Author's response to 3rd round of reviewer comments

A Lagrangian framework for detecting and characterizing the descent of foehn from Alpine to local scales
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We appreciate the reviewer's effort and thorough examination of our additional analyses provided in our last response. However, we have chosen not to incorporate most of the suggestions into the manuscript. In our view, the first comment extends beyond the scope of the manuscript, and we partly disagree with the interpretations in both comments. In an effort to bridge the gap, and as suggested by the reviewer, we have expanded the results section to elucidate some key features of hydraulic flow observed during foehn in our two case studies. Furthermore, we have further highlighted the potential significance of the hydraulic theory in our discussion and conclusions. We hope that this compromise reflects our commitment to addressing the reviewer's last concerns. For further clarification, please refer to the detailed explanations provided below (our answers in blue; the line numbers refer to the second revised version of the manuscript).

Major comments:

1. **Influence of up/downstream air mass difference on descent - up/downstream profiles:** I agree that it is difficult to determine a suitable upstream location for studying the influence of potential temperature differences on foehn descent within the complex topography of the Alps. The Sierra Nevada topography studied by Mayr and Armi (2010) is much simpler. The figures R1 and R2 produced for the response confirm this. I disagree with the authors on parts of their interpretation and urge to choose different upstream and downstream locations for the vertical potential temperature profiles.
    a. **Interpretation:** I attach an annotated version of the authors' Fig. R1 (Fig. A1-R1) and added a crucial missing part: the lowest elevation over which foehn can actually flow (estimated to be around 2.2 km). Contrary to what the response states, it becomes clear that the upstream air can only descend a few hundred meters at the first time shown (Fig. A1-R1a). The next time step (Fig. A1-R1b) confirms that upstream air needs to be colder for descent to the downstream bottom. The subsequent time steps (c-e) point to a problem with the choice of the upstream and downstream locations. I marked "cap" to show the top of downstream foehn layers. They are mostly well below the lowest possible crossing elevation and indicate that the upstream had been significantly modified by the many ups and downs between Milano and Vaduz as Fig. 3 of the manuscript shows. Milano-Vaduz is actually one of the longest distances where foehn descent is modeled..
    b. **Profile locations:** Therefore I suggest redoing the figures for a downstream location within the subregion d2, where the majority of trajectories descend much closer to the Alpine crest (Fig. 3 of manuscript) and use the computed trajectories to place the upstream location about 20 km upstream of the Alpine crest. Then add and discuss the figure in the main text of the manuscript.

We thank the reviewer for his thorough consideration of our additional analyses provided in the previous response. However, we have decided not to translate the suggestions into specific changes in the manuscript and provide several reasons for this decision:

- In our last response, we presented the vertical profiles of potential temperature between Milano (upstream) and Vaduz (downstream). Based on these figures, we concluded that the cross-Alpine temperature differences only partially correlate with the temporal variability of the descent and appear to be neither a necessary nor a sufficient condition for descent to occur. The reviewer suggests that this conclusion may be influenced by our choice of upstream and downstream locations. This is surprising considering that Milano is an upstream location commonly used in many Alpine foehn studies to infer the upstream profile (e.g., Mayr and Armi, 2008; Würsch and Sprenger, 2015; Tian et al., 2024). To circumvent this challenge, the reviewer suggests that we focus on other upstream and downstream locations. However, such an approach is simply outside the scope of our manuscript, as our case studies specifically focus on a descent hotspot in the Rhine Valley and not in a valley further west. We believe that an analysis involving additional descent hotspots in other regions could be a valuable aspect of future research, as already mentioned in the manuscript's limitation section (L. 593ff) and in the conclusions (L. 662-663).
- As mentioned above, the reviewer acknowledges the significant challenge in selecting a suitable upstream location for foehn studies in the Alps. Using the online trajectories could help to deduce the regions from which air parcels originate. However, recent Lagrangian foehn studies have clearly highlighted that the upstream source regions of foehn air parcels exhibit substantial variability (e.g., Jansing and Sprenger., 2022; Jansing et al., 2022). Merely averaging the upstream positions of all air parcel trajectories would not provide a representative upstream profile, particularly considering that the origin of foehn air parcels also evolves over time (e.g., Jansing and Sprenger, 2022).
- The reviewer argues that the descent, as inferred from the potential temperature difference of the two profiles, appears to be limited to a few hundred meters at the first times shown (Figs. R1a, R2a). Our previous response does not deny this. However, if the temperature differences were the only decisive factor, we would still expect to detect some descending trajectories, as we do not prescribe the trajectories to reach the downstream bottom directly. It is plausible that none of the air parcels descend more than 500 m during these early stages of the two events, potentially falling below our detection thresholds. Nevertheless, this scenario seems unlikely given the typical undershooting of downward-accelerated air, as described in the reviewer's second comment.

In conclusion, we find that the proposed analysis framework, which compares two profiles, is too simplified for the complex, three-dimensional orography of the Alpine range. Our experimentation with this approach yielded inconclusive results with respect to the significance of the cross-Alpine potential temperature differences. Moreover, any additional analyses of other regions and with more elaborate methodology would go beyond the scope of our manuscript. Consequently, we have opted against incorporating any of these analyses into the manuscript.

