Dear editor, dear referees,

We would like to thank you for all your comments. This input has allowed us to refine the manuscript by adding more thorough detailed explanations, to correct some minor points and to improve in a large sense the manuscript. This response comprises three sections, first the answers to the main comments of both referees, then the answers to the specific comments of the first and second referees. The authors are conscious that the methodology and topic of this study are to some extent new concepts and that they consequently raise a number of comments. We hope however that in this document we have addressed the referees questions fully and clarified the aspects that needed further elucidation. The co-authors are unanimous that this manuscript presents a valuable methodology for interpreting atmospheric measurements at mountain sites across the globe. This manuscript presents a new technique and extensive data analysis applicable to many of your readers. Based on our extensive efforts in addressing each comment of the reviewers, we ask you to accept for publication the revised version of the manuscript in ACP.

First we want to mention that the values of the ABL-TopoIndex and of the correlation coefficients presented in figures 9 and 10 have changed from those presented in the first version of the manuscript. The differences in the ABL-TopoIndex values are due to the modification of the domain size to 500 km x 500 km. The correlation coefficients changes are due to the modification of the domain size, to the exclusion of SUM due to its outlier status similar to NCOS and to the inclusion of the middle altitude stations (HBP and MSY) in the correlation analysis. Further explanations are given in the following answers to the referee’s comments.

1. Answers to main comments of both referees

- **GIS and Topotoolbox**: TopoToolbox is a set of matlab functions that offers analytical GIS utilities in a non-GIS environment. In that sense it is possible to apply GIS-specific methods and to analyse aerosol parameters and cycles in the same environment as the topographic analysis. The TopoToolbox enables the analysis of relief and flow pathways in a DEM as well as the calculation of standard terrain attributes (slope, curvature, flow accumulation,…). The basic functionality of TopoToolbox was therefore used, but further programming was necessary in order to calculate all the necessary parameters constituting the ABL-TopoIndex. As suggested by the referees, the authors added some further clarifications to describe these parameters with sufficient details in the paper, so that the ABL-TopoIndex could be reproduced in any other programming language.

- **Domain size for the calculation of the ABL-TopoIndex:** The ABL influence at high altitude sites can be divided into a local phenomenon bringing polluted air masses from the adjacent valleys to the measuring station and a broader impact including the whole mountainous massif and a possible influence of nearby plateaus and plains. Poltera et al. (2017) clearly demonstrate that convection above the adjacent valleys rarely influences the high altitude sites but, when it is the case, this local convection does lift air masses with a certain aerosol load. The aerosol layer that comes from a much broader region has a lower aerosol concentration but influences the high altitude stations over a long period of time. An airborne Lidar measurement of the ABL top over the whole alpine massif (Nyeki et al., 2002) clearly stated that the convective boundary layer is formed over a large-scale and leads to an elevated and extended layer. They also quantified that this “large-scale” extends more than 200 km from the mountainous massif. The rectangular domain size of 750 km x750 km centered on each site corresponds to a distance of at least 375 km in each direction and was initially chosen to ensure the inclusion of the entire massif and a further portion of the adjacent plains. To address the concerns of the reviewer we have restricted the domain to 500 km per side, but think that a domain size smaller than that would no longer correspond to the reality of the aerosol layer formation.

The authors also agree with the second referee that CBL flow will not advect air masses from a distance as large as 375 km. Without precipitation, the residual layer or aerosol layer will however expand over several days. The distances of 375 km and 250 km are covered in 21 and 14 hours, respectively, at an average advection velocity of 5 m/s. The chosen domain size corresponds therefore largely to the development of the CBL and its merging into the residual layer.

- **Methodology and set of quantitative parameters:** The authors are aware that this study consists mostly of a new methodological approach with concepts probably unfamiliar to many atmospheric scientists. The goal was to try to statistically quantify the role of the topography in the ABL influence at high altitude sites. The authors intentionally did not include dynamical parameters such as wind fields that would have required the use of atmospheric models such as ECMWF. While the applied methodology was described with some detail in the original submission we have expanded, and to some extent reorganized the description based on the reviewers comments. First we define a number of topographic criteria that should determine the ABL influence at a high altitude site; second, quantitative parameters are found for each topographic criteria; finally, statistical methods that are valid for environmental studies are applied to the quantitative parameters. Tested qualitative parameters that were finally not selected are briefly described in the supplement to the manuscript. The reasons for not keeping these criteria to calculate the ABL-TopoIndex are now extensively described in the revised manuscript supplement, so that the reader can now better understand the final choice of parameters.
As already mentioned in the paper (section 4.3), the ABL-TopoIndex could probably be improved by adding some further parameters and its validity can also be assessed by other pollutants measurements at high altitude sites.

- **Weak correlations between topography and diurnal and seasonal cycles:** As mentioned by the first referee, the correlations between the topography parameters and the aerosol diurnal cycles are surprisingly weak. This is due to three main reasons: For most of the stations, there are a lot of days where the diurnal cycles are obviously visible. It is however quite difficult to extract the diurnal amplitude as a statistical value due to several factors including: non-regularity in diurnal cycle time of occurrence (e.g., due to different synoptic weather type, cloud presence, advections, long range transport); in the strength of the diurnal cycle (insolation amount, cloud presence); in the absolute level of aerosol present (e.g., due to presence of residual layer, superposition of long range transport); and to the superposition of both seasonal and diurnal cycles. The only possible methodology is to remove the first lag autocorrelation in the data, before extracting the diurnal cycle amplitude from the autocorrelation at 24h (see the supplement to the paper). The removing of the first-lag autocorrelation is a necessary step that introduces noise in the data. Additionally, as explained in the manuscript, only stations partly influenced by the ABL will show a clear diurnal cycle. Stations that remain the whole day in the FT should exhibit no diurnal cycle, whereas stations always in the ABL will have different diurnal cycles due to other periodicity in the sources and to the mixing conditions. As the location of the station with relationship to the ABL can change with season this further complicates the identification of diurnal cycles. Another factor is the presence of the residual layer during the night in summer, which drastically decreases the amplitude of the diurnal cycle. In terms of relating topography and seasonal cycles. Additionally an important thing to consider is that many of the datasets used here are shorter than 5-6 years leading to difficulties in the determination of the seasonal cycles. This is probably a primary cause for the lack of correlation between seasonal cycles and topography parameters. We have revised the manuscript to make this point more clearly as described in our response to referee#1 below.

2. Answers to referee #1 comments:

This paper presents five metrics that can help quantify the boundary layer impact at high altitude stations. The metrics are based on topographic data and provide information on topographic characteristics including steepness, height difference between station and adjacent valley, and size of the drainage basin. The metrics are calculated for large number of stations. The focus in this paper is on a subset of these stations where aerosol measurements are made.
Overall, I think that the paper is a decent contribution to the scientific literature. The novel part is the quantification of the topographic characteristics surrounding a high altitude stations. The contribution of certain topographic characteristic to trace gas measurements at these stations is often speculated and discussed and it is nice to see a paper where an attempt is made to quantify the characteristics. I am not sure though how useful the characterization of the topography as done in the current study will be for future studies/site planning.

I found it rather surprising that correlations between topography parameters and the diurnal cycle are weak.

