Author's response to 2nd round of reviewer comments

A Lagrangian framework for detecting and characterizing the descent of foehn from Alpine to local scales
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Submitted to WCD, egusphere-2023-1536
December 15, 2023

We would like to thank the reviewer for reading our manuscript a second time and we are pleased to note that the evaluation was more positive. We genuinely appreciate the effort the reviewer made to come up with many suggestions for improvement! To recognize the effort, we explicitly mention the reviewer in our acknowledgements. Whenever we have considered the comments to be actionable, we also implemented them in the manuscript. More detailed explanations, including specific statements regarding the changes we have made, are found below (our answers in blue; the line numbers refer to the first revised version of the manuscript).

Major comments:

1. **Scope of manuscript:** The new title clearly conveys the focus of the manuscript on a new method for detecting and characterizing descending air behind obstacles. This change should also be reflected in the abstract and throughout the manuscript:
   a. Abstract, second paragraph: Please specify which topographic features favor descent. The sentence "the small-scale elevation differences of the underlying terrain largely determine the magnitude of the descent" contradicts statements in the main article that the level of neutral buoyancy (virtual topography) is decisive, along with gravity waves (although I disagree with the latter).
   b. Line 25: Virtual topography, which per definition applies to the properties of the incoming flow, can be formed by many other mechanisms than nocturnal cooling.
   c. The last paragraph (lines 27++) can be removed as it is a summary of a summary and the parts about multiple case studies and different foehn regions can easily be incorporated earlier.
   d. Most importantly, the abstract should state that the results are based on numerical simulations, which have a considerable degree of uncertainty. As a result, the cause of descent cannot be definitively resolved. Instead, the authors should state that, within the limitations of the model simulations, they have identified characteristics of the descent.

We agree with the reviewer that the focus conveyed by the title should also be reflected in the abstract and the manuscript. We believe that this focus is indeed conveyed throughout the manuscript – our change in title from the first round of revisions only intended to more clearly highlight the key message. Nevertheless, we appreciate the reviewer's additional comments and have tried to incorporate them where we think they improve the manuscript:

a. It is impossible for us to specify particular topographic features that favor descent more clearly than by referencing "local mountain peaks and chains". Looking at Fig. 3 in the manuscript, a large number of mountain peaks and chains along the entire Alpine arc are associated with descending motion, so it is not feasible to make a specific list of these peaks. The statement "the small-scale elevation differences of the underlying terrain largely determine the magnitude of the descent" is inferred from the clear relationship between Δz and Δtopo (Fig. 5 in the manuscript). As gravity waves themselves are strongly tied to the local terrain characteristics, this statement would still be valid if they were the decisive factor for the descent. However, considering the spread in Fig. 5, it is clear that other factors also play a role, such as the virtual topography. Taking all this into account, we rephrased our statement in the abstract, not implying a causal relationship anymore, and we now also refer to the influence by other factors (L. 10-11).

b. We agree that virtual topography can be formed by other processes (e.g., synoptic cold-air advection). However, in our study, we specifically investigated a case where it formed due to nocturnal cooling, so it would be misleading to generalize the statement in the abstract. However, we have rephrased it to make it clear that our statement refers only to the second case study and is not meant in a more general sense (L. 21-22).
c. We would like to retain these concluding remarks. Despite the partly repetitive nature, we consider it important to conclude the abstract with an overarching statement that highlights the significance of our work. Besides, such summarizing remarks also serve as a motivation for readers to delve deeper into our paper and build upon our work for future investigations.
d. We acknowledge the importance of explicitly mentioning this limitation in the abstract. In L. 5 we already state that we employ model simulations. In addition, we now added a sentence to L. 23 in order to clearly convey that, given the model limitations, our study does not intend to definitively resolve the causes for the descent.

2. The **explanation of hydraulic theory** should be expanded to explicitly state that it takes into account the most common south foehn situation in the Alps that air masses on the downstream side of the crest are colder, whether for synoptic, mesoscale or valley-scale (e.g. nocturnal cooling) reasons. Similarly, the one-sentence explanation (lines 92-93) why isentropes descend to the lee in the gravity wave theory needs to be expanded. If isentropes descend because of orographic drag, which (among other factors) depends on the effective height of the obstacle, then a smaller effective mountain due to the virtual topography both upstream (by blocking) and downstream (by cooler air) will cause a smaller deflection and thus make a descent to the floor of the downstream topography unlikely.

We appreciate the suggestion of the reviewer. Based on the comment, we have slightly revised and restructured the paragraph introducing the hydraulic theory to make this aspect clearer (L. 79ff).

Regarding the explanation of gravity waves and descent, we think our current statement in the manuscript is misleading, because it brings us back to the “chicken and egg” discussion: Which comes first, the deflection of isentropes, or the gravity wave – and is there a causal relationship between these two features? Therefore, we rephrased the respective sentence (L. 85-86). We have also added a statement listing several possible influencing factors that determine the amplitude of mountain gravity waves (L. 85-86).

