Review of „The impact of embedded valleys on daytime pollution transport over a mountain range“ by Lang et al. 2015

General comments:

The authors used idealized LES simulations with WRF to investigate the impact of mountain geometries on daytime pollution transport. The manuscript relates to recent idealized studies, which investigated the impact of different valley topographies on the boundary layer structure and the daytime vertical exchange between the boundary layer and the free troposphere. The main objectives of this manuscript were to (i) investigate the interactions between plain-to-mountain and slope wind systems and (ii) to analyze their influence on daytime pollution distribution over complex terrain. The authors performed a tracer analysis over a quasi-two-dimensional mountain range with embedded valleys bordered by ridges with different crest heights and a flat foreland. They varied the valley depth systematically by varying the valley floor height and compared the results to single ridge topography. Due to mountain and advective venting, the vertical exchange was 3.6 times higher over complex terrain than over a flat plain, although the calculated exchange strongly depended on the definition of the atmospheric boundary layer height.

The manuscript is well written, the structure is clear and figures are of good quality. The research questions are clear and the authors refer to and compare their results to related previous work in a sufficient way. Although the present study is based on idealized LES simulations, the use of an orography consisting of embedded valleys bordered by asymmetric crest heights provides a valuable step towards a better understanding of more realistic, complex orography. In my opinion, the manuscript is excellent in most parts. However, I still have several comments and suggestions. The most relevant issues are related to the usage of different boundary layer heights (see specific comment 1) and the lack of some discussion points (see specific comment 4). I believe this manuscript is suitable for publication in ACP after minor revisions.

Specific comments

1. The usage of ABL, AL and CBL is confusing. Currently the authors use atmospheric boundary layer, boundary layer, convective boundary layer, ABL and CBL1, CBL2, CBL3 and AL. Although the definition of the atmospheric boundary layer over complex terrain is difficult, the authors should make an effort to clarify its nomenclature. I think the ambiguity and readability of the manuscript would benefit a lot. In the introduction the authors provide a definition of the ABL (over homogeneous, flat terrain). In Section 3 the authors describe one method to detect the AL and 3 methods to detect the height of the ABL and name these CBL1, CBL2 and CBL3. What about reducing the number of terms? The daytime ABL might as well be named CBL. The authors could introduce the CBL in the Section 1 and then define the AL height and different CBL heights

in Section 3 (CBL1, CBL2 and CBL3). This would also be in accordance with the nomenclature in e.g. de Wekker et al. (2004).

2. Once abbreviations are introduced, they should be used. For example, HMIN0 denotes the reference run. I suggest that the authors use HMIN0 instead of reference run once it is defined. The same accounts for convective boundary layer and CBL.
3. A clear and uniform denotation of the different ridges and slopes containing information about their position in x-direction should be included in Section 2. For examples, the ridges could be named first ridge (for the small ridge at -13.9 km) and main ridge (for the second ridge at 0 km) and the slopes could be named slope 1, slope 2 and slope 3 (from left to right). The authors refer to lee side slopes in the manuscript (e.g. p. 11, l. 25; p.12, l. 3; p. 12, l. 5). In an environment without a large-scale wind this seems not adequate. In the caption of Fig. 4 the slopes are already numbered in a similar way.
4. I think it would be interesting and helpful to discuss certain topics of the paper more detailed. The assumption for the model setup (constant sensible heat flux) is not realistic and cause limitations for the transferability of the results to reality (usually you have instationary and inhomogeneous (shading) forcing conditions). This should be discussed either in the conclusions or in an additional discussion section. Furthermore, some of the model results (downslope windstorms, non-stationarity of the flow, …) would benefit from a discussion either at the respective section in the manuscript or in an additional discussion section. These points are also mentioned in the comments below.

p. 2, l. 7: It might be good to add the information to the abstract that the valleys are bordered by asymmetric crest heights, as this is an interesting feature of the study.