2. **Hydraulic response and virtual topography:** The carefully prepared Figs. R3 and R4 (even with an inset of the cross-section location!) are a big help! Thanks! However, I disagree with their interpretation.
    a. **Descent limited to difference in potential temperature:** These figures actually show that the upstream air can only descend approximately as far as its level of neutral buoyancy on the downstream side! I marked up Fig. R3c-h and attach it as Fig. A2-R3. The obstacle I refer to In the following is between km 30 and 40 in the figure. In the first cross section (left column, Fig. A2-R3c,e,g), the 292 K isentrope marked up in orange rises upstream as time progresses, i.e. the upstream air becomes colder. Between 5 UTC and 9 UTC the upstream air becomes still colder whereas the air downstream warms as is visible by the sinking of the 290 K and 288 K isentropes close to the next obstacle. In the final time step shown at 09 UTC, the 292 K isentrope has risen just far enough to cross the obstacle and can descend fairly far downstream with accompanying higher flow speeds. In the second cross section (second column, Figs. A2-R3d,f,h), a similar upstream cooling is shown by the rise of the 294 K isentrope (marked up in orange) and needed before the air can descend further downstream. The momentum of the descent is sufficiently large to *under*shoot the level of its neutral buoyancy and thus deform the isentrope downwards closest to the slope (which is reminiscent of positively buoyant air parcels in convection *over*shooting their level of neutral buoyancy).
    b. **Waves are a response to virtual topography:** Fig. A2-R3 actually disentangles the seeming chicken-and-egg question of whether (gravity) waves modify the virtual topography or whether the waves are triggered by the virtual topography. A black outline in the second cross section of Fig. A2-R3 shows the region where the answer becomes obvious. There is no wave above the actual real topography. The wave is in response to the descending flow and the shape of the virtual topography that it causes. The wavelength corresponds to the virtual topography (an inverted peak) downstream of the real peak. For a further discussion, see Armi and Mayr (2011, section 4c) and Armi and Mayr (2015, section 3b first paragraph).
    c. **Key features of hydraulic flow**: Focusing just on the last time step and the region between km 25 and 45 in Fig. A2-R3(g, h), one finds these key features of hydraulic flow: (i) descent of the overflowing layer already ahead of the hydraulic control location (= peak), (ii) asymmetric flow across the obstacle with accelerating flow on the downstream side, (iii) a rebound in a hydraulic jump to the conditions further downstream, and (iv) a less stably stratified and slow layer on the downstream side (between 298 K and 300 K) separating the foehn flow from the flow aloft.

Parts of Figs. R3 (or R4) should also be included in the final manuscript to discuss this topic.

We again thank the reviewer for his detailed consideration of the vertical cross sections presented in our previous response. We provide specific answers to each of the three comments:

a. We agree that the upstream air tends to cool as seen by rising isentropes in Fig. A2-R3. However, we disagree with the reviewer’s interpretation of the vertical cross sections. In fact, descent is already visible in Figs. A2-R3d,f *before* the 294 K isentrope reached the obstacle’s height. Furthermore, upon considering, for instance, the 298 K isentrope in Fig. A2-R3f, it becomes apparent that a small across-ridge potential temperature gradient corresponds to a considerable deformation of the isentrope downstream of the obstacle. This can hardly be attributed to the undershooting effect alone. Thus, it is plausible that the descent is not solely influenced by the across-ridge difference in potential temperature, but also by the emerging gravity wave! The same rationale similarly applies to Fig. R4 provided in the previous response, with which we argued for a more active role of gravity waves in the descent process.

b. As already mentioned in the previous response, this analysis does, in our opinion, *not* solve the chicken-and-egg question. The wave is anchored to the real topography and emerges *simultaneously* with the first deformation in the virtual topography (Fig. A2-R3d). While the reviewer suggests that "*The wave is in response to the descending flow and the shape of the virtual topography that it causes*", one could just as plausibly argue that *"the descending flow and the shape of the virtual topography are in response to the wave that formed"*. The qualitative examination does not allow this question to conclusively be addressed. In fact, the second set of vertical cross sections in the previous response (Fig. R4) also points towards an active role of gravity waves in influencing the descent of foehn! As already outlined in the manuscript (L. 546ff) and our prior response, a more elaborated framework would be necessary to draw general conclusions regarding the driving mechanisms.

c. We concur that some key features reminiscent of hydraulic flow are discernible in the vertical cross sections in Fig. A2-R3, but also in the manuscript (Figs. 10a,b). To adequately address this observation, we have incorporated a description of these key features in the revised manuscript (L. 398ff). Furthermore, we have further emphasized the potential significance of the hydraulic theory to explain the descent in the discussion (L. 530ff). Finally, we also added a similar statement to our conclusions (L. 625ff).

In summary, while we acknowledge certain points raised by the reviewer, we find ourselves in partial disagreement. Our analysis does, in our opinion, not confirm the hypothesis that the descent must solely be attributed to the differences in potential temperature, and that gravity waves merely form in response to the descent and the shape of the virtual topography. The qualitative nature of our analysis does not allow us to draw clear conclusions with respect to the driving mechanism. We have already taken this caveat of our study into account by adopting the scope of our manuscript. For example, we changed the title of our study, adopted the abstract and included a comprehensive limitation section, as detailed in our previous responses. Furthermore, beyond the limitations of our study, a variety of publications emphasizes the active role of gravity waves in foehn flows (e.g., Zängl et al. 2004a; Zängl et al. 2004b; Drobinksi et al. 2007; Saigger and Gohm, 2022). Giving the existing disagreement within the literature regarding the driving mechanism, we refrain from making definitive statements in either direction, as our analysis does not yield a conclusive answer. We trust the reviewer will accept our diverting view on this matter.

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