

The impact of embedded valleys on daytime pollution transport over a mountain range

Reply to Reviewers' Comments

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1 Introduction

We thank both referees and the editor and acknowledge their efforts to improve our manuscript. We have revised the manuscript according to their fruitful comments and suggestions. In the manuscript, changes in the text are written in red color.

In the following, comments of the reviewers and replies of the authors are written in italic type and normal font, respectively.

2 Comments of Referee #1

Minor 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 2.3.4. in Section 3 (CBL1, CBL2 and CBL3). This would also be in accordance with the nomenclature in e.g. de Wekker et al. (2004).*

⇒ We agree and removed the term “atmospheric boundary layer (ABL)” from the manuscript. We now introduce the CBL and AL in Sect. 1 and define the different CBL and the AL heights in Sect. 4.

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.*

⇒ We agree with the term “convective boundary layer” and refer only to the abbreviation “CBL” after its introduction in Sect. 1. However, for the simulations we prefer to use both versions, i.e., acronyms and more descriptive names such as “reference run” and “simulations with elevated valleys”. This allows us to point to specific terrain features when comparing different simulations (e.g. “elevated” versus “deep” valley) and also avoids repetitions.
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.*

⇒ We agree and have added a definition of the different ridges and slopes in Sect. 2 (p. 8, l. 2-5).
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.*

⇒ We tried to describe the motivation and limitation of the model setup more clearly in Sect. 2. In terms of surface forcing our approach is the same as in Wagner et al. (2014a). However, we now refer also to sensitivity tests performed by Schmidli (2013), which support the constant surface heat flux forcing applied in our simulations. In other words, it is clear that the flow and CBL structures at an arbitrary point in time of a simulation with constant heat flux and one with a time-dependent forcing will be different. However, at a time of similar integrated heat input (e.g., at noon) the structures will be comparable. Additionally, we refer to the recent study of Leukauf et al. (2015) which shows a nearly linear relation between the amplitude of the sensible heat flux and the amplitude of the net shortwave radiation. Hence, prescribing the heat flux instead of the radiative forcing will not fundamentally change the results. The discussion on downslope winds and flow separation was extended according to geometric and dynamical considerations (cf. minor comment #1 of referee #2 and close-ups of the flow in the end of this document).

Specific comments

1. *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.*

⇒ We added this information in the abstract (p. 2, l. 7).

2. *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 ...”*

⇒ We agree and rephrased this sentence according to your suggestion (p. 2, l. 21-22).

3. *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).*

⇒ We are thankful to the reviewer for this detailed literature survey and added this information within a new paragraph about CBL height detection (p. 4, l. 11-25; see also reply to comment below).

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

⇒ We absolutely agree and included this additional information in the manuscript (p. 3, l. 14-15).

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

⇒ We agree and modified the expression accordingly (p. 3, l. 28-29).

6. *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 ...”.*

⇒ We agree and added your suggestion (p. 4, l. 1).

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

⇒ Generally, mountain and advective venting are mesoscale processes exporting CBL air to the free atmosphere, where mountain venting is characterized by a vertical transport and advective venting by a horizontal transport through the CBL top. These definitions are independent of the respective forcing. Examples for dynamically induced mountain and advective venting are orographic lifting and large-scale winds, respectively. Nevertheless, we agree that without a more detailed explanation this information might confuse the reader. Hence, we removed “thermally and dynamically induced” from the manuscript.

8. *p. 4, l. 14 - p. 5, l. 2: The structure of this paragraph is confusing. The authors mix modeling problems 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 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 ...”*

⇒ We agree and have split the paragraph accordingly to your suggestions into two separated ones (p. 4 and 5).

9. *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.*

⇒ We chose a constant sensible heat flux to keep the forcing as simple as possible. Sensitivity tests of Schmidli (2013) show that a prescribed incoming shortwave radiation of 400 W m^{-2} results in a mean surface sensible heat flux of about 150 W m^{-2} . Additionally, Leukauf et al. (2015) state that the amplitude of the sensible heat flux increases about linearly with the solar forcing. We have added this information in the respective paragraph (see also answer to specific comment #4 of referee #2).