p. 2, l. 22: The described structure of the typical daytime ABL (CBL ?) is strictly speaking only valid for flat, horizontally homogeneous terrain. Maybe rephrase: “The typical daytime ABL (CBL ?), which forms under fair weather conditions over horizontally homogeneous and flat terrain, consists of …”

p. 3, l. 4-9: Thermally driven flows not only provide a vertical transport mechanism, they also impact the temperature and humidity distribution via horizontal and vertical advection and hence the CBL height. When determining the CBL height via temperature profiles it is assumed that temperature structure is dominated by vertical mixing and reflects the result of turbulent vertical mixing. This may often not be the case over complex terrain. Several studies reported low (e.g. Rampanelli et al., 2004; Rampanelli and Zardi, 2004; Adler and Kalthoff 2014) or non-existent (e.g. Khodayar et al., 2008) mixed layers in valleys, although convection was present. Thus, the definition of the CBL height via temperature profiles over complex terrain may be often problematic (e.g. Weigel and Rotach, 2004). Catalano and Moeng (2010) propose that classical CBL definitions (based on the minimum of the vertical heat flux or the maximum potential temperature gradient) are inapplicable over complex terrain. I suggest shortly mentioning these problems in the introduction (maybe in a new paragraph about CBL height detection, see comment below).

p. 3, l. 12-16: Different thermally driven flows also interact, which is nicely described in Zardi and Whiteman (2013).

p. 4, l. 1: I suppose it is more a superposition of slope and plain-to-mountain wind which transports the air further upslope.

p. 4, l. 2: Upper-branch return flows are often weak, obscured by large-scale flows and often not observed (Zardi and Whiteman 2013). I suggest adding: “Under ideal conditions an upper-branch return flow …”.

p. 4, l. 8: Why are mountain and advective venting dynamically induced mesoscale flows? Please clarify.

p. 4, l. 14 – p.5, l. 2: The structure of this paragraph is confusing. The authors mix modeling problems due to coarse resolution models with problems of CBL height detection. I suggest splitting the paragraph into two. para 1: Problem of CBL height detection: “When studying vertical exchange processes between the CBL and the free troposphere the detection of the CBL height is critical. Most conventional concepts for the determination of the CBL height are developed …” para 2: model problems: “As about 50 % of the Earth’s land surface … climate studies. Today’s operational …”

p. 6, l. 15: Why did the authors choose a constant sensible heat flux and not a constant net radiation? I suppose it is not possible to transform the net radiation into the sensible and soil heat flux (via an energy balance equation). However, the sensible heat flux normally adapts to the ambient temperature so that it changes with height. The authors should at least discuss the selected boundary conditions with respect to reality.

p. 7, l. 10-14: The mountain range consisting of three ridges rather extends over about 40 km than 60 km. Thus, the comparison with the European Alps might be a little optimistic. Which embedded valley in the Alps are the authors referring to?

p. 8, l. 15: Why does the tracer source cover the lowermost 8 model levels? Is this arbitrary? Or does it relate to the Prandtl layer? Please indicate.

p. 8, l. 25: “… the model output ~ψ(x, t) …” ?

p. 9, l. 5-6: Why did the authors choose an averaging interval of 40 min? Maybe mention that in the following only ⟨ ⟩ variables are plotted. Are all the variables temporally averaged over 40 min? Why is it only possible to show variables for simulation times after 1.5 h? How does that relate to the averaging interval of 40 min.

p. 9, l. 12: I suggest removing the first sentence.

p. 10, l. 2: “… a threshold of 0.001 K m-1 as proposed by Catalano and Moeng (2010).” Catalano and Moeng (2010) used the additional constraint that the heat flux is less than 15% of its maximum value. Did the authors use this constraint as well?

p. 11, l. 10: Why do the authors average temporally? Does the flow field not get stationary after a certain time? If not, what is the reason for the non-stationarity? Is it related to the constant sensible heat flux?

Over what period is temporally averaged when the cross sections after 6 h are shown? This applies to all figures in the following. Maybe clarify this in Section 3.

p. 11, l. 12-14: I cannot see that the CBL heights over the first ridge are similar in all simulations. It looks more like the CBL height in HMIN0 is higher due to the upwind region over slope 2.

p. 11, l. 17-18: In my opinion, only the CBL heights in HMIN0.5 and HMIN 1 are roughly similar to that in S-RIDGE. What do the authors mean with depth of the ML? How do they determine ML from Figure 3? Do they mean the depth of the CBL, which is the CBL height minus terrain height?

p. 11, l. 24-25: I cannot distinguish different slope wind depths in Fig. 3a. Maybe move this information to the paragraph when the profiles of mean cross-mountain winds are discussed (p. 12, last para).

p. 11, l. 28: I cannot see a return flow above the CBL1 height over slope 2.