Some further explanation are given in the answers to the main comments on page 2 of this document and the manuscript was revised in order to better explain the reasons for weak correlations. The manuscript was changed at § 3.5 : "*The ABL-TopoIndex is s.s. correlated with the diurnal cycle minimal and maximal strengths of the absorption coefficient. This correlation is once again principally due to the hypso% and G8, and to a lower extent, the LocSlope. The correlation with the diurnal cycle minimal amplitude occurs because the stations that remain in the FT during the whole day should not present any systematic diurnal cycles. The maximal amplitude of the diurnal cycles occurs when the site is in the FT during the night (without any influence of the RL) and influenced by the ABL during the day. The only s.s. correlation with station altitude is found for the scattering coefficient seasonal cycle. Similar to the correlation with the percentiles, there is a high anticorrelation between the particle number concentration diurnal cycles and G8 suggesting that the slope steepness in the vicinity of the stations inhibited both the transport of polluted air masses and NPF. Apart from a correlation at 90% confidence level between DBinv and the absorption coefficient, the lack of further s.s. correlations with the seasonal cycles can be attributed first to the relatively small time period (2-5 years) covered by most of the datasets leading to difficulties in the statistical determination of a yearly periodicity due to inter-annual variability, second to the low aerosol concentration at high altitude sites inducing measurements part of the time near the detection limits of the instruments (see for example the problem with the absorption coefficient at § 2.4) and third to the necessary whitening procedure (see supplement) increasing the dataset noise* . "
And also at § 4.1, the following sentence was added: "*The impact of the RL on the aerosol concentration is probably one of the most important reason to the low correlation between the topographical parameters and the aerosol cycles.*"

1) the choice of the five metrics appears somewhat subjective. At some point in the manuscript (section 4.3) it is stated that "Several other parameters such as the topographical wetness index, the catchment area, the accumulation, dispersion and transit percentages, the hypsometric index and the prominence were tested but were finally eliminated as being not relevant for various reasons." . It remains rather vague why these parameters were eliminated. It would be good if the authors could make a list (e.g, in a table) of all the relevant parameters that the "TopoToolBox" produces and then also clarify what exactly was done to come up with the final five parameters.

- As explained in the answers to the main comments (p. 1 of this document), the tested parameters are only partly provided by the TopoToolbox, some of them were developed or modified for this study. TopoToolbox provides a set of Matlab functions to analyze the relief and the flow pathways in the digital elevation model, some of them having absolutely no direct relation with the ABL-TopoIndex. In that sense, it is not possible to list all the TopoToobox functions. We have now included more discussion of the reasons to choose the 5 used parameters (§ 2.3) and we have also added Table S2 in the supplement to describe some other topography and hydrology parameters and the motivation of their rejection as relevant parameter to calculate the ABL-TopoIndex: not to use some parameters have to be explicit and the manuscript was accordingly changed:

Parameter	**Definition**	**Reason for rejection**
Upstream catchment area= flow accumulation	Upstream area contributing to the flow accumulation at the grid cell	1) It has no direct effect on the ABL influence since it lies at higher altitude than the station 2) It is a partial measurement of the area higher than the station elevation, but only on the mountain side where the station is situated
Topographical wetness index = compound topographic index	=ln(A/tan(B)), where A= upstream catchment area and B= slope gradient. It is a measure of the extent of flow accumulation at the given point; it increase as A increases and B decreases.	The wetness index is a ratio of two parameters. The slope gradient is already used (G8) in the ABL-TopoIndex and A was not considered as useful to describe the ABL influence (see previous point). The authors prefer single to combined parameters
Drainage basin = dispersive area	Downslope area potentially exposed by flows passing through the given point on the topographic surface	Air convection flow paths cannot be directly assimilated to water flow. The drainage basin in the inverse topography was consequently used as describing the size of the “reservoir” for air convection.
Efremov-Krcho landform classification scheme, Dispersion and transit percentages	It is a landform classification scheme (Florinsky, 2012) attributing a characteristic (dissipation, transit or accumulation) to each grid cell.	This classification scheme depends on the curvature of the terrain and, contrary to water flow, it has no relevance for air masses transport. It was however tested on some stations but failed to give a clear characteristic for the station region.
Hypsometric curve (HC), hypsometric integral (HI) and	The shape of the HC and HI values provide vital information about erosional stages of the relief and tectonic, climatic and lithological factors controlling landforms development. Convex-up curves are typical for youthful stage and concave-	Both HC and HI characterize the shape of the whole mountainous range and are therefore not defined for the station location. They cannot be used to characterize the station location.

	up curves of old stage. (Siddiqui and Soldati, 2014)	
hypsometric index (HI)	HI= (mean elevation-minimum elevation)/(maximum elevation-minimum elevation) allows different watersheds to be compared regardless on scale. It could reflect both tectonic activity and lithological control. (Siddiqui and Soldati, 2014)	HI also concerns a domain and not the station location. It cannot be used to characterize the station location.
Topographic prominence	It is the vertical distance between a summit and the lowest contour line encircling it but containing to higher summits within it. It is a measure of the independence of a summit.	It is not applicable to stations that are not situated at a summit. Moreover, since it restricts the area to a domain without higher summits, it corresponds to domains with very different sizes depending on the station.

2) How are the parameters produced by TopoToolBox similar to or different from the more widely used ArcGis software packages? many people who would like to apply the concept of a topographic index may be familiar with ArcGis software packages so a way to make the concept more widely used is to explain how these parameters could be calculated using ArcGis software.

- TopoToolbox just offers GIS utilities in a Matlab environment. The parameters used to calculate the ABL-TopoIndex are hopefully clearly enough described to allow any user to find or to write their equivalent in any GIS software packages, including ArcGis. For example, catchments or watersheds are probably calculated in a similar way. Since we have not used ArcGis, it is difficult to estimate exactly the potential of ArcGis compared to TopoToolbox.

3) page 3, line 30/31: Free convection cannot be driven by forced mechanical convection. This sentence is technically incorrect.

- The referee is correct that free convection cannot be due to any forced mechanism. The sentence was modified: "*In the case of cloudy or rainy conditions as well as in the case of advective weather situations, free convection is no longer driven primarily by solar heating, but by ground thermal inertia, cold air advection and/or cloud top radiative cooling*."

4) section 2.3, line 13: It should be explained here why a domain size of 750x750 km was chosen. The authors discuss somewhat later in the manuscript the sensitivity to domain size but a justification for the chosen domain size should be provided here. The domain size currently sounds rather arbitrary.

- The answer to this question is given in the answers to the main comments (p. 1 of this document). A large domain size has to be chosen in order to take into account the whole mountainous range and part of the adjacent plains/plateau contributing to the formation of the aerosol layer. In that sense a domain of 500x500 km^2 could also be justified and was consequently used in the revised version of the manuscript, leading to small variation of the ABL-TopoIndex for some stations. We have also clarified our reason for the size of domain in the revised manuscript at § 2.3: "*A quantitative estimation of these criteria depends clearly on the domain considered. The minimal size requirement for such a topographical analysis is that the domain should contain the whole mountainous massif. An airborne Lidar measurement of the ABL over the Alps (Nyeki et al., 2002) clearly stated that the convective boundary layer is formed over a large-scale and leads to an elevated and extended layer. It also quantifies this "large-scale" to extend more than 200 km from the mountainous massif. A* *rectangular domain size of 500 km x 500 km centered on each site was then chosen (see § 3.2 for a discussion of the effect of the domain size).*"

5) page 7, line 6: "with the size of the local scale depending on latitude". Please explain/expand.

- The gradient is applied between 2 grid cells and the length of the domain covered by 2 grid cells depends on latitude (see § 2.2) and correspond to 2-4 km. The manuscript was changed*:" This parameter takes into account the slopes towards lower and higher elevations over a local scale (2-4 km that is the distance covered by two grid cells, with the size of the grid depending on latitude)"*

6) page 8, line 18: use plural "autocorrelations". Also on next line, "auto-correlations" is hyphenated. Find out how it needs to be written and be consistent.

- OK, the text was changed and the hyphenation was removed. The supplement was also corrected.

