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 #2

2.1 Minor comments

1. *You attribute the flow separation in the deepest valley configuration to the upslope thermally driven wind (e.g., discussions on p. 14327, l. 5, and p. 14327, l. 18-20). I think it is quite possible that you would find flow separation for this valley geometry regardless of the slope wind, and instead owing to mechanical considerations. In fact from looking at the figure the air at the crest of the first ridge appears to be either potentially equal to or potentially cooler than the air in the valley atmosphere. This suggests that the proposed reasoning for the flow not reaching the valley floor may be, at least in part, incorrect. As the dynamics of flow separation are not at the core of this study this is a relatively minor issue, but might warrant addition examination or at least a broadened literature discussion on the topic. For example, you might include discussion of the non-dimensional valley depth and some historical or recent work examining this issue of whether a given stratification and wind will ventilate a valley. I've included a sample of a few potential references at the end of this document. To truly examine the source of the flow separation (which leads to a different dispersion regime) you might conduct an additional simulation by imposing a background flow comparable to the plain-to-mountain wind strength, but without the surface heat flux forcing. Does the flow still separate? If such a simulation would be time or resource prohibitive I think a discussion of the topic would suffice to contextualize the results.*

⇒ We agree with your comments. We have added figures below that show a close-up of the flow over the valley slope and which partly confirm your arguments (cf. Fig. R1 and R2). In the reference run, the advected air at the crest over the first ridge has the same potential temperature than the air in the valley. Due to the deeper valley in HMIN0 compared to the elevated valleys in HMIN0.5 and HMIN1, a more distinctive upslope circulation can be established over slope 2. In our opinion both facts (nearly identical potential temperature and stronger upslope flow in the valley in HMIN0) mainly prevent the plain-to-mountain wind to descend to the valley floor during the entire simulation. Additionally, we discuss geometric and dynamical aspects and refer to a selection of your suggested references. Please see also the answers to your specific comments #14 and #15 below.

2.2 Specific comments

1. *p. 14319, l. 6: Perhaps change "daytime" conditions to pseudo-daytime conditions with constant surface sensible heat flux.*

⇒ We changed "daytime" to "idealized daytime conditions with constant surface sensible heat flux" (p. 5, l. 9-10).

2. *p. 14320, l. 9-10: Is there a reference for the stats module? Is this something your group has created? Publically available? Does it handle terrain following coordinates?*

⇒ The stats module was developed by Johannes Wagner during his PhD thesis. The stats module is unfortunately not publicly available. It can handle terrain following coordinates. In the description of the methodology, we refer to Wagner et al. (2014a) who describe the online flow decomposition in more detail.

3. *p. 14320, l. 14-15: Please note that the use of constant sensible heat flux and no moisture flux is a major simplification for PBL development in complex terrain, especially since the Bowen ratio can vary significantly with elevation and from one portion of a slope to another.*

⇒ We adapted the relevant paragraph (also according to the specific comment #9 of referee #1.)

4. *p. 14320, l. 18: I'm confused by this sentence, are you saying that if you were to average the first 6 hours of a sinusoid with an amplitude of 235 that you'd get* 150 $W\,m^2$*? Note non-arid valleys suggests that there would be a substantive latent flux involved, which is set to zero here, which is fine, you might just need to make the limitation more clear in the summary.*

⇒ This is exactly what we meant. If one integrates the first 6 hours of a 12 h sinusoidal heating with amplitude of approximately $235\,\mathrm{W\,m^2}$, one would get a mean forcing of $150\,\mathrm{W\,m^2}$. We have tried to improve the sentence and have declared the limitations of the set up more clearly (p. 6, l. 26-28).

5. *p. 14320, l. 23-24: Neglecting Coriolis effects: for large plain-to-mountain wind systems Coriolis can be important. Perhaps note this simplification.*

⇒ We agree and noted this simplification. We also estimated and mentioned the Rossby number for the larger scale circulation (p. 7, l. 6-8).