We agree that the sensible heat flux may change with height [cf. Fig. 1a in Schmidli (2013)]. Nevertheless, the motivation of our approach is based on sensitivity test of Schmidli (2013) who found only minor quantitative differences and a similar flow structure when comparing simulations with constant and time-dependent forcing. We added this information to the manuscript (p. 6, l. 15-28).

10. *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 the Alps are the authors referring to?

⇒ We agree and compare the scale of the embedded valleys with real valleys such as the Inn Valley now. The latter has a valley width (from peak to peak) of approximately 15 km (p. 8, l. 6-8).

11. *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.*

⇒ This choice is not completely arbitrary but motivated by similar depths of the pollution layer observed in the morning in the Inn Valley (e.g., Gohm et al 2009). We added this information to the manuscript (p. 9, l. 3-4).

12. *p. 8, l. 25: "... the model output $\tilde{\psi}(\vec{x}, t)$..." ?*

⇒ No, the model gridbox average $\bar{\psi}(\vec{x}, t)$, which is dependent on mesh size and the time step of the numerical model, can be separated into a mean and a fluctuating part according to Eq. (4). This relation is used to compute the resolved turbulent part $\psi''(\vec{x}, t)$.

13. *p. 9, l. 5-6: Why did the authors choose an averaging interval of 40 min? Maybe mention that in the following only $\langle \rangle$ 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.*

⇒ Wagner et al. (2014b) tested different averaging intervals which reveal that at least 30 min of averaging is necessary to obtain values of resolved turbulent kinetic energy, which are independent of the averaging interval used. As a value of 40 min was also chosen by Schmidli (2013), we decided to use it in our simulations too. All variables are temporally averaged over 40 min (see also reply #16). According to the method described in Wagner et al. (2014a) the computation of the sensible heat flux involves two steps of averaging: at first the resolved perturbations (e.g., w'' , θ'') have to be computed by averaging over 40 minutes. This means that after 40 minutes it is possible to compute the (instantaneous) resolved heat flux. This heat flux has to be averaged itself over 40 minutes to get a mean resolved heatflux. This means that after 40 minutes another 40 minutes of averaging are necessary. Because of this procedure mean heat fluxes are available at the earliest after 80 minutes. These calculations are done "online", i.e., during the model integration. However, the data output interval is 30 min. Hence, the first useable mean heat flux field is available after 90 minutes (1.5h).

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

⇒ We removed the first sentence and adapted the first paragraph of Sect. 4.

15. *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?

⇒ No, following the studies of Schmidli (2013), and Wagner et al. (2014a,b, 2015) and to produce comparable results, we did not use the additional constraint that the heat flux is less than 15 % of its maximum value. We changed the formulation in the manuscript accordingly (p. 10, l. 18-20).

16. *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.*

⇒ In order to be able to decompose the flow into mean advective, resolved turbulent, and subgrid-scale parts according to the methods described in Sect. 3, we need to spatially and temporally average. This approach enables the computation of the decomposed heat fluxes (Fig 7). As described in the Appendix A of Schmidli (2013) the condition to spatially and temporally average is an approximately stationary and homogenous flow in along-mountain direction. This is approximately fulfilled in our simulations. The reason why we only show space and time averaged variables is that we are mainly interested in the mesoscale flow structures, which are hard to recognize in a fully turbulent flow field. All variables are averaged according to Eq. (5), which is a centered average in time over 40 minutes. Therefore, the output shown after 6 hours is averaged between 5h 40 min and 6 h 20 min. In our opinion this becomes clear from Eq. (5), but we added the additional information, that all variables shown are temporally and spatially averaged in the end of Sect. 3.

17. *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.*

⇒ Thank you for pointing to this potential misunderstanding. We now use the formulation “over the crest of the first ridge”. Additionally, we added the difference of the CBL1 over the valley between HMIN0 run and the simulations with elevated valleys in the following sentence (p. 12, l. 4-8).

18. *p. 11, l. 17-18: In my opinion, only the CBL heights in HMIN0.5 and HMIN1 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?*

⇒ We agree and adapted the statement. Yes, we mean the CBL depth, which is defined as the CBL height minus the terrain height. To prevent further misunderstanding we added this explanation as a footnote to the manuscript (p. 12).