7) page 17, line 6/7:"Usually the spring leads to higher concentration of ABL species than the autumn". Why?

- At most sites (and not only at high elevated sites) the CBL height is found to be higher in spring and summer than in autumn and winter. The correlation with the down welling solar radiation at the surface clearly explains the summer high ABL height and hence the summertime peaks. Some other authors found an anti-correlation with the surface pressure and the lower tropospheric stability and a correlation with the near surface wind speed and temperature. A cumulative effect of all these parameters leads to a usually higher CBL height in spring (Guo et al., 2016, Pal and Haeffelin, 2015). However, since I do not find a clear referenced explanation for the often observed difference in ABL height between spring and autumn, I prefer not to insert any further explanation in the manuscript. The sentence was however changed to: "*Usually the spring leads to higher aerosol species than the autumn probably bounded to higher ABL height."*

8) page 18, line 14/15: Please explain why absorption coefficient is "the best tracer for anthropogenic pollution and biomass burning and consequently of ABL influence.". Unclear to me.

- The GAW-recommended basic aerosol measurement program consists of the particle number concentration, the scattering and absorption coefficients. All three of these parameters are higher in ABL than in FT. As stipulated on page 18 (in originally submitted manuscript), the aerosol absorption coefficient (or black carbon (BC) concentration) is the best tracer for ABL influence among the three aerosol parameters discussed. This is because the main sources of BC (anthropogenic pollution due to combustion processes and biomass burning) are in the ABL but are scarce near the high altitude sites. Additionally, BC aerosol is not produced by any secondary processes. In contrast, the particle number concentration and, to a lesser extent, the scattering coefficient are also influenced by gas-to-particle conversion mechanisms such new particle formation and condensational growth, which are secondary processes depending on the ABL influence in a more complex way and also on other parameters such as the solar insolation, the temperature and other thermodynamic processes. In that sense and among the basic aerosol parameters measured at most stations, the absorption coefficient is the best tracer for ABL influence. § 4.2 was changed accordingly: "*The absorption coefficient is primarily due to the presence of black carbon emitted from combustion processes occurring mostly in the ABL and rarely near the high altitude stations; additionally, BC aerosol is not produced by any secondary processes. Among the aerosol parameters studied here, the absorption coefficient is consequently the best tracer for anthropogenic pollution and biomass burning and consequently of ABL influence."*

9) page 19, line 8: "by the smoother pressure decrease". I don’t understand that explanation. Please clarify.

- An anti-correlation between the slopes around the station and the number concentration can be explained by new particle formation that is enhanced if the pressure difference experienced during the upslope transport is not too large. The sentence was clarified: “*The greater correlation of slope with the number concentration rather than with the absorption coefficient can be explained both by the very scarce sources of black carbon in the near vicinity of most of the high altitude stations and by the smoother pressure decrease experienced by the precursors during their upslope transport along gentle slopes leading to more condensation processes and nucleation*.”

10) page 20, line 1: "and at all altitudes" awkward phrase. Rephrase sentence.

- *This was just a mistake and was removed.*

11) page 19, line 11: "There are consequently few correlations between topography parameters and the diurnal cycles". This is an important finding that should be explained better in this section. Does this imply that investigators trying to discuss diurnal cycles at high altitude locations waste their time by trying to find any correlation with topography?
Please discuss this better.

See also the answers to the main comment on page 2 and 3 of this document. The study of the diurnal cycles at high altitude sites can really bring important results if specific cases are analyzed and compared. In this study, a statistical approach has to be used to obtain a reliable estimate of the diurnal cycle amplitude and leads consequently to weaker correlations. This paragraph was changed: “*The aerosol diurnal cycles are influenced by numerous phenomena (see Sect. 4.1) leading to a non-trivial relationship with the ABL influence. If the study of the diurnal cycles can bring valuable results if specific cases are analyzed and compared, the statistical approach is less obvious due to the noise in the data (low aerosol concentration and whitening process), to the inter-annual variability of the meteorological processes and to cloud, precipitation and long-range advection involving a large day to day variability. There are consequently few statistical correlations between topography parameters and the diurnal cycles. The clearest correlation is the influence of the insolation on the aerosol diurnal cycles amplitudes. This dependence between the latitude and the aerosol concentration was already mentioned by Kleissl et al. (Kleissl et al., 2007) and is easily understandable, the convection and the new particle formation being directly dependent on the solar radiation intensity. The other correlations are found between some topography parameters (ABL-TopoIndex, hypso%, G8 and LocSlope) and the absorption coefficient, which is the best tracer for ABL influence among the aerosol parameters.*”

12) Figure 3 caption, line 5: "horizontal" should be "vertical" here, I think.

- Yes, it was changed

13) Figure 11, caption: "Calculations corresponding to the various domain sizes can be identified by the various flow paths lengths.". I don’t understand how the calculations can be identified. Please clarify.

- The plotted colored lines have various lengths that are for example visible around SBO station. This section was deleted following suggestions by the second referee.

14) Figure 12 caption. "similar to Fig. 8". I don’t see how this is similar to Fig. 8.

- You’re right, it should be changed to Fig. 11. This section was deleted following suggestions by the second referee.

3. Answers to referee #2 comments:

The manuscript "The topography contribution to the influence of the atmospheric boundary layer at high altitude stations" by Collaud Coen and co-authors investigates the role of the local to regional topography on aerosol observations made at high altitude sites. They derive parameters that are supposed to reflect the average influence of the atmospheric boundary layer on each site and rank the sites by these parameters. A comparison with different observed aerosol parameters is presented and supposed to show the validity and usefulness of the approach. However, I see several major problems with the suggested approach comprising all aspects of the presented work: the methods used to derive topographic parameters, their selection for a final index, and the choice of aerosol parameters that should reflect ABL influence. Although the manuscript touches on an important question of atmospheric monitoring and could be valuable for future network planning, it cannot be published in the current form and has to undergo major revisions.

Specific concerns

1) The analysis is only focusing on the influence of thermally induced wind systems on the aerosol observations at high altitude stations. Other vertical lifting mechanisms like foehn, deep convection, and frontal passages are completely neglected, although they can be as important depending on location of the site and the season (e.g. tropical vs. high latitude stations, summer vs. winter). The relative contribution by other lifting mechanisms to local "ABL" events will vary strongly between sites (e.g. volcanic island in the subtropics (rare) vs. coastal range mountain in mid-latitude west wind drift (frequent)). The methods presented here need to consider these differences, for example by limiting the observed aerosol observations to cases where vertical lifting mechanisms other than thermally induced flow can be ruled out.

- First, the authors agree with the referee that convection and thermally induced wind systems are not the only mechanisms that bring polluted air masses to high altitudes. The other vertical lifting mechanisms described by the referee contribute to indeed enhance the pollutant concentrations at high altitudes up to the free troposphere and it is also correct that these effects will vary depending on site, season, latitude, etc. However, as we explained in the introduction of the manuscript, we restricted this study solely to the influence of the topography on the thermally induced wind systems and the CBL growth. This study considers neither the dynamics of the atmosphere nor the soil properties. Such detailed and specific analysis is best left to the scientists responsible for the individual stations but is too complex when evaluating multiple sites with disparate data sets. To take into account the atmosphere dynamics, 3D models (and not only a 2D model of the earth surface) are necessary, which is clearly not the goal of this study and definitely outside its scope. Due to computational constraints, most current global models doesn’t do a good job of representing the actual topography, the model grid spacing tends to be too large (on order of 1-2 degrees of latitude and longitude) most global models provide low frequency output – typically monthly (although sometime daily). This means that targeted regional models would need to be used to describe each of the 46 sites here, again – this is a topic best left to the local experts responsible for each observatory.
- Second, our approach is to do a global and statistical analysis to understand the role of the topography in the ABL influence across an array of 46 mountain sites. Our hope was to begin to develop common rules that can be applied to all stations. It was never meant to analyze specific cases for clear thermally driven transport at individual stations. Doing so would also greatly reduce the usable time series and result in statistically small data sets. As stated in the

introduction, there is presently no single method to screen ABL-influenced from FT air masses at high altitude sites. It is therefore quite difficult to sort the cases where the ABL influence is only due to thermally driven transport. Even if possible for all types of environment, the limitation of the aerosol dataset to cases where vertical lifting mechanisms other than thermally induced flow can be ruled out would need further complex data sets for each station (for example: pressure, humidity, wind measurements at each side of the stations, 3D back-trajectories, synoptic classification scheme and probably some gaseous species concentrations).