3. **Difficulty of simulations:** The manuscript cites the difficulty of simulations with a 1-km grid to properly handle the interaction of the flow with the cold pool (e.g. Umek et al. 2022). This difficulty is particularly relevant to south foehn in the Alps, where colder air is typically present on the northern side *already from below the crest onwards*, not just further downstream in the lowest parts of the valleys. This difficulty therefore affects the handling of the whole descent process in the numerical simulations, which must be clearly stated in the manuscript.

We want to thank the reviewer for this comment. Picking up the input, we now explicitly mention the fact that air is typically colder already from below crest level onwards on the northern side of the Alps, making it particularly challenging to simulate south foehn events and the descent process. We added such a statement to our limitation section (L. 567ff).

4. **Gravity wave vs. hydraulic explanations** of foehn descent and distinguishing between them: This is an excellent data set, despite the uncertainties of the numerical simulations! it may still be possible to get closer to finding a definitive answer to the question of gravity wave vs. hydraulic explanations of foehn descent. Here are some specific suggestions: First, after foehn air has descended the flow response will be indistinguishable between gravity wave and hydraulic explanation. It is therefore paramount to examine the onset of the foehn descent. I envision several possibilities of doing that:
   a. Examine vertical profiles upstream and downstream of the obstacle from before onset until after the onset, similar to Mayr and Armi (2010).
   b. Alternatively, find regions where foehn has descended as well as similar topographic obstacles behind which foehn has not descended yet. What are the differences? Are the upstream conditions not similar? Note that Reinecke and Durran (1990) found extreme sensitivity to initial conditions in a 70-member ensemble simulating foehn during the TREX campaign. Descent and consequently leeward wind speeds differed despite similar upstream conditions prior to the onset of foehn. This undermines the argument in the manuscript that increased upstream wind speed would favor descent.
   c. Examine vertical sections across the obstacle from before till after onset, and also for regions where foehn does not descend much: Do the wavelengths of the gravity waves correspond to the shape of the virtual topography or to that of the real topography upstream experienced by the impinging flow (to test if your statement that incoming wind speed also plays a role; cf. Mayr and Armi, 2010)?

We acknowledge the reviewer's substantial efforts to provide these additional suggestions for further analysis! We fully agree that our dataset presents a promising opportunity to address this fundamental question: Is the descent occurring due to gravity waves, or can we explain it with hydraulic theory? Or do we need both explanations?

Before elaborating on the specific suggestions, we would like to emphasize the challenge common to all of them, adding to the complexity of drawing overarching conclusions: How to define the upstream conditions? While defining upstream conditions for simple, quasi-2D problems may be relatively straightforward, the Alps pose a 3D problem. The air parcels descending in the lee of the Falknis (refer to Fig. 7 for an overview) already crossed numerous

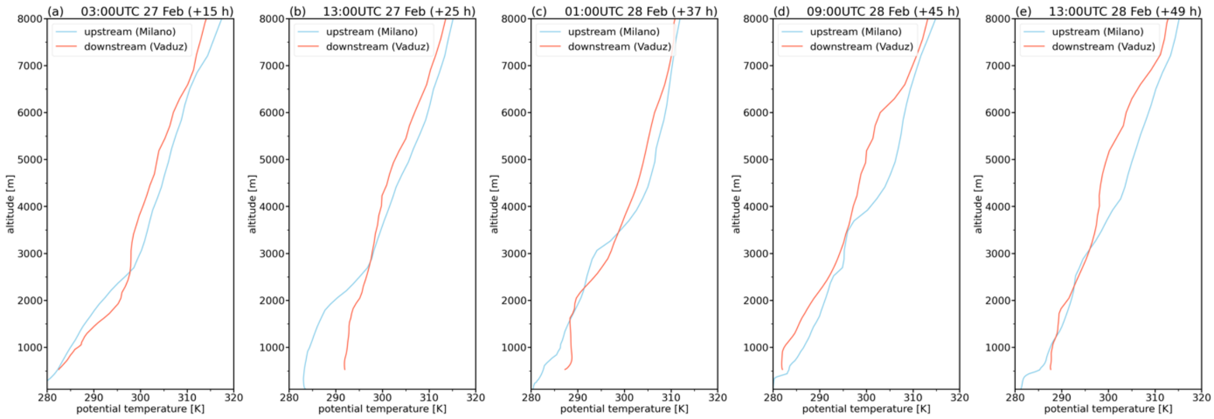

Figure R1. *Vertical profiles of potential temperature upstream (i.e., at Milano) and downstream (i.e., at Vaduz) of the orographic obstacle for the Feb 2017 event. Shown are different time instants.*

mountains and valleys before impinging on the Schesaplana mountain range. Additionally, the Alpine range exhibits a distinctive curvature as well. One could consider the local upstream conditions just a few kilometers to the south, or the mesoscale air mass differences, as demonstrated by Mayr and Armi (2010) for the Sierras. When comparing the vertical profiles, we adopted a similar logic as in Mayr and Armi (2010) and therefore compared the profile of Milano (9.28 °W / 45.43 °N) with that of Vaduz (see location in Fig. 7 in the manuscript).

