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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "The topography contribution to the influence of the atmospheric boundary layer at high altitude stations" *by* Martine Collaud Coen et al.

Anonymous Referee #2

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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

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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.

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).

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.

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.

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 fair-weather 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.

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 ..."

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.

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 un-organised. See: http://glossary.ametsoc.org/wiki/Convection. Why not talk about "thermally induced flow paths" instead

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

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

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.

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).

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.

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.

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.

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.

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.

p6,L11: Very questionable that these parameters are quantitative

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

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calculated parameters as part of table 1.

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

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 normalised for example by their variance.

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.

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.

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.

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

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

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

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).

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.

p14,L30: Isn't the failure of the 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.

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).

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.

p16,L26: NO3 being NO3_aq or ions?

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.

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

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

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.

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.

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

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

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

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.

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