- Finally, Zellweger et al. (2003) concluded that, in contrast to the NOy mixing ratio, the major process for upward transport of aerosol is the thermally induced vertical transport. The choice of aerosol parameters to validate the ABL-TopoIndex can therefore be considered as the best one to study the thermally driven air mass transport.
- For all these reasons, the authors consider that the inclusion of the atmosphere dynamics and of the wind systems is beyond the scope of this study.

2) Furthermore, the method completely neglects the role of local to regional emissions. Emissions within the region of interest will be very different for the various sites and they will largely determine the amplitude of "ABL" events observed at the sites and also influence the larger scale tropospheric background. At least qualitatively emissions need to be considered and there is no lack of fairly high resolved, global emission inventories (e.g. for BC).

- The authors agree that the regional emission sources have an influence on the pollutant concentration measured at high altitude sites. However, the timing and relative magnitude of temporal cycles (as determined by auto-correlation) with ABL influence does not depend on the pollutant concentration in the ABL. To use the emission inventories, the atmosphere dynamic and particularly the wind components should also be taken into account in order to assess which sources on the 500 km x 500 km influence the high altitude sites (see answer to previous referee comment). Moreover, while the absorption coefficient could perhaps be "normalized" by the BC emission inventories, the scattering coefficient and the number concentration depend also on gas to particle conversion (e.g., new particle formation and condensation). The modeling of the gas-to-particle conversion from the emissions inventories and meteorological data is however rather complex. Moreover, the highest aerosol concentrations at the high altitude sites often depend much more on long range transport of mineral dust or biomass burning than on the regional sources. The authors are therefore of the opinion that, first, these large uncertainties would annihilate the potential benefits of the inclusion of the emission inventories and, second, that the inclusion of the atmosphere dynamics is beyond the scope of the paper. Additionally the amplitude of the diurnal cycle which is discussed in section 3.5 should be independent of the regional sources; this is therefore another way to "normalize" the aerosol concentration without reference to emissions information.

3) A similar problem is the selection of the observed aerosol parameters. Absolute aerosol parameters will depend on more factors than just the local to regional ABL input and are therefore not useful to access the question of FT vs. ABL influenced air mass. It would be more promising to identify pollution or "ABL" events in each data series and correlate the frequency of these with any set of topographic parameters. Why would the 5th percentile of the absorption coefficient be a good indicator of ABL influence? The 5th percentile only reflects the lowest concentrations and not the frequency of pollution. Looking at the skewness of the distribution could be another indicator. Larger skewness would also indicate more frequent pollution events.

- To our knowledge the only method to detect local CBL development as well as the top of the aerosol layer is to use a ceilometer (or a lidar). There are however very few high altitude station around the world with a ceilometer time series from a lower altitude adjacent station thus limiting any statistical analysis.

- The 5th percentile clearly reflects the lowest concentrations and therefore the ability to sample clean FT air masses at the high altitude stations. The lower the ABL influence is (through the CBL and the aerosol layer heights), the lower the 5th percentile will be. The authors do agree that the median of the aerosol parameters is much more dependent on regional and local sources, whereas the 95% depends on rare high aerosol concentrations probably due to long-range transport of mineral dust or biomass burning. A normalization of the aerosol parameter with the 95% has consequently also not much sense.
- The aerosol parameters discussed in the manuscript (number concentration, absorption and scattering coefficient) are approximately lognormally distributed variables. The skewness toward the lower values is therefore not defined. The skewness toward the higher values reflects the occurrences of very high aerosol concentration that generally relates to long range transport of mineral dust and biomass burning. The skewness is consequently not the right parameter to detect ABL-influence.
- Apart from the 5th percentile, the best parameters are clearly the diurnal and seasonal cycles. These are however much more difficult to statistically extract from the time series (see the answers to the main comments on p. 1 in this document) and exhibit few correlations with the topography parameters.

4) The selection and methods to derive the topographic parameters seem to be very arbitrary and no methodological way was followed to present a set of parameters that explains the observed inter-site variability. The final results seem to suggest that mainly one of the parameters is able to predict this variability (hypso%) showing even higher correlation coefficients than the final combined topographic parameter. It also remains unclear why a region as large as 750 km times 750 km was chosen for the analysis. Clearly the flow during one diurnal cycle (and that's what a thermally induced flow system spans) cannot advect air masses from a location as distant as 325 km. Assume an average advection velocity of 5 m/s, which is already a fair value for the kind of fair-weather, low pressure gradient situation required for thermally induced flow, then it would take 18 hours to cover the 325 km. Also plain to mountain winds are known not to extend from the mountains by more than around 100 km. Hence, the use of a smaller region or the use of several sets of parameters for smaller regions should have been considered. These larger sets of topographic parameters and/or any combination of them could than have been fed into a statistical model of the observed aerosol parameters using parameter selection techniques to derive the most important topographic parameters.

- The methodology applied in this study consists first in identifying topographical criteria that would tend to increase the ABL influence and then finding parameters that can be quantitatively estimated and related to the topographic criteria. The authors do agree that some choices were not sufficiently motivated in the first version of the manuscript so that now both the used parameters (section 2.3: ABL-TopoIndex) and the rejected parameters (Table S2) are now better described. (see also the answer to the specific comment "p.6 L12" in this document)
- The reasons to choose a large size of the domain are given in p. 1 of this document (answers to the main comments) and also now discussed in the revised paper. The authors also have now restricted the size of the domain to 500km x 500km. This restriction has a very low impact on the results. : "*A quantitative estimation of these criteria depends clearly on the domain considered. The minimal size requirement for such a topographical analysis is that the domain should contain the whole mountainous massif. An airborne Lidar measurement of the ABL over the Alps (Nyeki et al., 2002) clearly stated that the convective boundary layer is formed over a large-scale and leads to an elevated and extended layer. It also quantifies this "large-scale" to extend more than 200 km from the mountainous massif. A rectangular domain size of 500 km x 500 km centered on each site was then chosen (see § 3.2 for a discussion of the effect of the domain size).*"
- The third specific concern of the referee clearly supports our contention that there are no parameters that can act as an indubitable sign of ABL influence. The best statistical parameter would be the annual cycle of the diurnal cycle amplitude which should be the greatest for

stations sampling the FT part of the year. It was however not statistically possible to extract this parameter from the available time series for the following two reasons: 1) a lot of the time series were too short (< 2-5 years), as explained in the answers to the main comments (p. 2 of this document), and 2) the low aerosol concentration measured at high altitude combined with the pre-whitening process lead to a large uncertainty in the statistical determination of the cycle amplitude. In that sense, there is, to our knowledge, no reference measurement that would definitively identify the ABL influence and allow selection through a statistical model the most important topographic parameters.

- Apart from the used and the rejected parameters (now more clearly described in the revised manuscript), the authors do not see any other "direct" parameters that can be possibly used. There are other more sophisticated parameters that are linked to the valley's topography that could be added to the ABL-TopoIndex in a further study (see § 4.3 describing possible future work), but the authors found it necessary to validate the present study by a publication before investing further time in exploring more complicated parameters.

