Final response to reviewers

On the descent of the Alpine south foehn
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General statement:

We would like to acknowledge both reviewers for taking the time to reviewing our manuscript. We will consider the valuable feedback and address the criticized aspects in a revised version of the manuscript and we are therefore confident that the revision will result in a significantly improved manuscript. In this document, we respond to each of the reviewers' comments and outline how we intend to address them (original reviewer comments in black, our answers in blue). We also highlight the specific changes we plan to make to the manuscript.

Reviewer 1:

We appreciate the reviewer's evaluation of our manuscript and we take note of the strong criticism expressed by the reviewer. We concur that our manuscript overemphasizes the gravity wave mechanism, while wrongfully neglecting the hydraulic mechanism. We would like to stress that, with this study, we did not intend to provide a definitive answer to the underlying physical mechanisms leading to the descent. Instead, our primary objective was to introduce a novel Lagrangian framework that offers the possibility to investigate the descent using a mesoscale NWP model. We believe that this aspect was not emphasized enough and therefore also not sufficiently appreciated by the reviewer. Furthermore, we disagree with some of the reviewer's statements. Nevertheless, we are committed to addressing the concerns by making appropriate changes to the manuscript. For more details, please refer to the specific responses to all three points below.

Descent of air shapes gravity waves, not the other way around: The descent of the air behind topography makes it possible for large gravity waves to form, rather than gravity waves causing the descent as the paper argues. When air impinges on an obstacle in stably stratified air, a gravity wave will be launched. Its shape and amplitude, however, will not only depend on the non-dimensional height of the obstacle (the product of relative height above incoming isentrope, stability and the inverse of speed), which is a function of the *upstream flow only,* but also on the shape of the obstacle *including its lee side*.

Downstream descent is limited by real or virtual topography. Virtual topography is formed by the level of neutral buoyancy of the overflowing air with respect to the pre-existing air on the downwind side. Descent is only possible when the level of neutral buoyancy of the overflowing air in the downstream air is lower than the upstream altitude of the air, i.e. when the potential temperature of the upstream air is lower than the pre-existing downstream air at the same altitude. Armi & Mayr (2015) showed this in detail with observations from instrumented aircraft (in-situ and cloud radar), dropsondes, radiosondes and satellite data.

The present paper even acknowledges the fact that gravity waves depend on how far air can descend (and not vice versa) in lines 568-569 of the conclusions by stating that “nocturnal cooling and the resulting formation of a cold-air pool impede strong descent of foehn air by effectively attenuating the mountain waves” without concluding that the descent cannot go further than to where this isentrope is located on the downstream side.

The third row of Fig. 10 (09 UTC 28 Feb) shows minimal gravity wave activity because the underlying virtual topography produced by nocturnal cooling is so smooth. The previous and the subsequent afternoons (13 UTC) of rows 1 and 4, respectively, have warmer leeward temperatures caused by daytime warming mainly by sensible heat fluxes (and initially by the turbulent erosion of the cold pool). Consequently gravity waves are larger.

Many thanks for this detailed discussion of the effect of stratification and virtual topography on the descent of air parcels in the lee of mountain ranges. We partly agree with this comment, especially regarding the controlling factors that affect the shape and amplitude of the gravity waves excited by mountain peaks. We also agree with the statement that air in the lee will descend until it reaches the level of neutral buoyancy (under the assumption of adiabatic, stably stratified flow). However, we also disagree with several aspects raised by the reviewer:

