2nd review of Collaud Coen et al. "The topography contribution to the influence of the atmospheric boundary layer at high altitude stations"

The authors have at length responded to both referee comments. They mostly argue against major changes in the applied methods only dropping one part of the manuscript which was using hydrological flow path as an analogy to atmospheric flow, which was strongly critisised. Reading the authors responses and the revised manuscript I am only partly satisfied with the replies and modifications and suggest further modifications before publication in ACP is possible.

Points raised in the first review and commented by the authors:

1) **Neglecting lifting processes other than thermally induced, convective transport**

   My concern is not completely met by the authors reply. I still think it would be possible to create a meteorological criterion that would indicate situations with likely thermally induced flow from existing global scale model products. Don't forget that the latter have resolution down to 0.1 degree by now. Also it has been done successfully before with observational data, so why not check with model data. However, I see that this may go well beyond the scope of the current analysis and actually the authors' final point (although strictly valid only for one site) may provide an avenue of argumentation that is useful in the context of this study but may require some rethinking of ABL versus aerosol influence: It is correct that Zellweger et al. (2003) showed that increased aerosol surface area was mainly observed during thermally induced lifting events, whereas for other (gaseous) ABL tracers enhanced concentrations where also observed during other lifting events (synoptic and foehn). The reason for this is most likely washout of aerosol during the other lifting processes, since both foehn and synoptic lifting are usually connected with precipitation. So the argumentation in the current study should go somewhat like this. 1) In most cases aerosols can be a tracer for recent ABL contact, 2) however, many lifting processes co-occur with precipitation and, hence, aerosol washout. 3) The lifting process that often occurs without precipitation is thermally induced flow (but mind: thermally induced flow often also leads to deep convection and convective precipitation). 4) Therefore, the potential of this lifting process on aerosol concentrations at high altitude sites was studied. This should also be reflected in the title, which could be changed to:
   "Characterisation of topographic features influencing aerosol observations at high altitude stations"
   This would not claim the ABL influence but rather focus on the aerosol concentrations themselves and take notice of the fact that not all ABL events carry aerosols with them.

2) **Emissions**

   I don't agree that the emissions will not have an influence on the relative magnitudes of the diurnal cycle. The relative location of the dominating emission source is of crucial importance. One cannot assume that concentrations in the ABL or RL are horizontally homogeneous! For example the absence of local to regional emissions is most likely the reason why the sites ZEP, SUM and NCOS are later kicked out as outliers. Including a parameter that looks at the emission distribution as functions of altitude and distance from the sites would certainly be valuable and would potentially explain some of the inter-site differences. This should at least be given more though and attention in the outlook and should be looked at in future studies.

3) **ABL events**

My comment had in mind that transport frequencies will play a role as well when looking at the median data. I can see that 5th percentile is more likely to represent FT conditions and 95th percentile pollution events. But there should be more information available from the complete distribution function. The aerosol parameters may be distributed log-normally. In which case one could still look at the log-transformed values to identify different degrees of skewness. For a site frequently influenced by pollution events one would expect a positively skewed log-normal distribution. The diurnal cycles present a way forward from the simple absolute concentrations (as in Fig 9). But still these are averages over a whole year and the importance of thermally induced transport varies strongly with the amount of energy available for convective transport, depending on latitude and season. This might be the reason why, as the authors state, "these are however much more difficult to statistically extract from the time series". So if no kind of event detection or filtering of the observations for days with thermally induced transport is possible in the context of this study, I suggest to further comment on the factors that make the evaluation of the diurnal cycles difficult.