5) This continues from 4 but deserves its own point. The analogy between water flowing down a mountain and thermally induced flows rising up a mountain, which is used to derive the parameter DBinv and is used in the discussion of section 3.6, is not valid. It is simply not correct to assume that a large air catchment will result in large upward flow at the highest point of a mountain massif. Air does not flow up to the highest point as water flows down to the lowest point. The upward flow on a fairweather day with small pressure gradients happens along individual slopes all along individual valleys and results in many convergence lines but not a single convergence point as suggested here. The presented parameter probably has some value on the very local scale but may just be very similar to hypso% in the end. This parameter and its justification as well as the whole discussion of flow paths will need to be removed from the manuscript. It simply does not reflect the ongoing physics of thermally induced flow systems correctly.

- The authors do agree with the referee that the analogy between water flowing down and thermally driven air flow has very well defined limitations. In that sense we have removed the whole discussion about flow paths (§ 3.6 and figures 11 and 12) that involve a direct analogy between the water and the air mass flow paths.
- The DBinv used in the ABL-TopoIndex has a completely different motivation and impact. DBinv is a quantitative parameter for the size of the reservoir for air convection (criterion number 4). The authors do agree that upward flows do not result in a single convergence point at the station. However DBinv is a measure of the territory that can directly influence the station air masses by upslope winds. It is true that the considered domain represented by DBinv is too large to represent the direct influence of the CBL at the station, but it is of reasonable size to describe the influence of the aerosol layer (AL) (or residual layer (RL) during the night). It was clearly shown that the AL (or RL) have a clear impact on the aerosol concentration at high altitude stations (Collaud Coen et al., 2011, Poltera et al., 2017, Andrews et al., 2011 and references therein). Due to these reasons and to the influence of DBinv on the correlation of the ABL-TopoIndex with the aerosol parameters, the authors have chosen to keep DBinv in the ABL-TopoIndex definition.

Specific comments

Abstract: Clarify what is the scientific question at hand and what is your contribution to this problem. For example starting from line 21, start the sentence with something like "Here we ..."

- The abstract was modified and the following sentence was added at line 21: "*In this study, a topography analysis is performed allowing calculation of a newly defined index called ABL-TopoIndex. The ABL-TopoIndex is constructed in order to correlate with the ABL influence at the high altitude stations and long-term aerosol time series are used to assess its validity.*"

Page 8: How comparable are the aerosol parameters between sites? Besides the detection limit adjustment what kind of common quality assurance, quality control was applied to assure that these parameters can really be used for a ranking between sites.

- 23 of the 28 aerosol datasets are provided by GAW stations and the data were obtained from the EBAS data center. GAW stations have to follow the measuring rules and quality assessment edited by the WMO/GAW aerosol advisory board. These measurement principles are extensively described in GAW report Nr 200 (WMO/GAW standard operating procedures for in-situ measurements of aerosol number concentration, light scattering and light absorption). As required by the GAW aerosol advisory board, all measurements were performed at low humidity (RH<40%). Moreover the data owners also follow the quality control procedures of the EBAS data center. Four of the datasets (MUK, NWR, PEV and OMP) are not GAW stations but the measurements were performed by research groups operating at other GAW stations. Individual exchanges with the data providers from those four sites indicated that they collected those datasets using methods similar to their operations at GAW stations so that the quality and traceability are assured. The GAW stations are now given in bold in Table S3. The umbrella provided by the WMO/GAW program is, to our point of view, sufficient so that a further description of the quality assurance of the aerosol measurements is not needed in this paper.
- Other procedures such as the STP correction, the truncation correction of Nephelometer data, the negative data of the absorption coefficient were controlled and handled similarly for all datasets. Small time series breakpoints are not important since no trends were calculated.
- All the time series were visually inspected and any doubtful data were removed after discussions with data providers.
- All the times series but 2 were done on TSP or PM10 inlets, so that similar aerosol size distributions were measured.

P2,L34: The whole terminology is confusing "flow paths for air convection". Convection does not happen along flow paths. Convection is a vertical transport and mixing mechanism at small scales and as such defined as mostly unorganised. See: http://glossary.ametsoc.org/wiki/Convection. Why not talk about "thermally induced flow paths" instead.

- The authors agree that “thermally induced flow paths” is a much better terminology. Since § 3.6 on “Flow paths as a function of ABL heights” was removed, the expression “flow paths for air convection” no longer appears in the manuscript.

P3,L20: Commercial airline programs such as IAGOS CARIBIC (http://www.caribicatmospheric.com/) would be worth mentioning in this context as well.

- The following text was added to the manuscript: “*Instrumented airplanes can make detailed measurements of the vertical and spatial distribution of atmospheric constituents and are used either during limited measurement campaigns or on regular civil aircraft (see for example the IAGOS CARIBIC project), but, because of the limited temporal scope of most measurement campaigns, cannot provide long-term, continuous context for the measurements*.”

P4,L26: Mention that this is the picture for a continental ABL not for a marine ABL.

- Ok , done (@P3 L26): “. *In the case of fair-weather days, the continental ABL has a well-defined structure and diurnal cycle leading to the development of a Convective Boundary Layer (CBL), also called a mixing or mixed layer, during the day and a Stable Boundary Layer (SBL) which is capped by a Residual Layer (RL) during the night (Stull, 1988).”*

P4,L29: This is not necessarily correct. In regions with emissions the nighttime accumulation of the emitted species in the shallow SBL usually leads to nighttime concentration maximum of these species.

- As noted by the referee, it is completely correct that the emitted species accumulate during nighttime in the SBL, leading in some cases to high concentrations. This was now specified in the text at P3 L30.
  "During daytime, the aerosol concentration is maximum in the CBL and remains high in the RL. During nighttime, the surface-emitted species accumulate in the SBL."

P4,L17: The authors should mention other vertical lifting processes. Generally frontal lifting (synoptic systems), deep convection and, in mountainous terrain, foehn. The importance of these processes was nicely illustrated by Zellweger et al. (2003).

- The text was changed to *"Finally, ABL air masses can also be dynamically lifted by frontal systems, deep convections or foehn as well as be advected from mesoscale or wider regions and influence high altitude measurements by all these atmospheric processes."*

p4,L25: Zellweger et al. (2002) not in list of references. Probably meant Zellweger et al. 2003, but that does not include a discussion on CO2. Please correct.

- This was indeed incorrect and was changed: *"Many methods have been used to separate FT from ABL influenced measurements, including those based on time of day and time of year approach (Baltensperger et al., 1997; Gallagher et al., 2011), wind sectors (Bodhaine et al., 1980), the vertical component of the wind (García et al., 2014), wind variability (Rose et al., 2016), NOx/NOy, NOy/CO ratios or radon concentrations (Griffiths et al., 2014; Herrmann et al., 2015a, 2015b; Zellweger et al., 2003) and water vapor concentrations (Ambrose et al., 2011; Obrist et al., 2008), although none of these methods leads to an absolute screening procedure to ensure the measurement of pure FT atmosphere."*

p4,L34 to p4,L2: Here it is stated that there are other important influence factors other than thermally induced flow. But it is not explained why one should be able to neglect them. See major remark 1.

- Please see our response to main comments p.1 of this document. Further, to our knowledge, most of the meteorological models are not able to solve all the dynamic processes in complex topography. This study therefore concentrates on one question and tries to identify some relations between the topography and the thermally induced ABL influence. This restricted, but nevertheless ambitious objective (as well as the factors that are not taken into account), are clearly specified in the manuscript.

p5,L3: The term topographic index or topographic wetness index is already defined in hydrology (the authors used it as well). Therefore, the choice of this name for the parameter introduced here might be confusing, especially since some hydrological methods are applied to derive part of this parameter. Maybe just use ABL-index instead.