a. We computed vertical potential temperature profiles both upstream and downstream for the two events, as illustrated in Figs. R1 and R2. Focusing on Feb 2017 (Fig. R1), we see that the upstream air mass is already colder *before* the first air parcels descend along the Rätikon (Fig. R1a; descent timeseries can be found in Fig. 8b in the manuscript). This indicates that the potential temperature difference across the Alps is not the only factor driving the descent. Ten hours later (Fig. R1b), a substantial temperature difference is discernible, consistent with the strong descent at that time. However, at the time of the strongest descent during this event (Fig. R1c), there is actually a layer of warmer upstream air at 2 km AMSL, suggesting that the descent is weaker or absent at that time. A few hours later, when we observe a temporary pause in descent activity (Fig. R1d), the downstream profile indeed became colder, which points to the importance of cross-Alpine potential temperature differences. Finally, at 13:00 UTC 28 Feb (Fig. R1e), a second peak in descent can be observed, although the differences between the upstream and the downstream profiles are rather small.

   Focusing on the Apr 2018 event, the same finding as for the Feb 2017 holds true: The upstream profile is already colder before the first descent is diagnosed in the hotspot along the Rätikon (Fig. R2a). Later in the afternoon, despite pronounced potential temperature differences across the Alps, the descent activity was actually quite weak. Comparing the 20:00 UTC and the 16:00 UTC profiles (Figs. R2b,c), one would expect a stronger descent at 16:00 UTC if the potential temperature differences were the only driving factor. However, it is the other way around (see Fig. 11b in the manuscript). (see Fig. 11b in the manuscript). During the night, as nocturnal cooling formed a stable layer (Fig. R2d), the lowest levels at Vaduz are indeed colder compared to the upstream levels. The following day (Fig. R2e), we have a deep mixed layer downstream, which facilitates the descent of the colder upstream air.

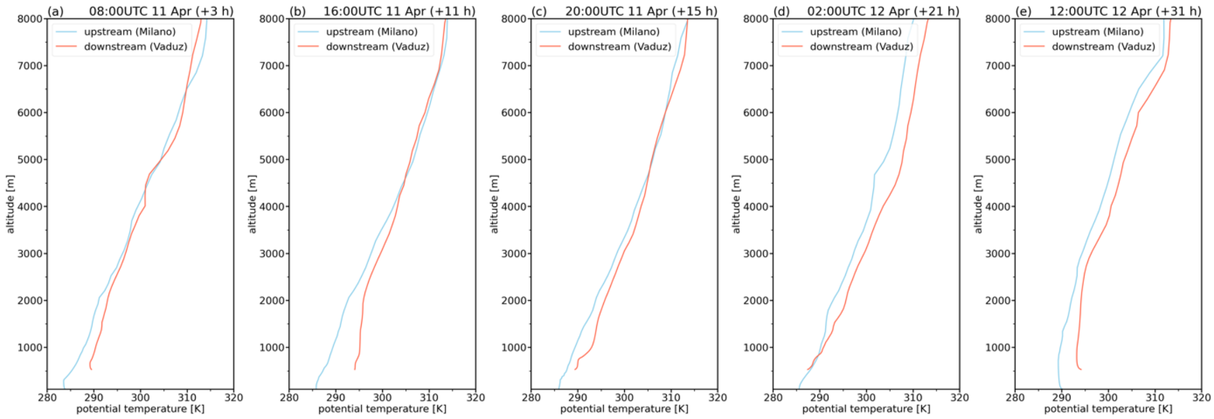

Figure R2. *Same as Fig. R1 but for the Apr 2018 event.*

To conclude this first part of our response, the cross-Alpine temperature difference does partially correlate with the temporal variability of the descent. In both cases, however, the analysis shows that the upstream air was already colder before the descent began. Furthermore, there are periods when the profiles suggest descent (colder air upstream), but it does not occur, and vice versa. In essence, while the cross-Alpine potential temperature differences are undeniably linked to the descent, they do not appear to be either a necessary condition (e.g., Fig. R1c) or a sufficient condition (e.g., Fig. R2a) for descent, at least according to our dataset.

b. We acknowledge the reviewer’s second idea. However, due to the challenges associated with defining a proper upstream location for trajectories traversing the 3D Alpine arc, we did not pursue this second suggestion. However, these thoughts on the onset are really interesting and worth investigating given the proper analysis framework. We leave it for future studies.

c. Taking up the reviewer’s third suggestion, we plotted vertical cross section along the lines c1 and c2 for all time instants during the Feb 2017 event. We cannot show all time instants in this document, instead we will only focus on the onset of the event (Fig. R3).