p. 12, l. 11-14: It might be helpful for the reader to mention the layers in which the return flows develop (e.g. between 2.5 and 3 km) and their position in x-direction (e.g. x < -10 km).

p. 12, l. 16-19: See previous comment.

p. 12, l. 24-25: I suggest rephrasing this sentence (specific comment 3). Maybe: "The depth of the slope wind layer is shallower over slope 3 than over slope 1 and 2."

p. 12, l. 29: "… than over the slope of the first ridge."

p. 12, l. 29 – p. 13, l. 2: Is this a continuous process? What happens after 4 h? Is there a pulsation in the flow?

p. 13, l. 3-5: Foehn has specific characteristics as stated in Richner and Hächler (2013): "Foehn is a generic term for a downslope wind that is strong, warm and dry." In the next paragraph (p. 13, l. 11-12) the authors state that "…the air over the first mountain ridge is potentially cooler than the valley air and therefore able to descend into the valley". This means that the descending flow is similar to a density current and not to Alpine Foehn. Thus, in my opinion a comparison with Alpine Foehn is not adequate.

p. 13, l. 14-18: Zardi and Whiteman (2013) state that the reason for the anomalous Maloja wind is the peculiar topography in the Maloja Pass region, where the Bergell ridgelines extend beyond the pass into the Upper Engadine Valley. I do not think that these peculiar topographic features apply to the present idealized simulations and thus recommend not referring to the Maloja wind.

p. 13, second para: This paragraph about potential temperature structure does not really fit into Section 5.1 (Flow structure). It might be better to move this paragraph to the next section and postpone the explanation of the descending flow. Similar sentences could be added to the end of Section 5.1: "The evolution of different flow regimes was caused by different temperature structures. This is discussed in detail in Section 5.2.".

In my opinion the downslope flow regime needs more attention. If the authors do not want to perform a more detailed analysis here, I would at least recommend shortly discussing open questions and issues and possible explanations. For example: Why does the plain-to-mountain flow accelerate when it descends into the valley? What is the potential temperature difference between the intruding air mass and the valley air? Does the potential temperature difference allow a penetration to the bottom of the valley or are there other mechanisms involved?

p. 13, l. 22-23: Why do the authors average the profiles over the whole valley domain? They mix atmospheric characteristics from close to the slopes (slope wind layer) with characteristics in the valley center (subsidence area). Wouldn't it be more straightforward just to show one profile e.g. from the center of the valley? Are the results similar when doing so?

p. 13, l. 25-28: In the introduction the authors state that the typical daytime ABL consists of a surface layer, a mixed layer (ML) and a stably stratified layer. Here they use the term well mixed convective boundary layer. Which part of the ABL do the authors refer to? I guess to ML? What is the CBL height at this time?

p. 14, l. 25-27: In Fig. 4 the authors show that at 2 h in HMIN0.5 there is still an upslope flow on the second slope. How is it possible that at the same time horizontal advection of cold air with the plain-to-mountain flow causes a cooling of the control volume?

p. 15, top: How is it possible that the downslope flows exist at 6 h in HMIN0.5 and HMIN.1 and not in HMIN0 (Fig. 3), even so the potential temperature profiles are similar at 6 h in all three runs (Fig. 6b). I think the manuscript would benefit from a more detailed discussion at the end of Section 5.2 relating the different flow structures to the temperature structures and heating in the valley.

p.15, l. 11-13: Are the tracer mixing ratios at 6 h also temporally averaged?

p. 15, l. 24-25: Reconsider the naming. Maybe better: "… over slope 2… and in the upper part of slope 3… " and please specify the region more precisely e.g. "(-13 km < x < -10 km) … "(-1 km < x < 0 km)".

p. 16, l. 1: What ABL heights do the authors refer to? CBL1, CBL2, CBL 3 or AL? See also specific comment 1.

p. 16, l. 7-9: I agree that AL heights in HMIN0.5 and HMIN1 are higher than CBL heights but the region where this is the case (around x<-8 km) is not the same as in HMIN0 (around -10 km < x – 3 km). This should be mentioned in the text instead of "As in the reference run, the AL heights are considerably higher than CBL heights,…" How much higher are the AL heights in HMIN0.5 and HMIN1?

p. 16, l. 9-10: Note that similar elevated pollution maxima were modelled by Fiedler et al. (2000) and elevated moist layers downstream of mountain ridges related to advective venting were observed by Adler et al. (2015).

