 19/20: Is it possible to quantify the effect of the clouds?

Generally the number of "no-clouds" profiles after the removal of the cloudy ones are still enough to have a representative statistical ensamble. The effect of deep convective cloud is however visible as a depletion in the time-series during the rainy season (see figure 5), in particular in the month of July, when there is a relative minimum in the frequency of detection of aerosol in the lower layers.

10) P. 15279 line 5-10: The different areas should be given in a map.

The regions were added in Fig. 1

11) P. 15280 line 5: From my point of view, the maximum of the polluted dust is more between Oct-Feb rather than between Sep-Dec as stated by the authors.

The reviewer is right, the mistake is now fixed.

12) P. 15281 line 8-12: The two mentioned corridors should be also marked in a map.

We modify the Fig. 6. to make it clearer and better identify the corridors. The text has been changed accordingly.

13) In Figure 5 the description of the plots should be according to the ones in the text.

The same description was added in the figure caption.

14) Figure 6 is very confusing. The huge amount of black dots does not transport the information in a good way.

Figure 6 was modified to transmit the information in a clearer way, see above (point 12)

Reference*:*

Stohl 1998, Computation, accuracy and applications of trajectories – a review and bibliography. Atmospheric Environment Vol. 32, No. 6, pp. 947-966, 1998

Schuster, G.L.,Vaughan M., MacDonnell D., Su W., Winker D., Dubovik O., Lapyonok T. and Trepte C., Comparison of CALIPSO aerosol optical depth retrievals to AERONET measurements, and a climatology for the lidar ratio of dust, Atmos. Chem. Phys., 12, 7431–7452, 2012