4) **Topographic parameters and their selection**

I am still not happy with one specific parameter and the way the parameters are finally selected and used in the ABL index. Just looking at Fig 9 and Fig 10 it is apparent that DBinv has almost no predictive power (besides the one lucky punch for the seasonal amplitude of the absorption coefficient which is not explained in any physical way). The parameter simply does not make any sense in a meteorological context (this was my point 5 in the previous review). There simply is no such thing as a reservoir for air convection. The parameter is large for sites that are at the highest point in their domain (e.g. BEO), so yes it has something to do with the relative topography (but hypso% seems to do the better job), but there is no reason whatsoever why a hydrological divide should also hinder atmospheric flow. Just look at the two examples given in Fig 4 and Fig 5. BEO is the highest point in the massif, so according to the authors, all air in the domain has the potential to flow upstream to the site. In contrast, the inverse drainage basin for PYR is limited to the north and east. But what happens during a fair-weather day with easterly winds at the site? FT air will move over the mountain ranges east of 90.1°E, receive the well know enrichments from ABL inputs (just as the authors describe for the Nyeki et al (2000, 2002) observations) and arrive at the PYR site. The hydrological border presents no boundary whatsoever for this transport. In contrast, for BEO I would argue that large parts of the "drainage basin" are at altitudes too low to trigger topographic convection to altitudes as high as the site itself. In addition and as mentioned before, a flow along the slopes to the highest point simply does not happen due to mass balance reasons (please check text-books on mountain meteorology). In summary, I would still suggest to drop DBinv from the calculation of the ABL index (as was already done for the flow paths which were based on the some wrong analogy). There is nothing lost in terms of explanatory power of the ABL index.

Instead one could also argue to also include the absolute altitude and the latitude in the ABL index, since they have even more predictive skills than DBinv (see Fig 9).
Another point is still the non-objective parameter selection. Thanks to the new table S1 one now has a chance to have a look at the used parameters. A quick “pairs plot” (see below) revealed that loc.slope and G8 are actually quite strongly correlated, which could mean that actually only one of them should be selected in a final predictor. Otherwise one gives this feature squared weights (due to geometric mean). The figure also shows that DBinv is largely independent of the other parameters (which could have been good), but that it also does not correlate well with the ABL index (the reason being its smaller relative variability compared to all other parameters). My idea about parameter selection was to build a regression model (linear or generalised) using the different topographic parameters as predictors and apply it to the aerosol parameters. This is in contrast to constructing one single index as the one and only predictor. Combined with a parameter selection procedure one could then test which parameters are the best predictors for which aerosol parameters. A regression model constructed in such a way would also allow some kind of physical understanding of the topographic parameters on the aerosol parameters that goes beyond rank correlations. A simple analysis of variance would also be beneficial and more conclusive as only looking at correlation coefficients between individual topographic parameters and aerosol parameters. I leave it up to the authors if they would like to improve their method in the suggested way or keep it in mind for future studies, but I strongly suggest to drop DBinv from the analysis.