- Based on the reviewer’s statement, the virtual topography seems to be the most important factor for the characteristics of the resulting gravity wave, which we do not agree with. We do not think that gravity wave formation, e.g., along the Rätikon, is only controlled by the stratification and virtual topography to the lee of the mountain, but also by the upstream flow conditions (upstream stratification and upstream wind speed) and the shape of the obstacle (as mentioned by the reviewer at the beginning of his statement).
- We are convinced that gravity waves play a more active role in the descent than the reviewer’s comment suggests. Several studies have shown that descending flow and locally enhanced foehn winds are associated with gravity waves forming in the lee of local orographic features. Relevant for our target region are, e.g., the studies by Drobinksi et al. (2003, 2007), Gohm et al. (2004), Zängl and Hornsteiner (2007), Zängl et al. (2004a,b). A more active role was also recently attributed to gravity waves in the west foehn in the Inn Valley (Saigger and Gohm, 2022). They state that the penetration of the westerly flow into the Inn Valley is partly controlled by the formation (or absence) of a gravity wave at the western boundary of the valley. All the aforementioned studies show that an ‘active’ role can and (partly) must be attributed to gravity waves in controlling and/or modulating the descent and the near-surface winds during foehn events. Of course, we acknowledge that stratification and virtual topography can be important controlling factors, as explicitly shown in the studies provided by the reviewer. However, we argue against such a strict and exclusive role of these two parameters. In addition, the level of neutral buoyancy (LNB), and thus the virtual topography, can also be modulated by the downstream effects of gravity waves, so that a clear separation of the different factors may not be possible.
- In our opinion, descending motion is an intrinsic feature of orographic gravity waves. The descent of air and the associated gravity wave occur and accentuate simultaneously. The reviewer’s statement, in our opinion, overemphasizes the role of the virtual topography and neglects the intrinsically coupled nature of the two phenomena. Is it the gravity wave that shapes the descent of the air, or does the

descent of the air shape the gravity wave? Since these two effects of stratified flow past orography are intrinsically coupled, there is no clear answer, but the reviewer only considers one side.

Nevertheless, the reviewer raises an important point by highlighting that our study is biased towards a gravity wave perspective. In the revised manuscript, we will therefore:

- Emphasize that the descent is an intrinsic feature of gravity waves, rather than claiming a unidirectional causal relationship between these two aspects (e.g., L. 16-17; L. 273-274; L. 321-322; L. 329-330; L. 374-375; L. 405-406; L. 460-461; L. 558-559; L. 595-596).
- Introduce the concept of virtual topography and its potential influence on the descent in the introduction, including the respective references (e.g., in the paragraph spanning L. 66-78).
- Discuss the limitations of the gravity wave perspective and of our manuscript more carefully (in a new limitations section 5.3 – see also next reviewer comment).

Alternative descent mechanism ignored: In the introduction the paper describes two main mechanisms that have been proposed in the literature to explain the descent - gravity waves and hydraulics - differential density (colder air upstream descending to its level of neutral buoyancy downstream). The claim in lines 75 - 77 that the hydraulic approach is applicable to shallow but not deep foehn is not correct (e.g. Armi & Mayr, 2007; Armi & Mayr, 2015; Winters & Armi, 2014).

The present paper only explores the gravity wave mechanism and ignores the hydraulic one despite the rich material available from the numerical simulations. The discussion subsection 5.2 ("What other factors influence the descent?") does not even mention the second mechanism anymore! This is a pity because the study has ample data to test both mechanisms whether they are the reasons for the descent and thus provide more material to further the discussion in the foehn community. The testable characteristic for the hydraulic response is that substantial descent starts when the overflowing air becomes colder (in terms of potential temperature) relative to the downstream air at altitudes below which it descends. Similarly, there need to be testable characteristics for the gravity wave mechanism that avoid, for example, the speculative attribution of the "intermittent periods of weak vertical motion" (lines 450-451) in the April 2018 case study to the "variability of wave-induced subsidence". With the alternative explanation this could be directly tested by examining whether the upstream air had become colder relative to the downstream one. However, the most important aspect to test for the adequacy of the mechanisms is the evolution of the onset of subsidence in the simulations.

The reviewer is correct in stating that we strongly focus on the gravity wave mechanism, but only briefly touch on the hydraulic/density-driven perspective as a potential mechanism for the descent. It was never the goal of the study to provide a comprehensive analysis of the different descent mechanisms. To highlight this more clearly, we will modify the manuscript as follows:

- The title of the study is too general, implying that we are doing such a comprehensive analysis. In the revised version, we will change the title to avoid implying a comprehensive examination of the underlying mechanisms.

- Since we do not provide an analysis of all possible descent mechanisms, we will explicitly mention this limitation of our study in the newly created discussion section (new section 5.3, see also first reviewer comment).
- We understand the reviewer's concern that the hydraulic mechanism was not even mentioned in the discussion section 5.2, and we apologize for this omission. Section 5.2 will be extended in the revised manuscript to include the hydraulic mechanism, including references to the relevant literature (e.g., Armi and Mayr, 2007; Armi and Mayr, 2015).
- We will also modify L. 75-77 to explicitly state that hydraulics have been applied to both shallow *and* deep foehn events in the respective previous studies.