At 23:00 UTC 26 Feb (Figs. R3a,b), the virtual topography formed by the 294 K and 296 K isentropes is very smooth, coinciding with virtually no across-ridge wind component. This situation results in negligible vertical motion. By 01:00 UTC 27 Feb (Figs. R3c,d), a very small across-ridge potential temperature gradient is evident. At the same time, the first gravity wave forms in the lee of the Schesaplana and a local lowering of the 296 K surface is discernible. We cannot say whether the wave causes the deformation in the virtual topography, or whether the wave results from the deformation in the topography (i.e., the local descent), as both features arise simultaneously. This corroborates our statement from the first round of revisions, namely that these two phenomena are intrinsically coupled, making it very challenging to deduce a causal relationship. Moving to 05:00 UTC, a wave begins to form in the lee of Falknis as well, being associated with a local deformation of the isentropes (Fig. R3e). Focusing on the cross section through the Schesaplana (Fig. R3f), the amplitude of the wave surpasses the height difference across the ridge formed by the virtual topography (i.e.., the 296 K isentrope), despite the presence of an across-ridge gradient in potential temperature. This points towards an active role of gravity waves in shaping the virtual topography, and hence influencing the descent. At 09:00 UTC (Figs. R3g,h), the waves are accentuated along both vertical cross

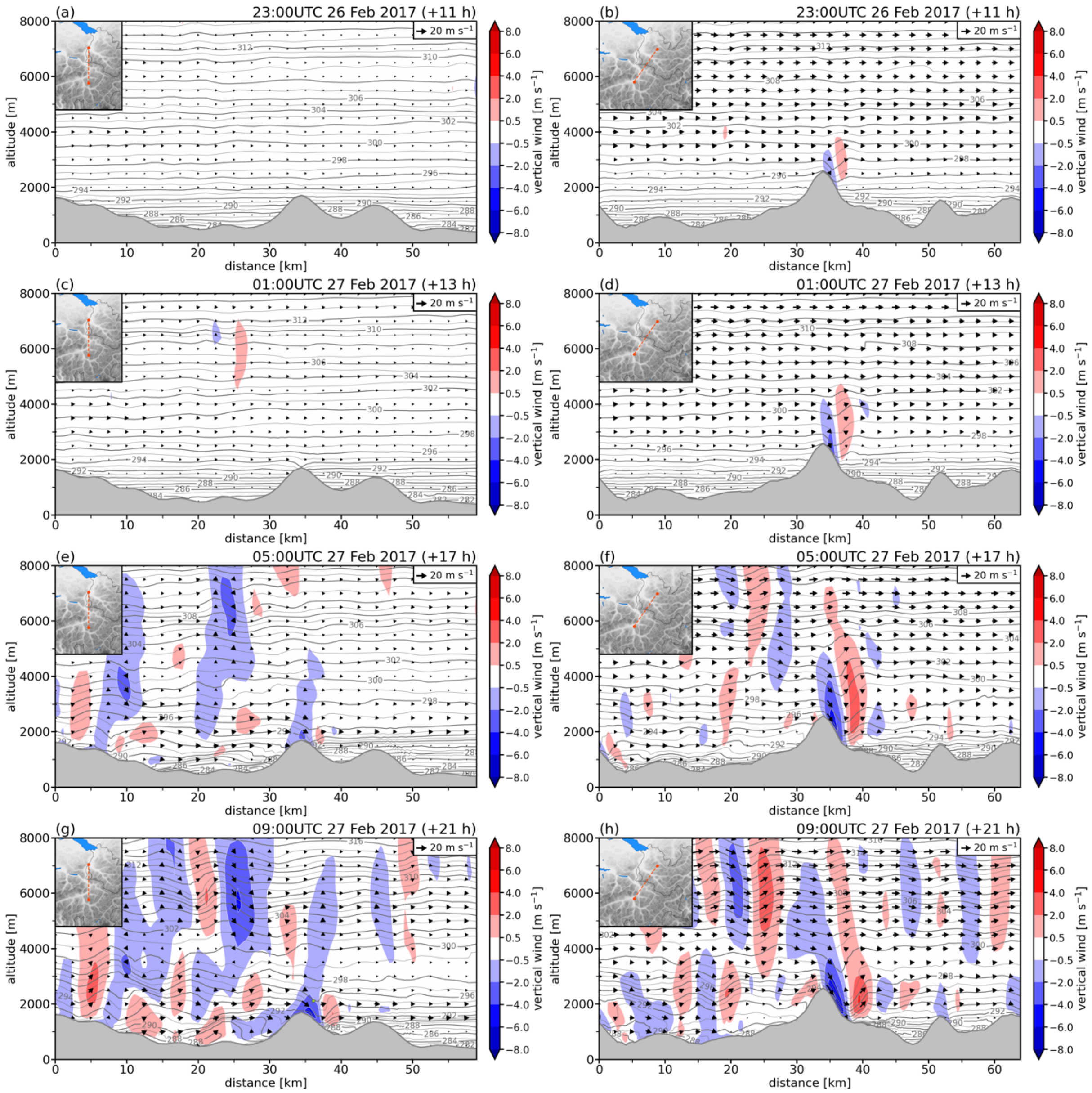

Figure R3. *Vertical cross sections of vertical wind (colormap), isentropes (gray contours) and wind along cross sections (vectors). The topography is indicated by gray shading. The left column shows the cross section c1, while the right column shows the cross section c2 (refer to Fig. 7 in the manuscript for orientation).*

sections. The wavelength of the wave in the lee of the Schesaplana approximately aligns with the shape of the real topography. However, a cold-air pool persists at the lowest levels, impeding the complete descent to the downstream valley floor.

