

**Review of “Is it north or west foehn? A Lagrangian analysis of PIANO IOP 1”
by Saigger and Gohm**

A case study of northwest or north foehn impacting the Inn Valley is presented. Foehn from this direction are much less common than southerly foehn for the Inn Valley and have not been examined in very many previous studies. Consequently, this study of such an event during the PIANO field campaign is timely and welcome.

Overall, this case study is well presented and reasonably well written. There are a number of places where I think it is a little too detailed and parochial. I think during the revision I’d urge the authors to trim it down a little and focus on what will be of interest to community more broadly and more generally. I’ve indicated a few places where some information could be deleted or abbreviated without any impact on the key results or conclusions.

I don’t have any major comments but I have a number of specific, relatively minor, comments that I’d recommend the authors address.

Abstract

Overall, the abstract seems quite long – does it conform to required word length?

Line 1 – reorder sentence “, Austria, that occurred on 29 October 2017 is investigated...”

Line 4 – “morning”

Line 14 – 10-40% of what?

1. Introduction

Line 30 – safety

L37 – perhaps bracket “(in the west-east aligned Inn Valley)”

L38 - rephrase “Although it has been known...” delete also

L45 – replace “about” with ‘of’

L72 – “purely”

L78 “these methods”

L83 – you should probably cite Elvidge and Renfrew (2016, BAMS) who were the first to use trajectory analysis to quantify foehn warming mechanisms. Miltenberger et al. (2016) follow and cite that study.

L84 – “time” not times – “were” not was

L91-99 – this paragraph is quite long – probably could be shortened.

Section 2

L127-132 – this list of stations and heights could be deleted. The stations are already named in Figure caption 1 and the exact heights are not necessary. You could perhaps group them as ‘mountain top or crest stations’ and ‘valley stations’ in the caption.

L139-151 – you don’t need to tell us how you calculated potential temperature, or if you feel the height adjustment method is necessary just ‘PT follows standard formula and a height adjustment is made as used by Muchinski et al.’ But I don’t think it is – that sort of adjustment is standard.

L191-196 – the stuff about the CFL criteria could probably be deleted or abbreviated.

L210-220 – again the background on ice-liquid potential temperature could be abbreviated to one sentence and no formula.

Note: you say “1000 m MSL” etc many times, strictly speaking this should be “1000 m above MSL” in every instance.

Section 3

Figure 4 shows model cross-sections of a rotor in the Inn Valley at two times. I was wondering if there were any observations of rotors at this time? I know these were observed during PIANO (c.f. Haid et al. 2020). It would have been nice to have seen some of the lidar wind observations from PIANO used to validate the model in this way.

L303 – I'd maybe rephrase, in Fig 7 the increase in wind speed and the shift to westerlies are both prominent, the speed increase perhaps more so.

L320 – you note a lack of turbulent mixing as a potential cause of the cold bias. This may be a good point to cite recent papers on foehn/cold-pool interactions, some of which have come from PIANO case studies, e.g. Haid et al.

L324 – I'd rephrase this line "They should therefore not be seen as the truth in all aspects but rather as one possible flow realization to learn more about the air mass transport from a Lagrangian perspective". One flow realisation makes it sound like another simulation from say an EPS would capture something different, but I don't think that is the case. I think the limitations of the model simulation are more to do with model physical parameterizations (the cold-pool breakdown is common and related to BL turbulence etc). I'd suggest rephrasing to state the simulation is reasonably good and sufficiently good for detailed analysis of the origins of air masses and so on, i.e., the following analysis. But it has limitations most likely related to long-standing weaknesses most likely related to parameterization weaknesses.

Section 4

L331 – Noting above, I'd rephrase to "reasonable agreement" rather than good agreement

L350-352 – could perhaps be deleted – doesn't add much

L377 – perhaps give the "times before arrival" of when these locations (Vosges etc) are passed

L394 – I am afraid I could not see an increase in θ_{il} in Fig 10b in the median line. I can see the increases in θ that are discussed. Perhaps you mean in the shaded area? Similarly in Fig 10h, I struggled to see the gain in moisture – the trace looked pretty flat to me. This does partly undermine some of this paragraph.

Section 4.3 – Temporal evolution

In all honesty, I am not sure this section added much to the paper. It seems a bit of a detail to me. The foehn event is already quite short (a few hours) breaking it into three phases seemed like more than readers would want to know to me. I'd suggest discussing whether it is important. Or perhaps whether it would be better re-located to section 5.1 where the valley-crossing section shows some nice changes in time?

Section 5

I thought Fig 12 was really good and illustrative of the changes in time.

I found Fig 13 less interesting. The vectors are not really discussed, do they tell us anything other than the flow is along the valley? You mainly talk about the cross-valley flow (shading), while the θ contours are hard to see. I wonder if this figure could be improved or simplified or replaced? Section 5.2 was also a bit long and started to get a little parochial. I wonder if you'd be better trying to edit down these 'local findings' and try to reframe this section into findings that are likely to be more generally applicable (e.g., to other locations or to other cases).

In general, I thought Sections 6 and 7 were very good, well done.