Of course, a comprehensive study of 'all' mechanisms of foehn descent would be most welcome. However, we think that this would go far beyond what we can present in this paper, whose principal goal is to establish a Lagrangian framework for characterizing foehn descent. Nevertheless, we have started such an analysis (Fig. R1), taking into account several potential factors influencing the descent along the Rätikon, namely:

- the low-level stratification in the valley, assuming that a weakly stratified valley atmosphere favors the descent along the Rätikon.
- the wind speed upstream of the Rätikon, assuming that a strong upstream flow promotes the descent.
- the flow-splitting upstream of the considered Rhine Valley section, assuming the mass flux into the Rhine valley affects the foehn descent along the Rätikon by reasons of continuity.
- the maximum height difference of the 3-km isentrope above the Falknis peak within the hotspot region, assuming that a large height difference corresponds to

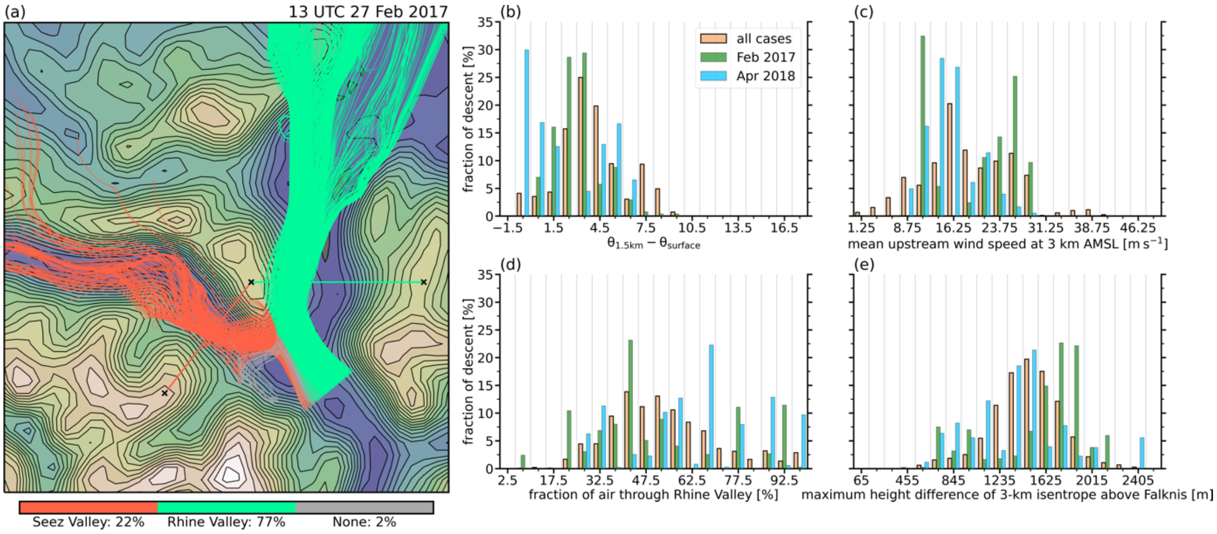

Figure R1. *Overview of the preliminary analysis of different controlling factors on the descent. (a) An example time step of forward trajectories starting along a line near Bad Ragaz (upstream of Rätikon hotspot). These forward trajectories were used to determine to what extent the air is directed westward into the Seez Valley, and to what extent it is directed into the Rhine Valley. (b) The fraction of descent (i.e., the number of descent segments relative to all descent segments in the Rätikon hotspot) binned according to the potential temperature difference between 1.5 km AGL and the surface at Vaduz (see location in Fig. 7c in the manuscript). Shown are the relations for the Feb 2017 case study (green), the Apr 2018 case study (blue), and for all simulated cases combined (brown). (c) Same but for the mean upstream wind speed at 3 km AMSL along an upstream line along the Rätikon. (d) Same but for the fraction of air passing through the Rhine Valley. This fraction is calculated by comparing the number of forward trajectories intersecting a cross section perpendicular to the Rhine Valley relative to all forward trajectories (see also panel a). (e) Same but for the maximum height difference of the 3-km isentrope above the Falknis peak within the hotspot region (see location in Fig. 7c in the manuscript).*

> a strongly inclined isentrope and thus a stronger descent. This could also be considered as a proxy for the maximum height difference of the virtual topography in the target region.