6. *p. 14322, l. 4: "during daytime" perhaps change to, "during the simulation" as there really is not a diurnally varying forcing in these simulations.*

⇒ We agree and specified "during the simulation" (p. 8, l. 18).

7. *p. 14324, l. 5: It might be nice to include the Ri bulk formula used here since your results are sensitive to the method applied.*

⇒ As the Richardson bulk formula according to Vogelezang and Holtslag (1996) is quite complicated, we decided only to refer to the more detailed description in De Wekker (2002). There it is explained in the appendix over a whole page.

8. *p. 14325, l. 7: I think this should read "wind is established" not "wind establishes"*

⇒ We agree and changed the expression (p. 11, l. 24).

9. *p. 14325, l. 11: I'm confused by why you distinguish between ABL and CBL here.*

⇒ According to the minor comment #1 of referee #1, we removed the term ABL completely from the manuscript.

10. *p. 14325: How does your definition of ML compare with CBL? Why the additional term?*

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

11. *p. 14325, l. 17: Would it be useful to define depth versus height? Height I assume is not a terrain sensitive quantity, but simply refers to the height of some variable (e.g. the EL). Where as depth is locally defined as height above the ground?*

⇒ The CBL depth refers to the CBL height minus the terrain height. To prevent misunderstanding we added this explanation as a footnote to the manuscript (p. 12).

12. *p. 14325, l. 23: "similar" to what, and I only see "cross mountain" winds in one location, namely above the ridge at -15 km.*

⇒ We removed the term "similar" from the manuscript. "Cross-mountain" refers to the wind component parallel to the cross-mountain section (p. 12, l. 14).

13. *p. 14326, l. 15: Is this no longer considered a plain-to-mountain wind? What separates the distinction of the plain to mountain wind versus the slope wind systems in the S-RIDGE simulations?*

⇒ We now state "an upslope wind layer superposed by the plain-to-mountain wind" (p. 13, l. 5).

14. *p. 14327, l. 5: (See main comment above) It might be good to add a line here indicating that flow separation versus flushing of valleys may not be due only to the thermally driven upslope flow convergence, but due to geometric and dynamical considerations. In other words, if you simply imposed a background wind on the topographic configuration it is possible that you'd get lee slope separation and reversed flow even for neutral stratification. The result might resemble a thermally driven upslope wind. Likely both processes contribute in these simulations, but you should make some mention of the dynamically forced component of this flow.*

⇒ We agree and thank you for your thoughtful comment. We changed this paragraph (which we moved according to specific comment #28 of referee #1 to Sect. 5.2) and address the geometric and dynamical considerations now.

15. *p. 14327, l. 13-15: I don't see evidence in the figures that the cross barrier cold advection undercuts the upslope flow. Specifically I don't see a flow reversible with height in either Fig. 5a, or Fig. 5b. If there is clear evidence of this at, say $T = 3.0$ h? Please include an additional panel showing as much, or state "not shown"*

⇒ Close-up figures of the lee slope region confirm your comment (see Fig. R1 in the end of this document). We believe that due to a weakening of the upslope wind over slope 2, the convergence zone is continuously shifted towards the valley floor. Eventually the downslope flow replaces local slope wind circulation after 4 h of simulation. We modified the explanations in the manuscript accordingly (p. 14).

References

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Vogelezang, D. H. P. and Holtslag, A. A. M.: Evaluation and model impacts of alternative boundary-layer height formulations, Bound.-Lay. Meteorol., 81, 245–269, doi:10.1007/BF02430331, 1996.