- P6L3: The referee is, of course, right in saying that the terminology of "topography" and "index" are already widely used in several scientific domains. The use of only "ABL-Index" however seems too vague to the authors, since it does not specify that only the effects of the topography are taken into account. For example, an ABL-Index could represent any number of ways of assessing ABL influence. The authors chose therefore a name (ABL-TopoIndex) where the three main underlying concepts explored in the manuscript are cited. The word "topography" was also abbreviated in order to minimize possible confusion with existing hydrological terms. Moreover the manuscript was carefully checked so that the word "TopoIndex" was never used alone. The authors prefer to keep it as written.

p5,L7: Unclear what is mend here by lakes. Again a wrong picture is drawn that suggests that there is a certain amount of air that can be transported by thermally induced flow systems. Lakes or cold air pools are more a phenomenon of the nighttime SBL but not an established concept for daytime flow.

- The authors do agree that, even if used in quotation marks, the word "lakes" is misleading. It is now replaced by the expression "*air mass reservoirs*" in the revised version.

p5,L19: Why was the relatively coarse dataset GTopo30 used? There are global DEMs with higher resolution. 1 km seems a bit coarse for the kind of sites in extremely steep terrain targeted in this study. Some of the local topography will be missed. In this context it would be interesting to see how the height of GTopo30 at the station locations actually compares to the real altitudes. I would encourage the authors to have a look at a higher resolution DEM like https://asterweb.jpl.nasa.gov/gdem.asp for any further analysis.

- The authors thank the reviewer for giving the suggestions of another high resolution DEM that they will use in case of further studies. A higher resolution model will clearly be of interest. It has however to be noted that the ABL influence is not really a very local phenomena so that the mean over 9 grid cells was used to obtain the ABL-TopoIndex. As expected the GTopo30 altitude at the station grid cell differs by more than 20% for 3 of 28 stations used for the correlation analysis, the GTopo30 altitude being always lower than the station altitude. These differences do not correspond to the real altitude difference between the real and the GTopo30 mean altitude over each grid cell. Corresponding to the methodology applied to the ABL-TopoIndex, the correlations were also done with the mean altitude of the 9 grid cells. It has however to be noted that the use of the station altitude or of the 9 grid cells mean altitude does not change the correlation results. The GTopo30 manual gives a minimal vertical accuracy of 250 m at 90% confidence level and a RMSE of 152 m (the Peru map, which has a lower accuracy (see GTOPO30 manual), is not used). The altitude of the grid cell containing the station as well as the mean altitude of the 9 grid cells used to calculate the ABL-TopoIndex are now given for all stations in Table S1 with the following comments: "*The real altitude of the station, the mean altitude of the grid cell containing the station and the mean altitude of the grid cell containing the station and of its 8 adjacent grid cells are given in Table S1, the last 2 altitudes are calculated from the DEM after its projection in UTM coordinates. Since the stations are usually at high altitude, the altitude of the DEM grid cell is usually lower than the station altitude. The mean and median of the differences between the station altitude and the one of the grid cell are 190 m (8.6%) and 140 m (5.8%), whereas the mean and median of the differences between the station altitude and the one of the 9 grid cells are 270 m (11.7%) and 220 m (10.3%), respectively. The maximal altitude differences is found for SZZ (1153 m) that corresponds to 3% of the station altitude. Due to its peculiar situation (see paper), NCOS altitude is 1110 m lower that its DEM grid cell altitude (2.8%) and this can perhaps explain NCOS outlier status. ZEP is only 306 higher than its grid cell altitude, but this corresponds to 65% of its altitude and also explain its very high ABL-TopoIndex and its outlier status. It has however to be noted that The GTopo30 manual gives a minimal vertical accuracy of 250 m at 90% confidence level and a RMSE of 152 m (the Peru map being anyhow not used).*"

p6,L11: Very questionable that these parameters are quantitative

- The parameters described under 2.3 are quantitative parameters that can be calculated for each point of the earth using a DEM. In that sense we think that the adjective "quantitative" is not misleading.

p6,L12 cont: Lots of arbitrary choices here. 750 km domain, median altitude vs. station altitude (could be any percentile; lower percentile would avoid negative values), slope between 1 and 10 km, 2-4 km mean gradients ... As mentioned above sets of parameters for different distances, etc. should have been derived and a statistical model with parameter selection been applied. It would also be nice to see all values for the calculated parameters as part of table 1.

- We agree that all the choices should be explained in this section and rather than later on in the paper:
  1) Concerning the size of the domain, please see the answers to the main comments on p.1 of this document. § 2.3 was also modified: "*A quantitative estimation of these criteria depends*

clearly on the domain considered. The minimal size requirement for such a topographical analysis is that the domain should contain the whole mountainous massif. An airborne Lidar measurement of the ABL over the Alps (Nyeki et al., 2002) clearly stated that the convective boundary layer is formed over a large-scale and leads to an elevated and extended layer. It also quantifies this "large-scale" to extend more than 200 km from the mountainous massif. A domain size of 500 km x 500 km centered on each site was then chosen (see § 3.2 for a discussion of the effect of the domain size).",

2) for the hypsD50, the referee is correct that any percentiles could be chosen. The median was taken first because it is a common averaging tool and second because presumably it would be lower than the location of each "high altitude station". The authors tried to summarize this more clearly in the manuscript by adding the following sentence: "*The median of the hypsometric curve was chosen first because a station claiming to be a high altitude site should typically be at higher altitude than half of its geographical environment."* Moreover, the station with hypsD50 can be found in Table S1.

3) LocSlope is defined on a radius of 10 km since the minimal distance between the station and the nearest plateau is usually equal to or larger than 10 km. This is now stated in the manuscript: "*The distance of 10 km to calculate the LocSlope was then chosen as representative of the maximal distance to the next adjacent plateau for almost all stations."*

4) the G8 is always calculated from one grid cell to the next, so that the distance of 2-4 km is given by GTopo30 and varies with latitude.

Moreover all values for the calculated parameters are now in Table S4

P7,L9: Confusing wording and concept. Drainage is a nighttime process, convection a daytime process???

- Yes, drainage winds are a nighttime process, but the manuscript discusses a "drainage basin". Drainage basin is a hydrologic term without time connotation and can be used for daytime processes. As defined by the dictionary, "a drainage basin is the area drained by a river and all its tributaries". It is also called catchment area, drainage area, watershed or river basin.

p7,L25f: It is true that the geometric mean will change in the same way for any percentage change in any of its parameters. However, it does not normalise the variability in the parameters in the desired way. If parameter a has a 10 times larger relative variability than parameter b, the variability of the geometric mean will be dominated by a. If this is an issue in the current case could be easily tested by the authors by analysing the relationship of the original parameters and the derived geometric mean. Better than the geometric mean would be the use of parameters that were normalized for example by their variance.

- The referee is correct that the geometric mean reports similarly any percentage change in any included parameters whatever the absolute value of the parameter is. This is the reason to apply the geometric mean for environmental indices that are built with very different parameters. The use of other types of averaging with any kind of normalization does not allow us to obtain this necessary (for this analysis) mathematical property. A normalization with either the maximum or with the variance will change the value of the ABL-TopoIndex but not the ranking of the stations. Moreover the authors checked that none of the included parameters dominates the results. To further develop this critical technical point, the manuscript was changed: "*Further, a given percentage change in any of the parameters will yield an identical change in the calculated geometric mean value. In that sense the variability of each parameter is also normalized, leading to similar modifications of the ABL-TopoIndex for similar parameter's variations.*"

p8,L17ff: It should be mentioned again when presenting the results that the seasonal and diurnal cycle that is looked at is actually the auto-correlation function. As such the amplitudes of the cycles is already normalised, which helps for the inter-comparability between sites.