We can provide the reviewer with another example that, in our opinion, suggests a more active role for gravity waves in the descent. For this purpose, we focus on the time period during the Apr 2018 event when we diagnosed a temporal break in the descent due to nocturnal cooling (see the manuscript for details). Figure R4 shows the temporal evolution in half-hourly steps along the cross section c3 during this night. At 02:00 UTC (Fig. R4a; this corresponds to Fig. 13b in the manuscript), the 294 K isentrope forms a smooth virtual topography that inhibits descent below 1.8 km AMSL. During the subsequent hours, a pattern of trapped lee waves emerges downstream of the Falknis peak (Figs. R4b-h). These waves are associated with a deformation of the 294 K isentrope, even in the absence of an across-ridge gradient in potential temperature along the cross sections. Based on this,

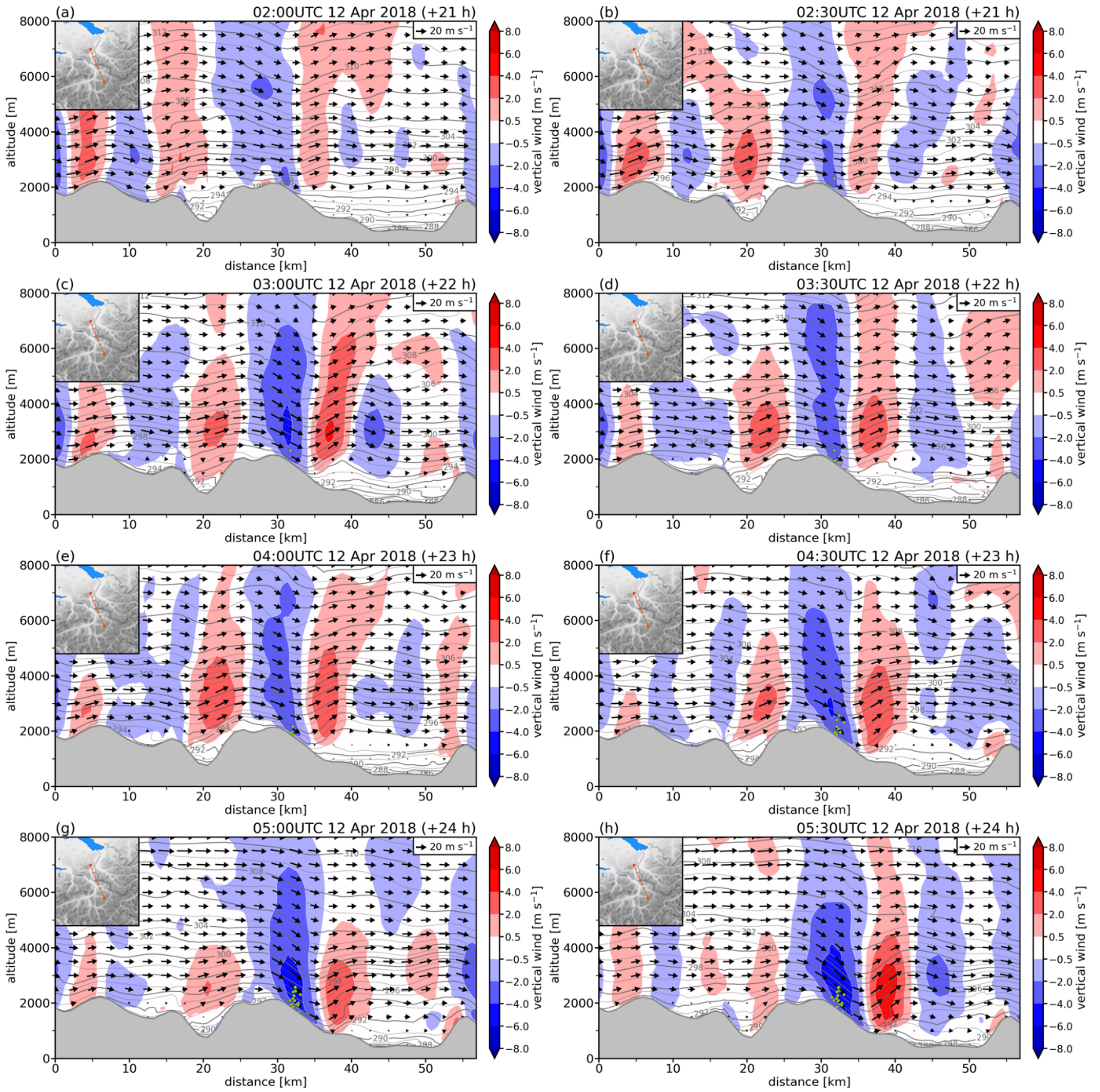

Figure R4. *Vertical cross section showing the same fields as in Fig. R3, but for the cross section c3 (refer to Fig. 7 in the manuscript for orientation).*