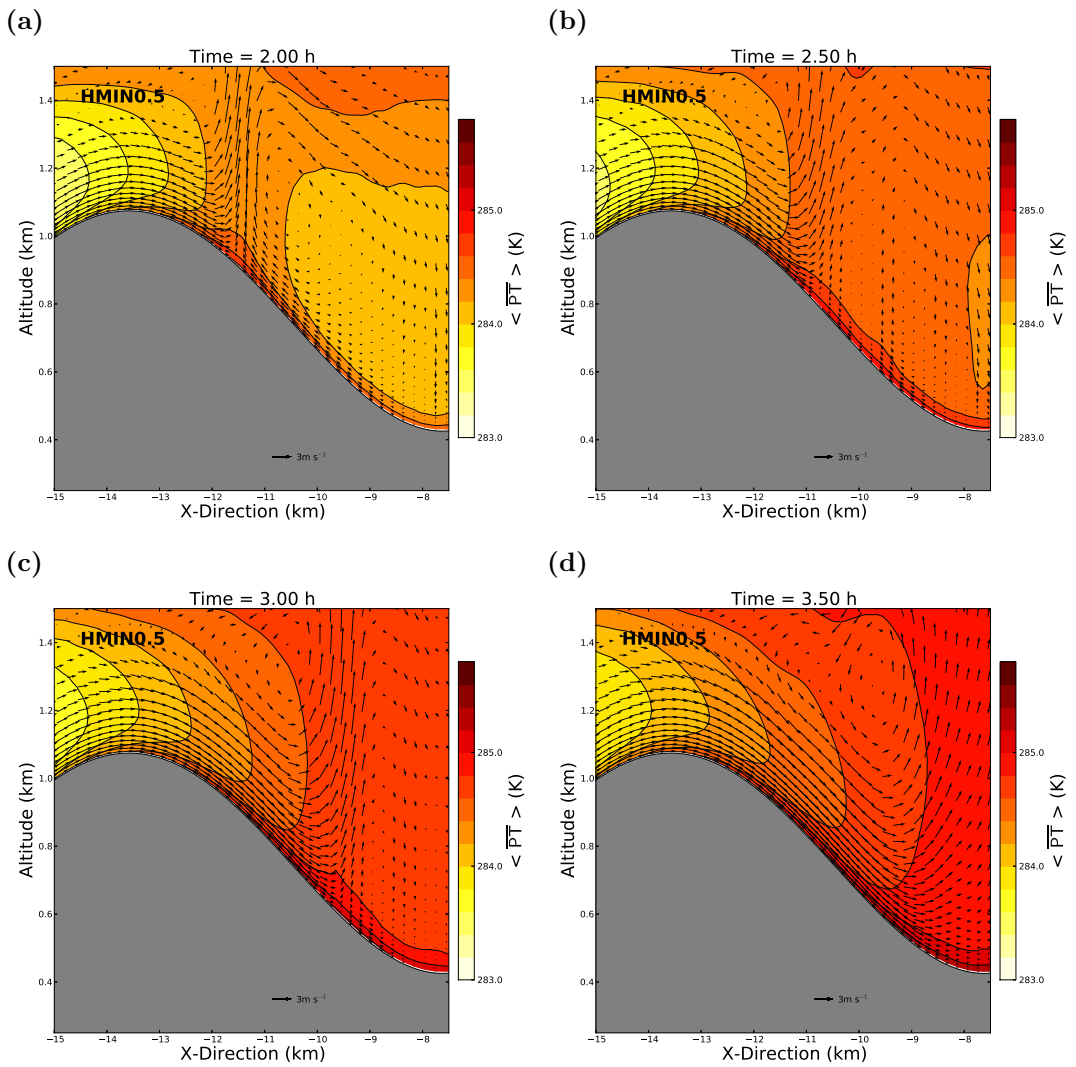

Figure R1: Cross sections of averaged (a–d) potential temperature as contour lines (increments of 0.25 K) after 2.00, 2.50, 3.00, and 3.50 h of simulation for the HMIN0.5 mountain shape, respectively. Wind vectors for components parallel to the cross section. Variables are averaged in time and space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