- Yes, it is a good idea to highlight this fact in the results section. The following sentence was therefore added to § 3.5: *"Both the diurnal and the seasonal cycles were calculated as the strength of the autocorrelation function (see § 2.4 and supplement) so that the underlying parameters are de facto normalized and that the cycles between the stations can be directly compared."*

p9,L15f: These changes are rather large. Especially considering that the ranking between sites changes with domain size. It should be possible to solve the transformation problem in such a way that G8 and LocSlope are really constant with domain size. Why would the domain size change the local transformation/interpolation anyway? This needs to be redone.

- The authors looked again at the problem of non-constant values of LocSlope and G8 for various domain sizes. Both these values are constant in the traditional latitude longitude coordinates. The UTM projection leads to minor changes in the LocSlope and G8 that can be explained by two reasons: 1) if the analyzed domain extends beyond 2 UTM zones, map distortion problems occurs. This is, for example, the case for BEO plotted in cyan on Fig. 6 and having large G8 modification as a function of the domain size. 2) the interpolations needed to do the UTM projection can also lead to variation and G8 is very sensitive to these variations. The UTM projection is however necessary to ensure a similar handling of stations at very different latitudes.

Section 4: The name of the section is misleading. The section does not present a ranking of the sites by TopoIndex but more a discussion along their geographic location.

- 3.4: The title was changed to "*Relation between the ABL-TopoIndex and the station location*"

p12,L5: The more correct name would be "Rocky Mountains".

- "Rockies" was changed "*Rocky Mountains*".

p12,L15f: Why was MWO not discussed in this context as well?

- It is right that MWO is the North America station with the lowest ABL-TopoIndex and needs some comments. The following text is now added: "*Mount Washington Observatory is located in the Presidential Range of the White Mountains. It is the highest peak in the Northeastern United States and the most prominent mountain east of the Mississipppi River. MWO is consequently the North American station with the lowest ABL-TopoIndex due to very low hypso% and relatively high G8 and low DBinv."*

p13,L13f: Looks like the authors themselves are surprised that there is any relationship between their TopoIndex and the chosen aerosol parameters ...

- The authors just wanted to state that their hypothesis was verified. If wrong criteria or parameters (see § 2.3) had been chosen, the correlation with aerosol parameters would have shown it. The word "happily" is however inappropriate in a scientific context and is (sadly) removed in the revised version.

p13,L26f: But hypso% is an even better predictor than TopoIndex. I guess that means that all other parameters only partly destroy this relationship but do not add any useful information. Especially the suspicious parameter based on water flow analogy, DBinv, seems to show very bad predictive skills (worse than altitude alone in some cases).

- As explained at the beginning of this document, the various modifications required by the referee's (smaller domain size, inclusion of the middle atltitude stations) as well as the removing of SUM time series from the correlation analysis lead to a somewhat different values of the Spearman rank correlation coefficients, even if the statistical significances remain similar for most of the case. In case of the correlation with the absorption coefficient, the importance of hypso% with regard to the other parameters constituting the ABL-TopoIndex decreases. LocSlope and G8 are now equally important parameters, whereas hypsoD50 has usually a lower statistical significance. We also checked that the statistically significance of the correlation between the ABL-TopoIndex and the aerosol cycles is clearly decreased if DBinv is removed from the ABL-TopoIndex definition. This is effectively the case, even if DBinv has globally bad predictive skills. Sections 3.5 and 4.2 were consequently modified.

p14,L18: Wasn't the point in Bianchi et al that the ABL influence is not a direct one, like you focus on here, but an indirect one of ABL air picked up a few days before arriving at the measurement site and therefore not being lifted by thermally induced flow but by convection or frontal systems.

- Thank you for this comment. It is correct that the greater ABL influence due to longer daytimes and stronger insolation does not relate to Bianchi et al., 2016. At this point, the authors just wanted to mention that stronger insolation usually also promotes NPF formation. The manuscript was modified consequently: "*The high correlation between the maximal diurnal cycle and the number concentration can also be explained by the promotion of NPF by the stronger insolation at low latitude.*"

p14,L30: Isn't the failure of the ABL-TopoIndex to identify these lower altitude sites a clear indication that the suggested method does not work at all? Otherwise these clear cases of larger ABL influence should be detected and the correlation should actually improve.

- De facto, the concept of the ABL-TopoIndex is really developed for high altitude stations with complex topography and cannot be applied to low altitude sites. NCOS was already identified as an outlier in the first version of the manuscript, and we found during the revision of the manuscript that SUM should also be removed from the correlation analysis because it is located on a high altitude plateau with a very smooth relief due to the ice sheet formation.
- The aerosol parameters used for the correlation analysis are also chosen to reflect the ABL influence at stations that are at least occasionally located in the FT. The causes of the aerosol concentration minima and maxim as well as the diurnal and seasonal cycles are completely different for sites that remain in the ABL during the whole day. In that sense, neglecting stations situated at too low altitudes (like ZEP) is absolutely reasonable. In our study, HPB and MSY, two middle altitude stations, decrease the correlation coefficient values without destroying the correlation. They are now included into the correlation analysis and the related section (motly section 3.5 and 4.2) were modified.

p15,L29: All of a sudden back-trajectories appear. It seems clear that these are not the hydrological flow paths. But from which model do these trajectories come from and why were they not used for all sites to also characterise the thermal flow systems (even if not fully represented in the model).

- Back-trajectories were calculated by the CHC data owners and used in other studies. They are used in this study just as a comparison with the main flow paths as a function of the ABL altitude. Anyhow, the section 3.6 was removed in the revised manuscript as recommended by the second referee, so that this point does not need a more detailed discussion.

p16,L16-17: This argument is going round in circles. The absorption coefficient is supposed to be an indicator of ABL influence because it correlates with topoIndex. But I though it needs to be shown that the topoIndex actually represents ABL influence ... Very confusing.

- This sentence is actually mixing some statements from both the results and discussion sections. It was therefore modified: "*Our results showed that of the three aerosol parameters tested in this study (number concentration, absorption coefficient and scattering coefficient), absorption coefficient has the greatest correlation with the ABL-TopoIndex values.*"

p16,L26: NO3 being NO3_aq or ions?

- This correspond to particulate nitrate (NO_3^-) (Zellweger et al., 2003) and this is included in the revised version of the manuscript.

p19,L17f: These parameters are mostly know to the hydrological community but need additional introduction for the more atmospheric readership of the current journal. As mentioned before, it would have been better to provide such parameters to a statistical model with parameter selection in order to get an objective selection of parameters that may explain ABL influence. However, most these parameters would also follow the misleading assumption that thermally induced flow works just opposite to water flowing downhill and, therefore, should possibly not be considered at all.

- The authors did not consider at all that thermally induced flow can be considered as the opposite of water flow and most of these parameters were actually not used because of such discrepancies. However, as explained in the answers to the main comments (p.1 of this document), these parameters and the reasons for their rejection are now detailed as a table in the supplement (see Table S2 on p. 3)

Table1: Add the GTopo30 altitude of the grid cell containing each site, along with all the parameters derived for the site (potentially as supplement).