we hypothesize that the gravity waves actively disturb the virtual topography (i.e., the cold air in the valley) by turbulent mixing. This disturbance facilitates the descent in the early morning hours, before bottom-up erosion by diurnal heating could play a significant role. Once again, this points towards a more active role of gravity waves in the descent process.

Wrapping up our response to the fourth reviewer's comment, we cannot draw definitive conclusions from the additional analyses we performed. The cross-Alpine potential temperature differences appear to be relevant for the descent, but do not emerge as the only decisive factor. The vertical cross sections indicate that gravity waves could also play an active role in the descent. In this light, we refrain from including any of these analyses in the manuscript, as they would only add to the already considerable length of the manuscript without providing a definitive answer regarding the causes of the descent. While this question is interesting and relevant, it is not straightforward to answer with our dataset. We acknowledge that properly addressing it would require a sophisticated framework beyond the scope of our present study. Consequently, we leave this very interesting aspect for future research.

5. **Extracting the effects of gravity waves:** Section 5.3 on the limitations of the study states in lines 598-599 that the effects of gravity waves are difficult to extract from mesoscale NWP data. Although this statement is part of a discussion on obtaining a Lagrangian momentum budget, I think it also holds more generally. Maybe the emphasis on gravity waves in the previous parts of the article can be reduced and the focus put more strongly on the core findings of the paper - how to identify descending particles, and to examine their characteristics?!

We fully agree that this statement about the challenges of extracting gravity wave effects from NWP data applies more generally. We now emphasize this at L. 549-550. Following the suggestion to further reduce the emphasis put on gravity waves, we have removed several non-essential statements related to gravity waves in Section 4 of the manuscript:

- L. 363
- L. 368
- L. 394-395
- L. 457-458

We want to emphasize that we have already reduced the focus on gravity waves in our initial round of revisions. Notably, in Section 3, references to gravity waves have been minimized, appearing only in two instances in the revised manuscript. We therefore hope that, in combination with our additional adjustments, we have reduced the emphasis on gravity waves sufficiently, allowing the reader to focus more on the core results presented in the paper.

Minor comments:

1. **Lines 4-5:** Can you be quantitative about the fraction of all descents examined that are dry adiabatic and along isentropes. And also state, when and where descent is not along isentropes?

How many of the trajectories are dry-adiabatic and along isentropes? The answer to this question can be found in Figs. 6a,c in the manuscript. However, the exact quantification is difficult – as no trajectory experiences *exactly* 0 K change in potential temperature and 0 g kg^{-1} change in specific humidity. If one considers the narrow intervals of $\Delta\theta \in$ [-0.5 K, 0.5 K] and $\Delta q_v \in$ [-0.5 g kg^{-1}, 0.5 g kg^{-1}], then 58% of the air parcels descend adiabatically and 86% of the air parcels descend with no major specific humidity changes. However, as these thresholds are somewhat arbitrary, we would prefer to refrain from such an explicit statement in the abstract or the manuscript.

Where is descent not along isentropes? The locations where air parcels do not descend dry-adiabatically are found in Figs. 6b,d. They are predominantly located to the south of the Alpine crest, where the impinging air mass during south foehn is still humid and oftentimes precipitation occurs during ascent, followed by evaporative cooling during descent. We now mention this in the abstract as well (L. 12ff).