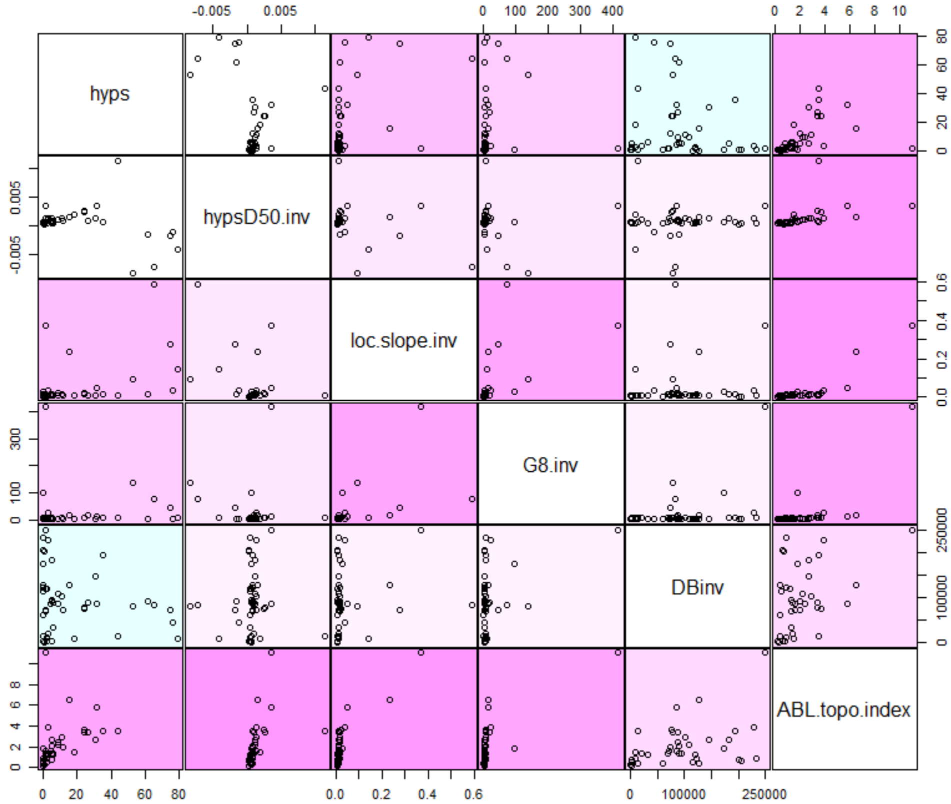

Figure 1: Pairs plot of topographic parameters and ABL index. Colour indicates strength of spearman rank correlation (cyan: negative correlation, magenta: positive correlation, darker colours represent stronger correlations).

Further comments on the revised manuscript (page numbers and lines refer to the track-changes version attached to the authors' response). These are partly new and came up when having a closer look at the topographic parameters now available in Table S1.

P2, L2: "drainage basin for air convection". Once more there is no such thing and the analogy is not valid. Since I suggest dropping parameter DBinv this term and concept can also be removed from the manuscript altogether. See the discussion above.

P6, L5f: Not clear what the accuracy refers to: the altitude differences between sites and GTOPO or the specification of GTOPO? However, what you should be worried about is not the accuracy of GTOPO which refers to the average altitude in each grid cell but the representativeness of GTOPO for the whole grid cell and the resulting mismatch with your station altitudes. So the results in Table S1 are the more important numbers.

I am also confused about the new text inserted in the revised manuscript. It is not the same as given in the replies (see page 14 of replies), which discusses the altitude differences in much more detail. Just there it is said that the altitude difference for NCOS is 1100 m, which is not true (see Table S1). However, some of the other large altitude differences should be mentioned in the manuscript not just in the reply.

P7, L5: Why are these two sites discussed when there are sites like WLG and MUK also showing larger hypso%? There are many more sites with large values as seen in Table S1. So the statement that most sites have hypso% smaller 5 % is not true! Actually 22 of 46 sites have values larger 5 %. Please be more precise in all your discussion! Don't let the reviewers do your work!

P7, L14: 6 out of 46 sites are situated under hyps50. What is the consequence? Should they even be discussed? The new sentences just before suggest that these sites cannot claim to be high altitude stations!

P7, L16f: This is a hypothesis so far not a fact. Please rephrase.

P7, L23: A "plateau" is a high plain (https://en.wikipedia.org/wiki/Plateau). So here the word valley or plain should be used instead.

P9, L30f: This argumentation is confusing and misleading. First of all it is not the hydrological drainage basin that is used in the ABL index but the inverse drainage basin (5d rather than 5c). Then the size of the basin is made responsible for the high ABL index. However, just above we have seen that a large inverse drainage basin does not lead to a high ABL index (inverse drainage basin for BEO is actually larger than the one for PYR). So I would say that one of the other parameters is the more important one in terms of this discussion, most likely hypso% which differs by 3 orders of magnitude between the two sites. However, the discussion of DBinv will need to be removed from the manuscript anyway.

P10, L6: This should be 50 x 50 to 1000 x 1000 km^2.

P13, L6: MWO supposed to have a "low DBinv", but a value of 127457 for DBinv at MWO means that the site has a DBinv larger than the mean, larger than the median and even larger than the 75% percentile of all sites (see Table S1).

P14, L12ff: Once more, I can only say that it is surprising that the ABL-Index does not work for such sites that are clearly not high altitude sites. How can we then trust the method to rank sites at different degrees of high altitude? One common factor at all three sites is the absence of local to region emissions! Hence, aerosols no longer are a good estimator of FT vs ABL conditions.

P15, L9ff: One should mention that these two parameters are actually strongly correlated (r=0.76, spearman)! The highest correlation between all parameter pairs! So it is no surprise that they both show similar characteristics and actually one could be sufficient for this study.