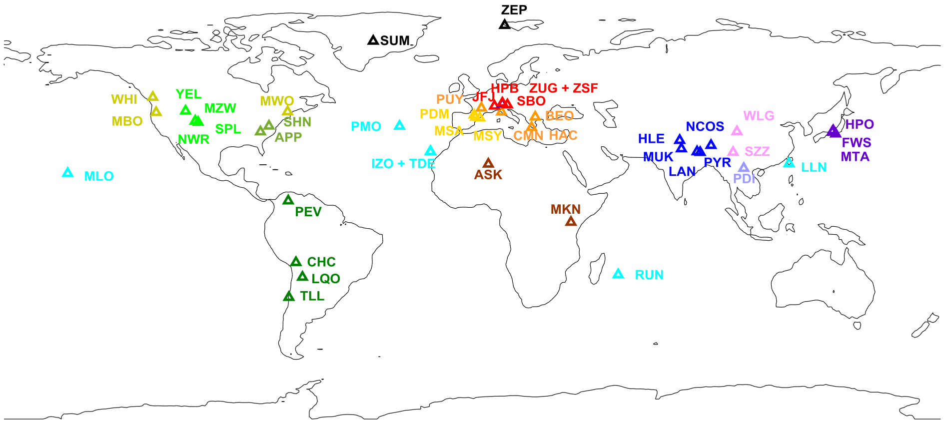
- The GTopo30 altitude of the grid cell as well as the mean for the 9 considered grid cells were added in the supplement with some comments. The altitude of the DEM grid cell as well as the mean altitude on the 9 used grid cells are given in the supplement Table S1.

Table2: The units for LocSlope should be m m-1 not Mm-1.

- Thanks for catching this! LocSlope has no units but there is a factor of 10^{-3} because the altitude is given in m and the horizontal distance in km. The values and units in Tab. 2 are corrected in the revised version.

Figure1: The figure quality is not state of the art. I suggest to use a topographic image as background. Larger station labels or symbols. Legend for mountain ranges.

- You will find thereafter Fig. 1 similar to the first version but with the right color scheme and a second version with the continental topography beyond the station location. If the first version allows to clearly visualize all stations, the second version also gives some information about the highest massifs around the world. The authors put the second version in the revised manuscript, but let the editor chose which figure should be finally used in the manuscript.



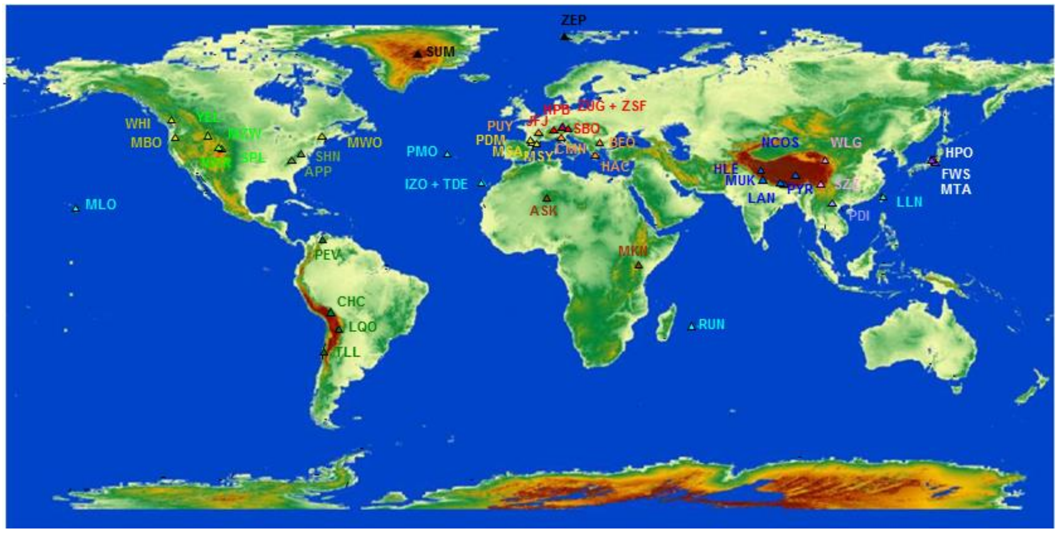


Figure2: The schematic is confusing. If you want to underline that there is a higher ABL influence on the right, why not show a visible, partially terrain following ABL in the mountainous area and an aerosol layer resulting from lift over processes. The schematic on the left is a very poor image of a mountain shape. Looks more like a life buoy with a signal post but not like the profile of a volcano.

-The referee is right, the schematic view was somewhat crude. The left schema is now changed. Since section 3.6 was deleted following the referee's comments, the added ABL was removed from Figure 2.

Figure4: The thick cyan line is not mentioned in the caption.

- OK, this now mentioned in the figure caption: "*The main flow paths from the station grid cell are given by the cyan lines.*"

Figure6: Sub-panel labels are missing in the figure but are used in the caption.

- OK, the sub-panel labels are now written in the figure.

Figure8: What are the different shades of colours? Neither explained in caption nor text.

- Some colors were changed in both Fig. 1 and 8 so that the color scheme of both figures are now similar. This is now mentioned in the figure caption of Fig. 8*: "The color scheme corresponds to that in Fig. 1."*

Figure9: Very difficult to comprehend. Too many colours and symbols in one plot. Why not display negative correlation coefficients as such on the negative part of the y axis. Instead of circles, different sized symbols should be used for different significance levels.

- As suggested by the referee, the statistical significance is now given by different symbol sizes and this clearly increases the readability of the figure. We keep however the negative correlation as downward triangles to keep the direct comparison between the absolute value of the correlation coefficients. Since the anti-correlated topography parameters are used as 1/parameter in the ABL-TopoIndex, the absolute correlation value is more important that its sign.

References:

Andrews, E., Ogren, J. A., Bonasoni, P., Marinoni, A., Cuevas, E., Rodríguez, S., Sun, J. Y., Jaffe, D. A., Fischer, E. V., Baltensperger, U., Weingartner, E., Collaud Coen, M., Sharma, S., Macdonald, A. M., Leaitch, W. R., Lin, N.-H., Laj, P., Arsov, T., Kalapov, I., Jefferson, A. and Sheridan, P.: Climatology of aerosol radiative properties in the free troposphere, Atmos. Res., 102(4), 365–393, doi:10.1016/j.atmosres.2011.08.017, 2011.

Collaud Coen, M., Weingartner, E., Furger, M., Nyeki, S., Prévôt, A. S. H., Steinbacher, M. and Baltensperger, U.: Aerosol climatology and planetary boundary influence at the Jungfraujoch analyzed by synoptic weather types, Atmos. Chem. Phys., 11(12), 5931–5944, doi:10.5194/acp-11-5931-2011, 2011.

GAW Report No. 200. WMO/GAW Standard Operating Procedures for In-situ Measurements of Aerosol Mass Concentration, Light Scattering and Light Absorption, (Edited by John A. Ogren), 134 pp. October 2011.

Guo, J., Miao, Y., Zhang, Y., Liu, H., Li, Z., Zhang, W., He, J., Lou, M., Yan, Y., Bian, L. and Zhai, P.: The climatology of planetary boundary layer height in China derived from radiosonde and reanalysis data, Atmos. Chem. Phys., doi:10.5194/acp-16-13309-2016, 2016.

Pal, S. and Haeffelin, M.: Forcing mechanisms governing diurnal, seasonal, and interannual variability in the boundary layer depths: Five years of continuous lidar observations over a suburban site near Paris, J. Geophys. Res. - Atmos., 120(11), 936–956, doi:10.1002/2015JD023268, 2015.

Poltera, Y., Martucci, G., Collaud Coen, M. and Hervo, M.: PathfinderTURB : an automatic boundary layer algorithm . Development , validation and application to study the impact on in-situ measurements at the Jungfraujoch ., Atmos. Chem. Phys., 1–34, doi:10.5194/acp-2016-962, 2017.