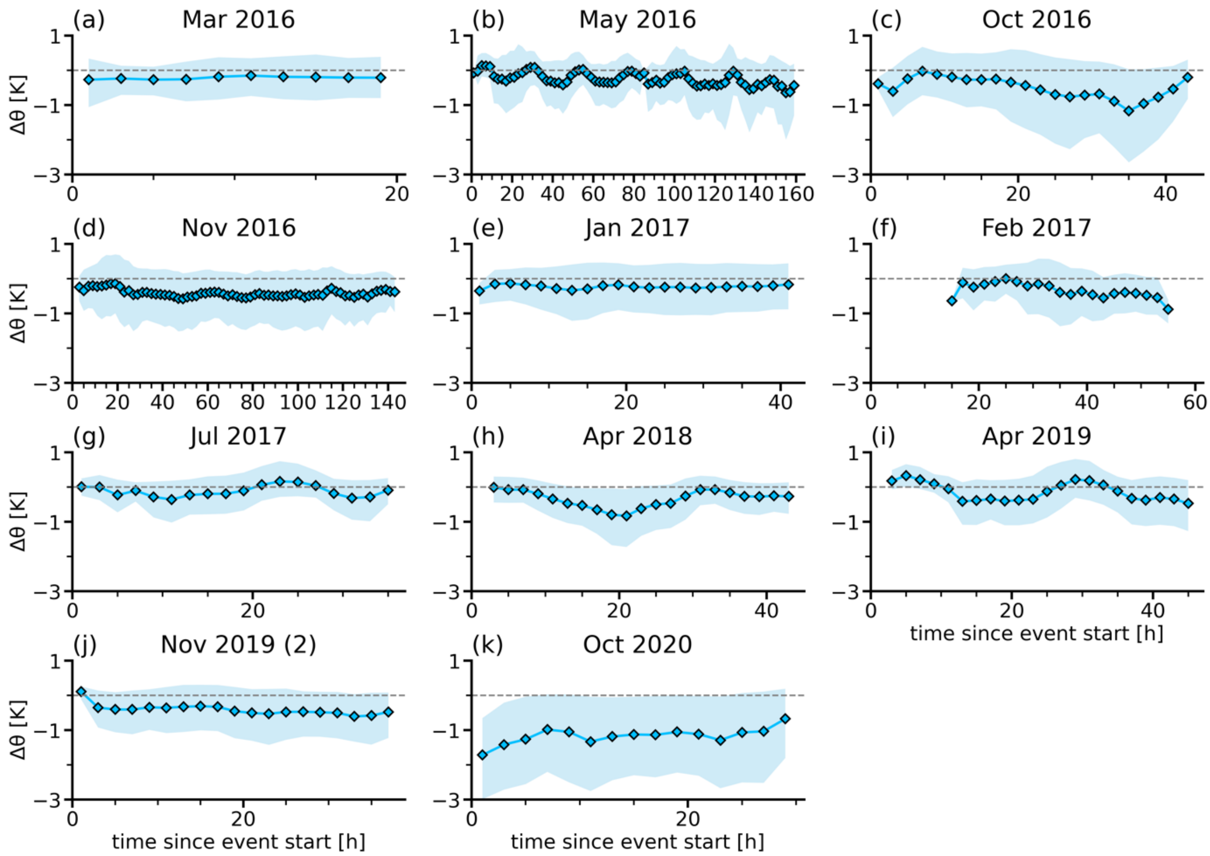

Figure R5. *Temporal evolution of the mean potential temperature change ($\Delta\theta$) for all descent segments (blue lines with markers) and its spread (10th to 90th percentile; blue shaded area) within two-hourly windows for the 11 events where more than 1000 trajectories were selected.*

When does the descent not follow isentropes? To this end, we would need to perform a temporal analysis of the events, which we only presented in the manuscript for the Feb 2017 and Apr 2018 events and for the hotspot region. Below one can find the temporal evolution of $\Delta\theta$ and Δq_v for all descent segments and all events with more than 1000 trajectories (Figs. R5 and R6). No distinct temporal evolution emerges. Besides a clear event-to-event variability, some cases exhibit a diurnal cycle in $\Delta\theta$, which is however not as clearly visible in Δq_v. To not extend the manuscript's already substantial length, we refrain from including any of these findings. Note that if the reviewer is interested, an analysis of the temporal evolution of all events (descent and its magnitude) can be found in the main author's dissertation (Jansing, 2023).

2. **Line 7:** Care needs to be used with the formulation "novel approach" here and in other parts of the manuscript. It is misleading since e.g. Miltenberger et al. (2016) and Saigger and Gohm (2022), both of which are cited in the manuscript, also used detailed trajectory analysis.

We think our approach is indeed novel in the sense that we are the first ones to systematically identify and characterize descent using trajectories, while the earlier studies either focused on warming mechanisms (Miltenberger et al., 2016) or used the trajectories more qualitatively, i.e., without an algorithm that objectively filters out the descending motion. However, to be more careful with our wording, we adjusted the manuscript accordingly. In the title, we omitted "novel" (see also below). In the abstract (L. 6), we replaced "novel" by "innovative". In the conclusions, we omitted "novel" (L. 623) or replaced it by "extensive" (L. 655).