19. *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).*

⇒ We agree and removed this information from the paragraph.

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

⇒ We are referring to the return flows towards the foreland and added this information to the manuscript (p. 12, l. 18).

21. *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).*

⇒ Thank you for this helpful suggestion. We added the heights of the layers to the manuscript, but not their position in x-direction, as the previous sentence in the manuscript specifies that return flows develop from updrafts towards the foreland (p. 13, l. 1-4).

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

⇒ Again, we added the height of the upper layer to the manuscript. However, is it not possible to specify an absolute height of the lower layer as it is located slightly above the terrain following CBL1 height. We mention this in the manuscript, too (p. 13, l. 7-10).

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

⇒ We agree and rephrased the sentence (p. 13, l. 15-16).

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

⇒ We added this extension to the sentence (p. 13, l. 19).

25. *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?*

⇒ Yes, the convergence zone is continuously shifted towards the valley floor (cf. Fig. R1). After 4 h the downslope wind over slope 2 and the upslope wind over slope 3 further intensifies and the flow regime 2 (cf. conceptual diagram in Fig. 15b) is established. No pulsation in the flow could be observed based on the available data, but the half-hour model output is probably too coarse for ruling out higher frequency pulsations.

26. *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.

⇒ We agree that a Foehn is typically "a wind warmed and dried by descent, in general on the lee side of a mountain" (WMO 1992). But the conceptual model of foehn that fits best the results of the latest large field campaigns [e.g., the Mesoscale Alpine Programme (MAP; Mayr and Armi 2008)] is, that the descent of upstream air is possible when the potential temperature of the descending upstream air mass is equal to or lower than the air in the downstream valley. Therefore, in our opinion the comparison with Alpine Foehn is adequate. However, due to your comment we removed this comparison from the manuscript.

27. *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.*

⇒ We agree that these peculiar topographic features do not apply to the present idealized simulations, but we did not want to state that the same causes as the Maloja wind lead to observed overflow of the first ridge in our simulations. We referred to the Maloja Wind in order to give an example of a similar phenomenon observed in reality that causes anomalous daytime downslope winds that are thermally driven. But we agree that this might be misunderstood by the reader and we removed this reference from the manuscript.

28. *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."*

⇒ We agree and moved this paragraph to Sect. 5.2. We also modified the end of Sect. 5.1 according to your helpful suggestions.

29. *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?*

⇒ The discussion of the downslope wind was extended according to geometric and dynamical considerations. According to the comments of referee #2, we

now state that a similar flow separation over slope 2 might also be possible due to dynamical reasons over steep slopes without a thermally-driven counter current. We tried to address your suggested discussion points in more detail in the manuscript (p. 14), however in our opinion a profound discussion of the leeside flow acceleration would go beyond the scope of the present study.

30. *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?*

⇒ We understand the problem you are referring to, and yes the profiles slightly differ (especially after 2 hours of simulation). But we are interested in the mean temperature characteristics of the whole valley in a bulk perspective. This is especially relevant when using the profiles in the analysis of the heating of the valley regarding the valley volume effect in the next paragraph. Additionally, this procedure allows us to compare our results to the profiles over the plain (not shown).

31. *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?*

⇒ According to Schmidli (2013), the definition of the CBL1 height equals the top of the ML. We now explicitly point to this fact in Sect. 4 (p. 11, l. 8-10) and also modified the present paragraph accordingly (p. 15, l. 1-4).

32. *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?*

⇒ Generally, it is true that at 2 h simulation time there is an upslope wind present at the middle of slope 2 ($x = -10.7$ km) as shown in Fig. 4. However, an advective cooling of the valley volume by the superposed plain-to-mountain wind is still possible above the upper part of the slope 2.

33. *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.*

⇒ We agree that the potential temperature profiles are similar at 6 h in all three runs. However, the shifting of the convergence zone is a rather continuous process. If we had heated for a longer simulation period, the superposed

plain-to-mountain might have descended to the valley floor. In contrast to this possible development are the geometric and dynamical considerations according to the comments of referee #2 (cf. Fig. R1 and R2 in the end of this document). To reduce the number of free parameters, we prescribe a constant surface heat flux in this study. Generally, we agree that such considerations are interesting but in our opinion not relevant for the key aspects addressed in our study.