However, the interpretation of these preliminary results proved to be challenging. For instance, descent can occur under relatively stable stratification in the Rhine Valley (Fig. R1b) and under strongly varying flow splitting regimes (Fig. R1d). This calls for a more systematic investigation to disentangle the different factors influencing the descent. In the current manuscript, we refrained from doing so, also in order to not extend the manuscript's already substantial length.

Similar to our preliminary analysis, the reviewer suggests a testable characteristic for the hydraulic response, namely the potential temperature difference of the overflowing air compared to the air below which it descends. However, we think that this testable characteristic would not provide an unambiguous answer with respect to the underlying mechanism. In fact, upstream air that is potentially colder than the downstream air at the same level, and thus descends, could also be associated with a gravity wave. Extending this reasoning, we do not explore the role of the two mechanisms (hydraulic response, gravity wave mechanism), since we lack the testable characteristics to unambiguously disentangle them. We will mention this in our limitation section.

In addition to the physics of the foehn descent, we want to highlight that the study also introduces a sophisticated and, as far as we see, novel method for diagnosing foehn descent. So far, most studies have looked at the foehn descent from a Eulerian perspective, whereas we present a Lagrangian view. We think that this methodological aspect should be well recognized, as it allows for example to diagnose descent time scales. It is a pity that this novelty was not adequately appreciated by the reviewer, which can be attributed to the fact that we put too little focus on it in the text. We will emphasize this methodological aspect more in the revised manuscript, e.g., by referring to it already in the title, but also by specifically addressing it in the abstract and the conclusions of the paper.

Questionable whether numerical simulations correctly reproduce processes that lead to downstream descent: COSMO, the numerical model used for the simulations, has terrain-following coordinates near the surface. The simulation setup also uses horizontal diffusion (lines 179-181). If the numerical diffusion acts along model surfaces, which are slanted in complex terrain, artificial vertical, cross-isentropic mixing will ensue since in general the terrain-following coordinates cross isentropes in a stably stratified atmosphere. As a result, air will not separate from steep downward sloping topography as in reality, but rather descend. There are no figures in the paper to substantiate this claim. However, this inference is supported by the fact that the strongest descent in the model simulations occurs along the steepest slopes behind the tallest mountains (lines 313-315 and Fig. 4b) whereas in reality steeper slopes are likelier to lead to flow separation. Pressure perturbations from a gravity wave alone will not be strong enough to force the flow down by 1500 m in stable stratification.
Observations in the ocean (Knight Inlet; Farmer & Armi, 2001, and references therein) and in the atmosphere (Owens Valley during the T-REX campaign; Mayr & Armi, 2010 see Figs. 5 and 6; Armi & Mayr, 2011) confirm that flow separates along steep slopes before it later

descends when small-scale mixing forms a wedge of nearly stagnant and neutrally stratified air (away from the terrain) that separates the descending flow from the flow aloft.
The detailed observations of the oceanic “foehn” flow into Knight inlet demonstrated the importance of correctly simulating the boundary layer separation. Numerical simulations with an atmospheric model (Afanasyev & Peltier, 2001) and an oceanic model (Cummins, 2000) erroneously produced a large overturning wave soon after the flow started flowing over the sill (the oceanic equivalent of a mountain crest) contrary to what the observations showed. Only when Cummins (2000) modified the topography to force boundary layer separation did the oceanic model correctly simulate the evolution of the flow. The authors will need to show that the model adequately handles flow separation; currently there are no figures in the paper that would allow one to do that. This can be done by first examining the time it takes for the descent along the slopes to become established. If this time is not (much) larger than $2\pi / N$ then the simulation will be incorrect. Second, the evolution of the descent must be visually inspected for congruence with observations of the initial flow separation and the way the wedge of the nearly stagnant and neutrally stratified air is formed that isolates the descending air from the flow above.

The reviewer expresses a fundamental issue: Is the COSMO model, and NWP at the kilometer-scale in general, capable of correctly capturing the descent in the lee of orographic obstacles? In fact, we argue that there are a few good reasons to believe that the essence is reasonably captured:

- The reviewer claims that the horizontal diffusion in the model simulations leads to artificial vertical mixing and thus introduces an unwanted, stronger descent in the lee of local mountain peaks. COSMO does calculate the horizontal diffusion along slanted model surfaces, but the horizontal diffusion along slopes is corrected by orographic flux limiting (see also Doms and Baldauf, 2021). The diffusive fluxes are gradually decreased as the elevation difference between adjacent grid points (i.e., the steepness of the coordinate surfaces) increases. When the elevation difference is greater than 250 m, the horizontal diffusive fluxes are set to zero. Using a grid spacing of 1.1 km, this results in a maximum slope angle of ~13°, above which the fluxes are set to zero. This value is well below the steepest slopes in the model (~30°). Considering this flux limiting, we are optimistic that the artificial vertical mixing should be less of an issue than suggested by the reviewer.
- Overall, our foehn episodes simulated by COSMO agree reasonably well with observations. A comparison between some of the COSMO simulations and station observations is provided in Jansing (2023). While mesoscale NWP simulations of foehn are known to be associated with distinct model biases (e.g., Wilhelm, 2012; Sandner, 2020), the mesoscale forcing is often adequately represented (e.g., Umek et al., 2021). However, if the COSMO model would not be able to capture the essential mechanisms of foehn descent, we would also not expect the model to reproduce typical foehn characteristics at valley stations (e.g., temperature increase upon foehn onset). Of course, there are also foehn episodes that are not represented well in the simulations, indicating that some mechanisms are still missed by the model. As an example, the representation of cold-air pools prior to foehn onset in mesoscale NWP simulations is still challenging (Umek et al., 2021).

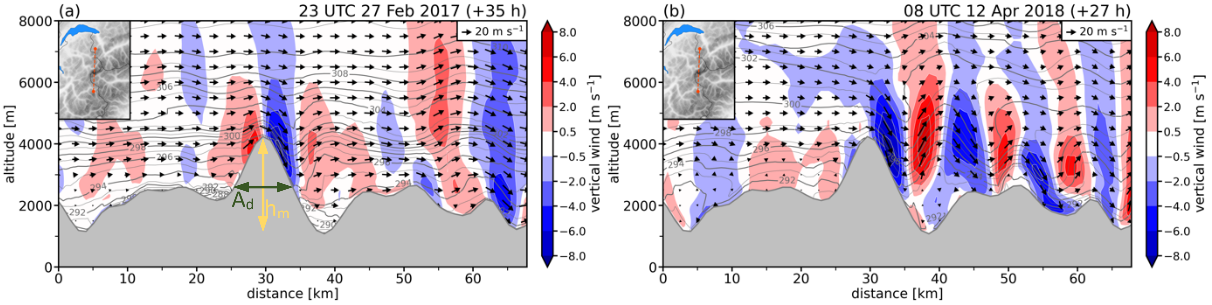

Figure R2. *Vertical cross sections of vertical wind (colormap), isentropes (gray contours) and wind along cross sections (vectors). The topography is indicated by gray shading. The map inset shows the location of the vertical cross section (red, dashed line). The yellow and the green arrows indicate estimations of the downstream half-width (A_d) and the maximum height of the peak (h_m).*

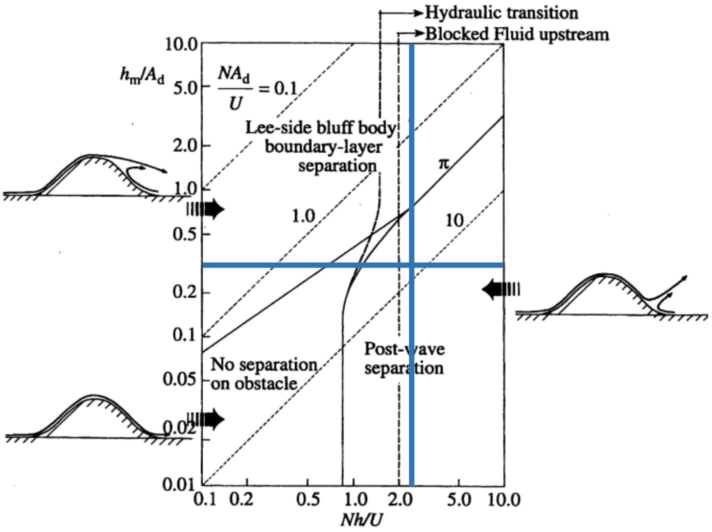

Figure R3. *Regime diagram of different leeside flow responses as a function of the non-dimensional mountain height (N*h/U) and the leeside slope of the obstacle (h_m/A_d). Reproduction of Figure 5.8 from Baines (1995), figure copied from Ambaum and Marshall (2005). The blue lines indicate the values estimated from the vertical cross section in Fig. R2a.*

Another critical comment from the reviewer concerns the representation of flow separation in the model. The reviewer suggests that the time it takes for the descent to become established along the slopes should be checked. However, inserting a typical value for N (~0.02 s^{-1}) leaves us with $2 * \pi / N \approx 5$ min, which is well below the output frequency of the 3D fields for our simulations. Such an approach is therefore not feasible for us.