p. 16, l. 20-21: This is a repetition (see p. 11, l. 14-16).

p. 17, l. 3-5: I do not see in Fig. 9 how vertical transport beyond CBL1 increases up to 55 % for cases with elevated layers. I can only see an increase up to 50 % at 6 h for HMIN1. It might be clearer to say: "… CBL1 height increases up to 50 % for the HMIN1 case and up to 55 % for the S-RIDGE simulation."

p. 17, Sect. 5.4: For clarity it might be helpful to include subsections. For example: 5.4.1: Tracer emission over slope 3; 5.4.2: Tracer emission at foot of mountain range (or slope 1); 5.4.3 Tracer emission at valley floor.

p.18, l.2-9: Why does tracer occur downslope of the emission point in HMIN0 (Fig. 10a)? From Fig. 3 and Fig 10 it seems like the updrafts and vertical transport of tracer above the main ridge are rather similar for simulations with valleys and with a single ridge. Why do the authors state that "In the simulations with valleys, the rather strong vertical updrafts transport most of the tracers vertically through the boundary layer top (Fig. 10a, b). … in the S-RIDGE simulation, both mountain and advective venting occur to the same extent"? Is there no advective venting in HMIN0 and HMIN0.5? What does "closer inspection" mean? It would be interesting to know.

p. 20, l. 21: You could add the information that the simulations were performed with WRF.

p. 20, l. 23: Please add the information that the embedded valley are bordered by two ridges of different heights.

p. 21, l. 1-3: "… opposes the plain-to-mountain wind, which flows over the crest of the first ridge,… ". Why does the plain-to-mountain wind pass the ridge crest in HMIN0? What determines the location of the convergence zone over slope 2?

p. 22, l. 2: On p. 16, l. 3 the authors state that the CBL heights are up to 0.8 km lower than the AL height.

Technical comments:

1. The authors only refer to full hours. Still in the figures (Figs. 3, 5, 6, 8, 10, 11 and 13) the minutes are included in the time stamp as decimal place. For uniformity, it might be better to remove the decimal places.

p. 8, l. 22: "… Schmidli (2013) and Wagner et al. (2014a)…"

p. 9, l. 16: "… for the top of the daytime ABL over homogeneous and flat terrain."

p. 10, l.4: "… using the same Richardson number…". As Ri is only used once more in the manuscript I suggest removing the abbreviation.

p. 10, l. 14: "… of horizontally averaged vertical sensible heat flux, normalized tracer mixing ratios, and …"

p. 10, l. 16: "… vertical sensible heat flux"

p.15, l. 22: "3.3 % km^{-1}"

p. 15, l. 27: "… over slope 2 (-12 km < x < -10 km)"

p. 16, l. 1: "5.9 % km^{-1}"

p.18, l. 2: "… to the free troposphere…"

p. 18, l. 18: "… which extends approximately 500 m higher up to about 3 km than …"

p. 18, l. 22: "… is more evenly distributed between…"

p.18, l. 29: "… up to the convergence zone on the second slope …"

p. 19, l. 3-5: "In contrast to the reference run, the tracer particles in the HMIN0.5 simulation are transported horizontally …. (Fig. 11b)."

p. 19, l. 5: For clarity, it might help to start a new paragraph here.

p. 19, l. 26: "… leads to a rather continuous increase in time …"

p. 20, l. 6: For clarity, it might help to start a new paragraph here.

p. 29: "… total vertical sensible heat flux profiles…". In the label of the colorbar: "Vertical sensible heat flux" and please add a blank in "W m^{-2}"

p. 30: Please add the information that the variables are temporally and spatially averaged.

p. 31: "… and (c) the middle of the third slope." Why do you not show the profiles at 6 h as well? Are they different? Please add a blank in "m s^{-1}".

p. 33: Please add the information that the variables are temporally and spatially (along y-direction) averaged.

p.35 Fig. 8e: Where does the peak in AL at -10 km come from? It is not evident in the mixing ratio distribution. Please change the y-axis labels "% km^{-1}" and the legend "K m^{-1}" and add the information that potential temperature is shown as black contours (?).

p.37: Please change the y-axis labels to "% km^{-1}" and the legend to "K m^{-1}".

p.38: Please change the y-axis labels to "% km^{-1}" and the legend to "K m^{-1}".

p.40: Please change the y-axis labels to "% km^{-1}" and the legend to "K m^{-1}".

References:

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