

Review for

‘Is it north or west foehn? A Lagrangian analysis of PIANO IOP 1’

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Synthesis:

The authors present a case study of a west to northwest foehn event in the Inn Valley that occurred during the PIANO field campaign in Oct 2017. The mesoscale WRF simulation is validated using the extensive measurement dataset gathered during IOP1, and then combined with Lagrangian air parcel trajectories to assess three core topics:

- Air mass origin, air streams and diabatic processes upstream of the Alps
- Inn Valley boundary crossing and the associated penetration mechanisms
- The classification of such events into north or west foehn

The study is well-written and structured in a meaningful way. However, upon reading the title of the manuscript, I wonder whether it actually addresses the most important result of the study. In my opinion, Section 5.1 and 5.2 concerning the valley boundary crossing and the associated penetration mechanisms are an innovative assessment of Foehn descent mechanisms and pathways, combining the Lagrangian and the Eulerian perspective. The authors might consider this to be reflected in the title of the study as well. However, it is purely a suggestion and the importance of the key results can, of course, be assessed differently. Besides this comment, I have some concerns regarding the verbosity of the manuscript. For example, while the large-scale overview and the WRF model validation is important to set the scene for the case study, it could be written in a more concise manner and, more directly, guide the reader towards the main conclusions of the study. Hence, I added some suggestions in the minor comments below, where I find the authors might consider re-writing and shortening certain text passages. In addition, I think the current embedment of diabatic processes into the study should be reconsidered. First of all, the respective research question could be rephrased more precisely. Secondly, the relevance of Section 4.2 and 5.3 remain, to a certain extent, unclear. Either, the authors emphasize the relevance of the discussion of diabatic processes for the penetration of the Foehn air, or embed the results into other literature addressing diabatic processes of Foehn flows. If so, I suggest adding an additional discussion chapter with respect to this topic. Or, alternatively, the results concerning diabatic processes could also be discarded from the study. I included the respective comments in the points below where I saw fit. Besides these minor concerns and some additional minor comments, I didn't identify any major concerns. Therefore, after the authors have addressed my comments, I would recommend the study for publication in *Weather and Climate Dynamics*.

Minor comments:

Abstract:

- L10, L18: Terminology: When invoking the Lagrangian perspective, I would either always use the phrase “air masses”, “air parcels”, or simply “air”, but not use them as synonyms, in order to avoid terminological confusion. The authors might want to

double check this throughout the manuscript and also clearly differentiate between “air parcels” and “air stream” (the latter being a coherent bundle of air parcels).

- L24-25: It is unclear to me what kind of foehn criteria you refer to here. Is north and west foehn distinguished based on an estimate of adiabatic heating? You could specify this with additional information, or re-write the last sentence of the abstract.

Introduction:

- Fig. 1: If I checked it correctly, the abbreviation IVB (Inn Valley boundary) is not defined where it is first used.
- L77: “... various diabatic and adiabatic processes within the flow” sounds a bit strange to me. Maybe you could leave “within the flow” or replace it by “along the transport pathway”.
- L78: “this” → “these”
- L79-L80: Could you specify a bit more what kind of events you refer to? “heat events” are probably “heat events in Japan”, “melt events” are “melt events in polar regions”?
- L84-85: It seems a bit unclear to what location the 20% refer to. To Innsbruck (i.e., the Inn Valley itself)? Or does this refer to upstream precipitation frequency? Please rephrase accordingly.
- L91-105: As one suggestion to shorten the manuscript: Is this discussion of the impact of moisture on gravity waves really relevant, or could it be shortened?
- L115: Research question three could be formulated in a more precise manner. It remains unclear, whether the authors refer to the role of adiabatic and diabatic processes for the descent or the heat budget of air parcels. You might want to consider rephrasing the third goal of the study (see also related comment in the synthesis).

Data and methods:

- Section 2.1 and Section 2.2 are both well-written, but rather extensive. Perhaps, the authors find one to two passages where they could shorten the methods section a bit, e.g., the listing of the measurements and stations used (L123-138), or the detailed description of the model setup (L160-169).
- L188: I don’t understand how the total number of trajectories can be only 18’688. If you have $dx=dy=200$ m on a 4x4 km square, this would result in $21 \times 21 = 441$ trajectories per horizontal plane. Combining it with $dz = 25$ m over a distance of $2500\text{m} - 700\text{m} = 1800\text{m}$, I end up with $441 \times 73 = 32’193$ trajectories. Maybe I misunderstood the trajectory setup. In that case, I would be glad if you can clarify it shortly.
- L191-192: Could you please explain what this jump option implies?
- L200: Please give some references to these other trajectory tools.
- L202-203: I think I understand what you mean but the sentence seems a bit hard to understand at first. Maybe rephrase in an easier way.
- L210: Are there not five hydrometeor classes in the microphysics scheme (cloud water, rain, cloud ice, snow, graupel)?
- L227: Is there any residual resulting from the equation applied to your data? Could you please comment on its magnitude (either here or when you present the respective results in Section 4.2)?

- L235-L240: I had difficulties in understanding this entire passage. I think you should consider reformulating the description of the Eulerian mapping of Lagrangian trajectory information. Also, I'm unsure whether you calculate "mean trajectory properties" or "median properties", since you write "by taking the median of various parameters".