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

⇒ Yes, the tracer mixing ratios are also temporally averaged. This information is also given in the caption of the Figure. Additionally, according to a comment above, we added the information that all shown variables are averaged in along-mountain direction and time in Sect. 3.

35. *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. $(-13 \text{ km} < x < -10 \text{ km}) \dots (-1 \text{ km} < x < 0 \text{ km})$.*

⇒ We changed the naming according to your minor comment #3 and specify now the region more precisely.

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

⇒ We meant all CBL and AL heights and specify this now in the manuscript (p. 16, l. 28).

37. *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 $x < -8 \text{ km}$) is not the same as in HMIN0 (around $-10 \text{ km} < x < -3 \text{ km}$). 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?*

⇒ We specified the height differences and the respective areas more clearly in the manuscript (p. 16, l. 25 - p. 17, l. 7). The AL heights in HMIN0.5 and HMIN1 are up to 0.9 km higher than the CBL1 heights.

38. *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).*

⇒ Thank you for these useful references. We added them to the manuscript (p. 17, l. 14-16).

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

⇒ We make this repetition since another aspect of this feature is discussed on p.11, l. 14-16. But we point now to the relevant section in order to emphasize that this is a repetition (p. 17, l. 22).

40. *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."*

⇒ We changed the statement according to your comment (p. 18, l. 2-4).

41. *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.*

⇒ Due to the relatively short paragraphs we decided not to include subsections. However, we do recognize the necessity for a clearer structure of this section. Therefore, we introduced the different subdomains as "keywords" in the beginning of the section. We also modified the introductory sentences of each tracer experiment and repeated the respective keywords (subdomains).

42. *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.*

⇒ As can be seen in Fig. 3, the upslope wind layer over slope 3 in the simulations with elevated valleys is much deeper than in HMIN0 and reaches almost up to the CBL1. Therefore, most of the tracers are transported within the slope wind layer upslope towards crest and are lifted within the updrafts to the free atmosphere. Furthermore, the deeper slope wind layer prevents subsidence in the center of the valley, whereas subsidence exists in the HMIN0 simulation. In the HMIN0 simulation, detrainment processes on the top of the slope wind layer and subsidence within the valley can lead to a turbulent tracer transport to the left and downward of the emission point.

The determination whether mountain or advective venting occurs is quite difficult for a pure thermally driven wind without a large-scale flow. Close-ups of the flow fields revealed very similar updrafts for all different simulations. However, the CBL is rather terrain-following and therefore not as "steep" over the crest of the S-RIDGE simulation than over the ridges of the simulations with valleys. In the S-RIDGE simulation, this favors a horizontal transport through the CBL height and advective venting is therefore more present than in the simulations with valleys.

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

- ⇒ We added the information that the simulations were performed with WRF in the conclusions of the manuscript (p. 21, l. 17).
44. *p. 20, l. 23: Please add the information that the embedded valley are bordered by two ridges of different heights.*
- ⇒ We added the information to the conclusions of the manuscript (p. 21, l. 20).
45. *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?*
- ⇒ In the reference run, the advected air at crest height over the first ridge has nearly the same potential temperature than the air advected upslope from the valley. Due to the deeper valley in HMIN0 compared to the elevated valleys in HMIN0.5 and HMIN1, a more distinct upslope circulation establishes over slope 2. In our opinion both facts mainly prevent the plain-to-mountain wind to descend to the valley floor during the entire simulation and determine the location of the convergence zone over slope 2. Geometric and dynamical aspects might be additional causes for this flow separation over slope 2 in the HMIN0 simulation (cf. minor comment #1 of referee #2).
46. *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.*
- ⇒ Thank you for your correction. The difference is 0.8 km for HMIN0, and even 0.9 km for HMIN0.5 and HMIN1. Therefore, we changed the given statement to 0.9 km. The comparison of the AL with the CBL2, shows maximum differences of 0.4 km for the various terrain geometries (p. 22, l. 25). We referred to this height difference by mistake.