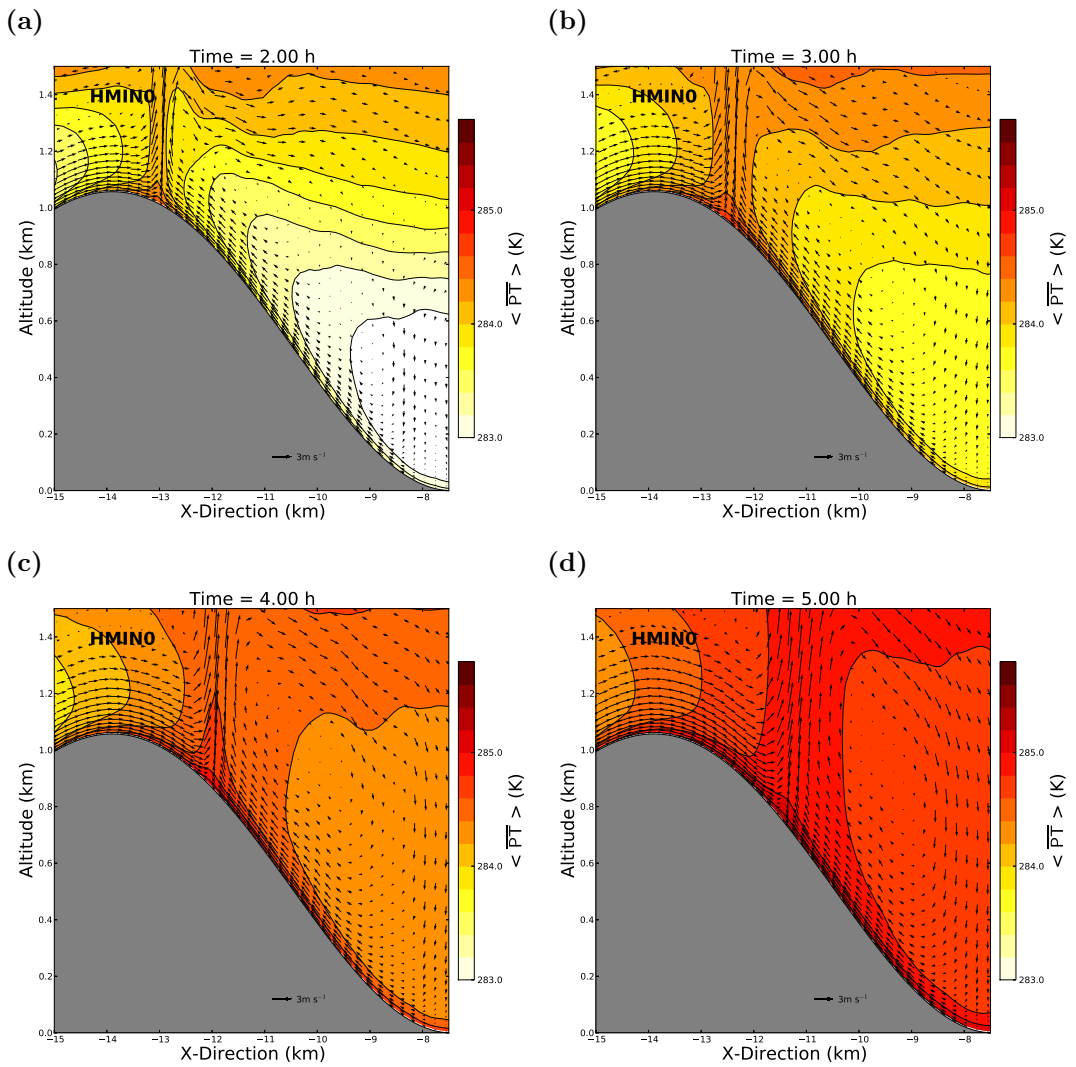

Figure R2: Cross sections of averaged (a–d) potential temperature as contour lines (increments of 0.25 K) after 2.00, 3.00, 4.00, and 5.00 h of simulation for the HMIN0 mountain shape, respectively. Wind vectors for components parallel to the cross section. Variables are averaged in time and space (along y-direction).