Meteorological overview:

- Section 3 is, as well, rather extensive considering it is not addressing any research question directly. However, the meteorological overview is still helpful to understand the case study and, therefore, it is fine to have an extensive overview. However, I added one suggestion to shorten a passage below.
- Fig. 5: With the chosen colorbar I have some difficulties inferring on the vertical stratification from the figure. Maybe a few contour lines of potential temperature could be added for readability?
- L269: Since the flow deflection is not visible very clearly at 700 hPa, you could add "(not shown)" to the first part of the sentence. Or, alternatively, display the 850 hPa wind field in Fig. 2b.
- Fig. 7 caption: The phrasing "... with values every integration time step at the WRF grid point closest to UNI" confuses me a bit. Probably you rather mean "... with values from every output time step of the WRF grid point closest to UNI."?
- L298-315: Could this be more concise since the comparison just serves for the model validation?
- L296: typo ("form" → "from"). I saw this typo multiple times, please check at other places throughout the manuscript.
- L318: "cold-air pool" → "CAP"
- L324-325: The meaning of this sentence is a bit hard to grasp. Maybe you could rephrase it or explain it a bit more precisely.

Air mass origin and history

- Section 4.2 As mentioned in the synthesis, the goal of this Section remains a bit unclear. What do we learn from it? Is it relevant for the penetration into the Inn Valley? In my opinion, it would need a bit more context why the thermodynamic assessment is of relevance for the foehn community (e.g., in an additional, short discussion chapter).
- Section 4.3: The Section is very interesting, since it aims to explain the differing thermodynamic properties and altitude of origins by the varying importance of two different air streams. However, the reader is left with two open questions at this point: First of all, since the total moisture change, the altitudes and the total potential temperature change is shown for all trajectories, their temporal evolution can only indirectly be linked to the varying fraction of the contributing air streams. The authors might consider it worthwhile to add the air stream medians of those three variables as well to Fig. 11a-c, to see whether the air streams also change their properties over time or just the contributing fraction varies. Secondly, the causes for the change in the relative contribution of the different air streams remains unclear. Maybe the authors can either speculate upon the possible reasons for the temporal evolution, or declare this to be beyond the focus of the study?

- L340,341,342: I think you should correct all the “means” here to “medians”, since your binned trajectory maps always display median properties if I understood it correctly. The same is true for later occurrences.
- L364: I think it would be the green line in Fig. 11d. But it might anyways be better to refer to Fig. 8 or 9a at this point, since Fig. 11 has not yet been discussed and appears considerably later in the manuscript.
- L364-366: The authors might want to reconsider the interpretation in this passage due to the following: First, are there really two air streams visible in Fig. 8 that merge over the Swiss Plateau? Especially considering the contour line in Fig. 9 it rather seems that one branch originates in the western part of the Swiss Plateau and another one in the central part of the Swiss Plateau, both of them merging approximately south of Zurich. Secondly, the figures don’t explicitly depict a west foehn effect in the lee of the Swiss Jura. Since it is not shown explicitly, I think you should refrain from this statement, or declare it hypothetically.
- Fig. 10: At this point, the number of trajectories ($7824 + 2672 = 10496$) is again different to the number you mentioned in the methods Section of the manuscript. Could you comment (probably best in the methods) how you end up with this sample size (due to the exclusion of trajectories arriving higher than 1700 MSL)?
- L440-442: This sentence could be rephrased in a more specific way to illustrate the linkage of upstream stability and the Alpine flow regime.

Local aspects

- L486: “Inn Valley boundary” → “IVB”
- Section 5.3: It’s questionable whether this Section really provides additional insight for the main conclusions of the study, since similar processes have been discussed earlier on a regional scale in Section 4.2. Please motivate more clearly why the diabatic effects are discussed.

Discussion

- Corresponding to earlier comments and the synthesis: There is some discrepancy between the discussion and the results. While a considerable part of the results addresses the air mass origin and diabatic processes, the discussion solely focuses on the penetration mechanisms and the correct classification (“west foehn” vs. “north foehn”). Although there are probably no publications addressing diabatic processes of this foehn type, there are still a few publications addressing these aspects for other foehn events which might be considered useful as reference to embed the results into the literature in an additional, short discussion chapter.
- Section 6.2: The discussion of the foehn classification is rather detailed. Given that the Lagrangian perspective anyway indicates that the applicability of a binary classification is questionable, the authors might consider to shorten this aspect of the paper.
- L593: “Richter” → “Richner”
- L610: “provide” → “provides”

Conclusions

- As a comment regarding the structure: The conclusion comprises an extensive list of eight bullet points. I think it would facilitate reader guidance if you could add some more structure to it, because it seems a bit unclear, if they are all equally important.

As a suggestion, they could be grouped according to the research question they address, and possibly the number could be reduced to one bullet point per research question/key result.

- After having read the conclusions, I still wonder whether the synoptic evolution of this case study represents a typical evolution for north or west foehn events. In other words, can similar results be expected for other northwest foehn cases? What changes do the authors expect for cases without orographic precipitation? Maybe you want to add one to two sentences addressing the generalization potential of the study.