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.*
- ⇒ We adapted the time stamps in Fig. 3, 5, 6, 8, 10, 11 and 13.
2. *p. 8, l. 22: "... Schmidli (2013) and Wagner et al. (2014a)..."*
- ⇒ We placed the citations in the right order (p. 9, l. 10).
3. *p. 9, l. 16: "... for the top of the daytime ABL over homogeneous and flat terrain."*
- ⇒ We added this information to the manuscript (p. 10, l. 10-11).
4. *p. 10, l. 4: "... using the same Richardson number..." ". As Ri is only used once more in the manuscript I suggest removing the abbreviation.*

- ⇒ We agree and removed the introduction of the abbreviation “Ri”.
5. *p. 10, l. 14: “... of horizontally averaged vertical sensible heat flux, normalized tracer mixing ratios, and ...”*
- ⇒ We added the term “sensible” to the sentence (p. 11, l. 5-7).
6. *p. 10, l. 16: “... vertical sensible heat flux”*
- ⇒ We did not add the term “sensible” to the “vertical heat flux”, as it is already mentioned in the previous sentence and also specified in the caption of Fig. 2.
7. *p. 15, l. 22: “3.3 % km⁻¹”*
- ⇒ We corrected the unit (p. 16, l. 21).
8. *p. 15, l. 27: “... over slope 2 (−12 km < x < −10 km)”*
- ⇒ We specify “over slope 2 (−13 km < x < −11 km)” now (p. 16, l. 26).
9. *p. 16, l. 1: “5.9 % km⁻¹”*
- ⇒ We corrected the unit (p. 16, l. 27).
10. *p. 18, l. 2: “... to the free troposphere...”*
- ⇒ We agree and corrected the expression (p. 19, l. 2).
11. *p. 18, l. 18: “... which extends approximately 500 m higher up to about 3 km than ...”*
- ⇒ We corrected the expression (p. 19, l. 16-17).
12. *p. 18, l. 22: “... is more evenly distributed between...”*
- ⇒ We agree and corrected the expression (p. 19, l. 20).
13. *p. 18, l. 29: “... up to the convergence zone on the second slope ...”*
- ⇒ We rephrased this sentence according to your comment (p. 19, l. 27).
14. *p. 19, l. 3-5: “In contrast to the reference run, the tracer particles in the HMIN0.5 simulation are transported horizontally (Fig. 11b).”*
- ⇒ We moved the reference “(Fig. 11b)” to the end of the sentence (p. 20, l. 3).
15. *p. 19, l. 5: For clarity, it might help to start a new paragraph here.*
- ⇒ We agree and started a new paragraph here.
16. *p. 19, l. 26: “... leads to a rather continuous increase in time ...”*
- ⇒ We added “in time” to this sentence (p. 20, l. 23).
17. *p. 20, l. 6: For clarity, it might help to start a new paragraph here.*

⇒ We agree and started a new paragraph here.

18. *p. 29: "... total vertical sensible heat flux profiles...". In the label of the color-bar: "Vertical sensible heatflux" and please add a blank in " $W m^{-2}$ "*

⇒ We added "sensible" to the caption and adapted Fig. 2.

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

⇒ The caption already states that the variables are averaged in space and time. We added the information that the averaging in space is done along the y-direction.

20. *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 " $m s^{-1}$ ".*

⇒ In this figure we are mainly interested in the evolution of the flow and the development of the different flow regimes. As the cross-mountain wind speeds after 6 h are already shown in Fig. 3 and to avoid too many lines that would degrade the readability of the figure, we decided not to include any additional times in this figure.

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

⇒ We added this information according to your comment.

22. *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 (?).*

⇒ We changed the caption, and the figure according to your comments. At approximately $x = -10$ km, there is no apparent separation in an upper and lower pollution layer. For the detection of the AL this implies that the minimum value of the negative vertical aerosol gradient is located at the top of the return flow (at approximately 2.8 km height). Everywhere else the strongest tracer gradient occurs at the top of the slope wind layer. Therefore, a striking peak exists in the AL at approximately $x = 10$ km. One can see this feature as a weakness of the approach to deduce a single AL height or as a general problem to determine AL heights in case of complex multi-layer distributions.

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

⇒ We adapted Fig. 10 according to your comments.

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

⇒ We adapted Fig. 11 according to your comments.