Following Baines (1995), leeside flow separation does especially occur along steep slopes and for low values of the non-dimensional mountain height ($N * h_m/U$). In Fig. R2., we show two vertical cross sections for the Feb 2017 and the Apr 2018 events going through Mont Blanc, which corresponds to the highest peak of the Alps that is associated with a steep northern slope. Following the regime diagram (Fig. R3) of Baines (1995), and inserting numbers roughly estimated from the vertical cross sections (see also yellow and green arrows in Fig. R2a) to calculate the leeside flow response, yields:

$h_m / A_d = 3000 \text{ m} / 8 \text{ km} = 0.375$
$N * h_m / U = 0.02 \text{ s}^{-1} * 3000 \text{ m} / 25 \text{ ms}^{-1} = 2.4$

The estimated values suggest that the flow either should not separate on the obstacle, or that post-wave separation should occur (see blue lines in Fig. R3). This fits well with the observed pattern along the two vertical cross sections (Fig. R2, but also for other times of these two events) and contradicts the reviewer's statement that we should expect flow separation. Of course, it is not guaranteed that the flow response will be exactly the same in reality, as the model topography is smoothed and wind speed and stratification may be biased in the simulations. In conclusion, the flow response in the model is consistent with the expectations from theory, but it is unclear how well this matches the real flow response in the very rough and complex terrain of the Alps. We will therefore emphasize this last point as a limitation of our study (see also issue 3 raised by reviewer 2 on potential model problems).

Reviewer 2

We want to express our thanks to the reviewer for thoroughly reading our manuscript and for the positive evaluation. The reviewer's comments and input will help us to improve the manuscript significantly. We provide answers to all of his major and minor issues below.

Major issue

1. In the abstract the authors claim to investigate the descent process "with unprecedented detail". Indeed their study identifies foehn descent in a more spatio-temporally extensive dataset then previously, but their is no detailed analysis of the physical processes resulting in the downward motion of the air parcels. Discussion of the physical processes is limited to inference from a few cross-sections for two case-studies. Given the simulation data that they have, it would be very interesting to try and quantify the causes of downward acceleration of air parcels (buoyancy, vertical pressure gradient, …). They allude to this possibility in the conclusion, which is fine and I would encourage to highlight this even more. Indeed the paper would benefit from a more detailed physical analysis, but at least the abstract needs to be modified to accurately represent the contents of the paper.

We fully agree with the reviewer's comment. It is a limitation of our paper that we do not investigate the actual physical mechanisms of the descent in more detail. We will adjust the abstract and also mention this limitation more clearly in a separate discussion section that explicitly mentions all the limitations of our study (see also answers to issues 1 and 2 raised by reviewer 1). We will also further emphasize the potential for future studies investigating the descent mechanisms with our dataset.

2. The section of the introduction starting at p. 4, l.121 is not very well structured and open research questions could be stated more explicitly. Please consider rewriting.

We thank the reviewer for pointing this out, and we will re-write the respective section. In the following, we present some preliminary suggestions for the new structure:

- We will summarize the MAP results more concisely, emphasizing that the descent has so far only been diagnosed qualitatively and for the Rhine and Wipp valleys.
- We will highlight Saigger and Gohm (2022) as a key paper that motivated us to adopt the Lagrangian approach.
- We will highlight more explicitly the open questions related to the descent (e.g., the fundamental properties of descending air parcels).

3. Potential model issues in the representation of descent, e.g. potential issues of the turbulence scheme over complex terrain, need to be discussed in a more structured and prominent location (e.g. section in the conclusion / discussion). Hints at potential modeling problems are found throughout the manuscript, but it is failed to present them in structured manner and a discussion of their potential impacts on the results is missing.