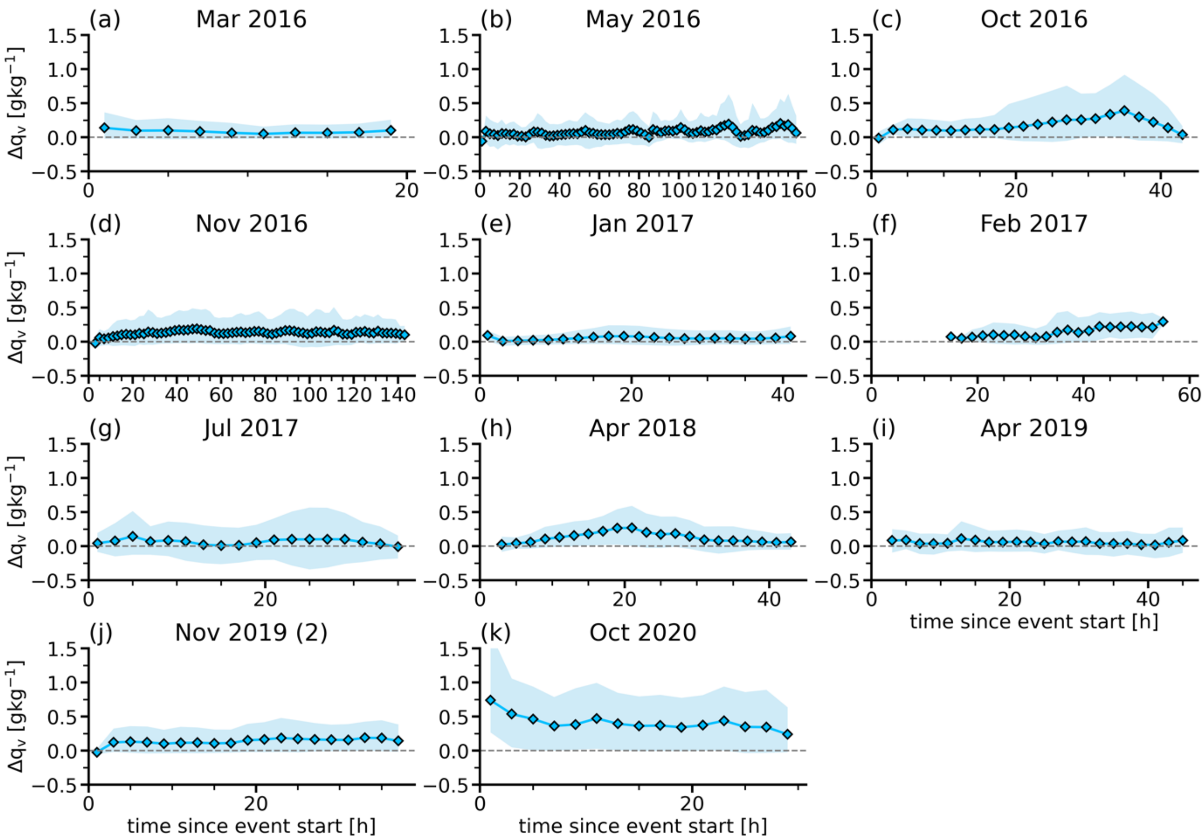

Figure R6. *Same as Fig. R5 but for the moisture change (Δq_v)*

3. **Lines 67-68:** With negative buoyancy from evaporation, should there not be convection? Also: "waterfall theory" is an expression that is not commonly used. Should that expression describe a hydraulic response such as in water descending behind a weir?

We are not quite sure what the reviewer is referring to. If the reviewer is referring to downdrafts in convective cells, which are known to gain negative buoyancy through evaporative cooling, then this is indeed analogous to the "waterfall theory". The respective publications argued for an important role of evaporative cooling in the descending motion. The term "waterfall theory" refers to the visual similarity of the descending air masses from the foehn wall to a waterfall. It is mentioned in several publications on the Alpine foehn (e.g., Steinacker, 2006; Sprenger et al., 2016). We have added these references to the manuscript (L. 64) to emphasize that the term was not invented by us, but taken from the existing literature.

4. **Lines 99-100:** Delete that sentence as it is not congruent with the previous exposition of two competing explanations of foehn. It does not bolster arguments in the manuscript. "Intrinsic" means independent of factors outside of a system, in this case, outside of gravity waves. Hydraulic theory posits that gravity waves are launched as a response to air that descends.

This sentence cites a statement from an accepted paper that went through the peer-review process (Elvidge et al., 2020). We will therefore not delete it. However, we rephrased it to make it clear that this conclusion is made in Elvidge et al. (2020) and not by us (L. 92-93).

5. Please **add** information of the temporal resolution at which model output is stored. You mention in your response only that it is longer than 5 minutes.

Thanks for mentioning this. This information is actually found in Table A1 in the Appendix of the manuscript.

6. **Lines 203-205:** Please add the important information that along-level diffusion is turned off for slope angles of more than 13 degrees (in this case), as you stated in your response. Other readers, not just this reviewer, might be unaware of it.

We agree that this is important information worth mentioning. We added the information to L. 173ff.

Textual comments:

1. Delete “novel” from title since the publication of the article implies that this is a new approach. (And it shortens the long title a little)

We agree that the title is rather long, and therefore we deleted “novel”.

2. Line 3: Delete “modern”. Both theories are more than half of a century old.

Agreed, we deleted “modern”.

3. Line 9: “precisely” is not needed. It implies that an accuracy metric is specified in the manuscript - which there is none.

Agreed, we omitted “precisely”.

References:

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