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

⇒ We adapted Fig. 13 according to your comments.

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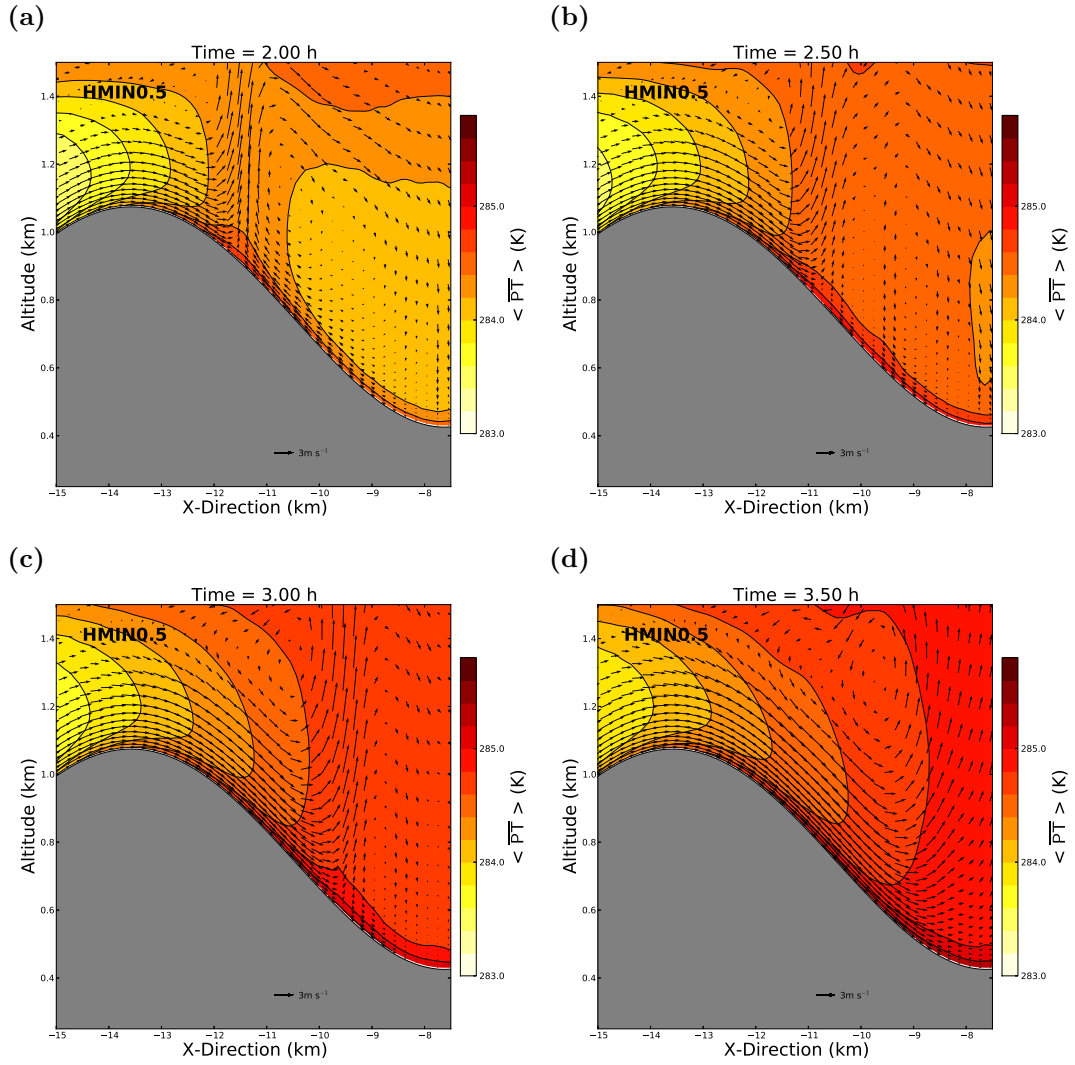


Figure R1: Cross sections of averaged (a–d) potential temperature as contour lines (increments of 0.25 K) after 2.00, 2.50, 3.00, and 3.50 h of simulation for the HMIN0.5 mountain shape, respectively. Wind vectors for components parallel to the cross section. Variables are averaged in time and space (along y-direction).

