On the descent of the Alpine south foehn

Lukas Jansing et al.

I thank the authors for their constructive and thoughtful responses to my review and the second reviewer's comments. The addition of subsection 5.3 on the limitations of the study is particularly helpful for readers to properly interpret the results. It also provides valuable suggestions for future research by the authors and other researchers. In light of these changes, I withdraw my recommendation to reject the manuscript and instead recommend accepting the manuscript conditional on undertaking some major revision steps. The authors have produced such a unique dataset from which more insights can be gleaned!

Note: Line numbers refer to the version of the revised manuscript that tracks the changes compared to the original version.

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.

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.

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)l. 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.

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)?

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?!

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?
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.

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?
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.
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.
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.

Textual comments:

- 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)
- Line 3: Delete "modern". Both theories are more than half of a century old.
- Line 9: "precisely" is not needed. It implies that an accuracy metric is specified in the manuscript - which there is none.

References:

Mayr, G. J., & Armi, L. (2010). The influence of downstream diurnal heating on the descent of flow across the Sierras. *Journal of Applied Meteorology and Climatology*, *49*(9), 1906-1912.

Miltenberger, A. K., Reynolds, S., & Sprenger, M. (2016). Revisiting the latent heating contribution to foehn warming: Lagrangian analysis of two foehn events over the Swiss Alps. *Quarterly Journal of the Royal Meteorological Society*, *142*(698), 2194-2204.

Reinecke, P. A., & Durran, D. R. (2009). Initial-condition sensitivities and the predictability of downslope winds. *Journal of the atmospheric sciences*, *66*(11), 3401-3418.

Saigger, M., & Gohm, A. (2022). Is it north or west foehn? A Lagrangian analysis of Penetration and Interruption of Alpine Foehn intensive observation period 1 (PIANO IOP 1). *Weather and Climate Dynamics*, *3*(1), 279-303.

Umek, L., Gohm, A., Haid, M., Ward, H. C., & Rotach, M. W. (2022). Influence of grid resolution of large‐eddy simulations on foehn‐cold pool interaction. *Quarterly Journal of the Royal Meteorological Society*, *148*(745), 1840-1863.