As before, we agree with the reviewer's concern. We will explicitly list the potential model issues in our new limitation section. These potential model problems include:

- The descent is strongly influenced by the terrain characteristics, but the topography is smoothed in the COSMO model. In reality, the descent might thus occur at different locations and with a different magnitude compared to the model simulations. For instance, flow separation might occur at the sharp edges of local mountain peaks, a feature not represented in our simulations as the kilometer-scale grid spacing is still too coarse and the terrain is smoothed (see also issue 3 raised by reviewer 1).
- Turbulence/turbulent exchange is misrepresented in mesoscale NWP models (1D parameterizations that are designed for horizontally homogeneous terrain). This also effects the representation of foehn flows in such simulations (e.g., Vosper et al., 2018).
- Nocturnal CAP formation can inhibit the descent, as has been seen in the second case study (Section 4.2 of the manuscript). However, the maintenance of CAPs is difficult for mesoscale models (e.g., Umek et al., 2021). Therefore, the frequency and magnitude of the descent might be overestimated in our model simulations.
- Gravity wave patterns look different in large-eddy simulations compared to kilometer-scale simulations (Umek et al., 2022). This also suggests that the small-scale features of the descent are not adequately captured with kilometer-scale model simulations.

Minor issues

1. p. 2, l. 42: Why would foehn flows ignite forest fires? I would rather expect they are produce atmospheric conditions, that are more conducive to igniting fires.

Agreed, we will rephrase the sentence.

2. p. 2, l. 60: "foehn wall might inherit a key role for the downward acceleration": I do not understand this sentence: What is inherited and by what?

What we meant was that upon flowing over the crest, hydrometeors in the clouds (i.e., the foehn wall) begin to evaporate, causing latent cooling and thus a downward acceleration of the respective air. We will rephrase the respective sentence.

3. Section 2.1: in addition to the height of the lowest model level, it would be interesting to state the average vertical grid spacing in the valleys, e.g. the lowest 2 km.

Over flat terrain and at a distance from the orography, there are 34 model levels below 2 km, resulting in an average vertical grid spacing of ~60 m. We will add such a statement to Section 2.1.

4. Fig. 3: Would be interesting to see the distribution of foehn trajectories passing the locations of descent. I.e. is the distribution mirroring more frequent foehn events / large mass flux, e.g. along the Rhine valley and what is the percentage of foehn air parcels that descent in the specific regions.

We are not sure what the reviewer is referring to here. The distribution of foehn air parcels descending in specific regions can already be seen in Fig. 3, also when looking at the two histograms along the edges of Fig. 3. A case-by-case overview of the descent locations is found in Fig. S3 in the Supplement.

5. p. 11, l. 274: „gravity waves […] force descending motion of air parcels“: The Lagrangian diagnostic are just another perspective of the Eulerian velocity fields and vice-versa. So it cannot be claimed from the evidence presented that gravity waves force descent of air parcels. Descending air parcels in some sentence constitute the downward motion in the Eulerian perspective. Maybe a better wording instead of „force“ would be „associated“. Similar statements are made e.g. on p. 13 l. 321 and in a few other places, and also need modification.

We fully agree and we will modify all the respective statements (see also issue 1 raised by reviewer 1).

6. p. 12, l. 311: „exact relation“: Given the scatter in the data, I do not agree that this is an exact relation.

Agreed, we will rephrase.

7. p. 16, l. 349: „and especially the cause for its formation“: I would suggest to drop this statement. The following section does not provide any evidence for why a hotspot should form in particular behind the Rätikon and not other topographic features / locations along the Alpine chain.

We will drop this statement.

8. p. 17, l. 366 ff: I would suggest to first discuss the general characteristics of the foehn event before providing details on the time instances discussed afterwards to reduce repetition.

Agreed, we will omit these details here and only give more information later when discussing the time instances.

9. p. 27, l. 548: „constrains“: I am not quite sure what you want to say here. Local terrain determines regions of descent / is anchoring regions of descent?

Yes, this is what we meant. We will rephrase.

10. p. 27. l. 551ff: Given the evidence (in the paper and the more general foehn literature), it would be more accurate to state that the elevation difference is an upper limit to the foehn descent and that (at least) in the model this is often (though not always - maybe you can even quantify how often) realized.

We will rephrase the statement. We are however not able to quantify how often this is the case, as our measure of elevation difference (Δtopo) only provides the local elevation difference, while the slope of the terrain might still extend further. Moreover, our simulation results actually suggest that a considerable fraction of the air parcels descend further compared to the changes in the underlying topography (Fig. 5), implying that they must arrive closer to the ground than where they began their descent.

Technical / language issues

We will adjust all the technical issues mentioned.

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