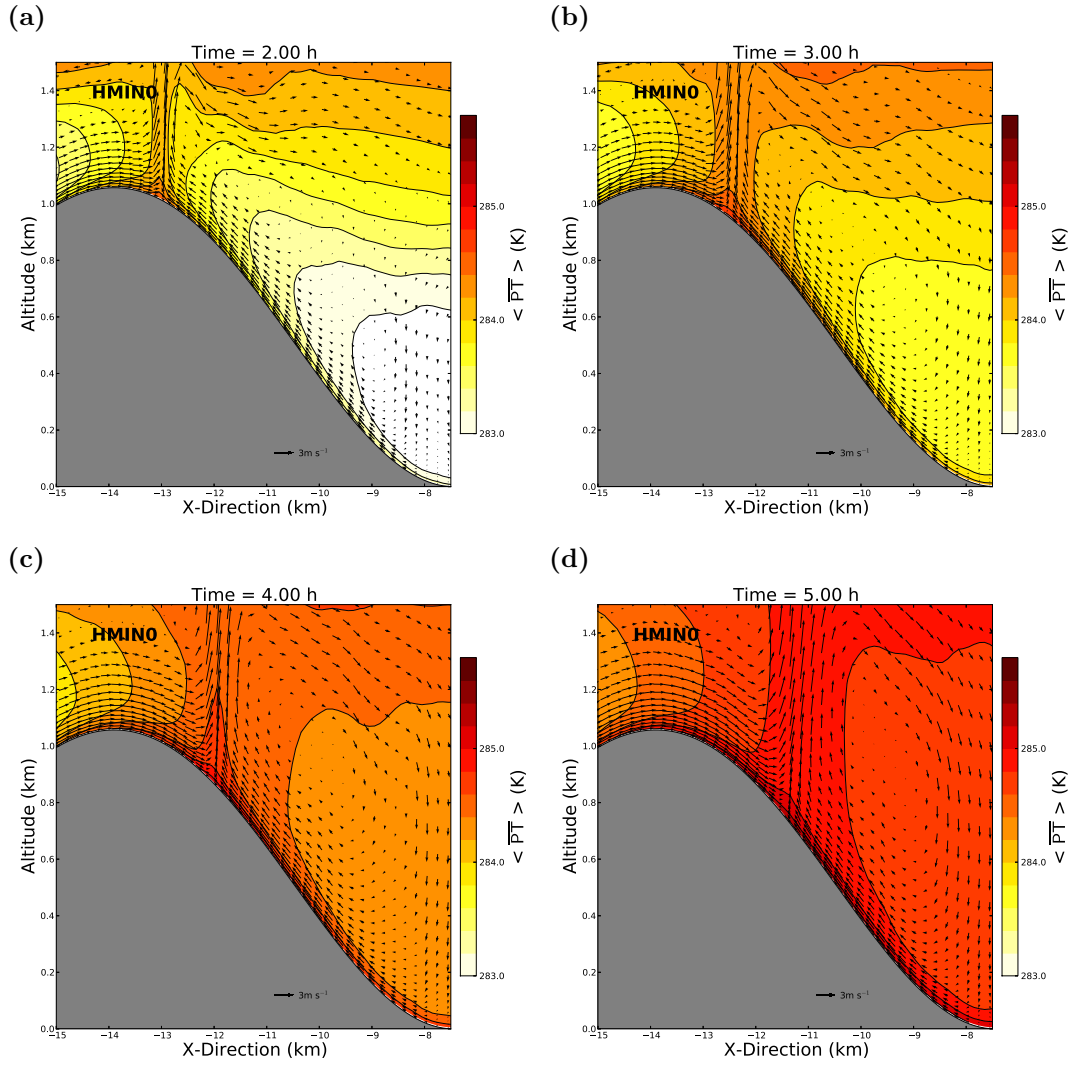


Figure R2: Cross sections of averaged (a–d) potential temperature as contour lines (increments of 0.25 K) after 2.00, 3.00, 4.00, and 5.00 h of simulation for the HMINO mountain shape, respectively. Wind vectors for components parallel to the cross section. Variables are averaged in time and space (along y-direction).