

Review of 'Comparison of temperature and wind between ground-based remote sensing observations and numerical weather prediction model profiles in alpine complex topography: the Meiringen campaign' by Bugnard et al submitted to Atmospheric Measurement Techniques

September 29, 2024

This review is for the second revision of the manuscript. I appreciate that the authors addressed my and the other reviewer's comments in this revised version. It is improved and clarified in some aspects such as the mechanism for the flow from the Sarneraatal over the Bruenigpass and the arbitrary threshold of 20 m/s to select thermally driven days. However, there are still areas that need improvement in my opinion, especially related to conciseness and precision.

The manuscript contains a lot of description and speculation, making it cumbersome to read. The description of the figures in the manuscript is very detailed with numerous mentions of heights, times, and values. This amount of detail might be appropriate for a report, but is somewhat distracting in a scientific article. The manuscript could be more precise and concise, if it focused on relevant aspects (e.g, instead of describing every little detail of the figures, it would help to focus on the aspects directly relevant for the objectives of the manuscript) and using a clear and precise wording. For example, like in these two sentences 'Concerning KENDA-1 data, the foehn breakthrough is modeled too early on March 11 at both stations, on time on March 20 at both stations and on April 23 at MER and too late on April 23 at MEE. The foehn arrival and end is modeled sometimes on time by KENDA-1, but positive and negative time shifts of up to 4h at both stations' (l. 511-514). This could be combined and be shortened: 'KENDA-1 models the foehn breakthrough 4-h too early at both stations on March 11, on time at both stations on March 20, and 4-h too late at MEE on April 23.' Cases like this are present throughout the manuscript and I strongly suggest that the authors try to use a more precise and concise language.

As mentioned in my previous review, I think a shortening of the case studies and the discussion with a focus on the most relevant aspect would be beneficial. Instead of shortening the case studies, the authors added additional analysis on the responsible mechanisms for the flow descending over the Brueningpass. I think this analysis adds value and helps to understand the observations, but, without shortening other parts of the manuscript (like the discussion or description of the figures), results in a rather long manuscript (of 39 pages with 13 figures in the main manuscript and 13 in the supplemental).

More attention should still be paid to details (correct formatting). For example, the formatting is off in the first paragraph of Sect. 3.1.3. Also, times should have units (e.g. UTC) which are consistently used throughout the manuscript. Sometimes a.m. is used. For all heights which are with respect to mean sea level

'a.s.l.' should be added. For times above ground, a.g.l. should be used. This is currently very inconsistent.

In addition to these general comments that should be addressed, I am giving some specific comments and suggestions below.

1 Specific comments

- Title: The placement of 'profiles' in the title sounds a bit off. Perhaps it would be better to say 'Comparison of temperature and wind profiles from ground-based remote sensing observations and a numerical weather prediction model in complex topography: the Meiringen campaign'.
- l. 55: Slope winds are driven by a horizontal temperature gradient between the air adjacent to the slope and the free valley atmosphere'
- l. 62-64: What is meant by wind intensity? The reversal from upvalley to downvalley winds in the evening is driven by the reversal of the along valley temperature and pressure gradient. Please clarify.
- l. 85-87: Please be more specific. Why is precise knowledge essential for NWP? To evaluate and improve the models? And why are REM a solution? Are they assimilated or used for evaluation?
- Fig. 1: Please add that BRZ stand for Brienz. It would also be helpful to add the names for BRU, LUN, BUC, and GIH to the caption.
- l. 109: 'a.s.l.' is already used in l. 94.
- l. 130: 574 m?
- l. 141-142: Is the terrain shown in Fig. S3 filtered with this 2dx filter? It looks very steep with large differences between adjacent cells. To be meaningful, the terrain that is actually used by the model should be shown.
- l. 180: SMN was already introduced.
- l. 183: Where is FRU? Not included in Fig. 1. How is the cloud amount estimation done at FRU?
- l. 185-186: Are only wind observations used at BRU? What about temperature? What is 'similar temporal resolution'? Order of minutes or hours?
- l. 199: Please add information that line of sight of about 10 km is in downvalley direction.
- l. 228: What is 'end of winter'? Was there snow on the ground after mid-December?
- l. 230: I would appreciate if a brief description of how the foehn index works was included.
- l. 250: Radio soundings are not mentioned before. Where and when were they performed? What cold bias?
- l. 257: At what height are the statistics computed?
- l. 283ff: Why are still differences used? In the response, the authors indicate that they now use gradients, but this is not clear. In Fig. 5b, the temperature difference is still shown (unit deg C). What is T inversion amplitude? Is this inversion strength? Amplitude is a bit uncommon in this context. In the response to the reviewers' comments (comment 25), the authors claim that they now include the potential sources of error when comparing ground-bases and free atmospheric observations ('T inversions observed on the ground ...'). However, this explanation does not occur in the manuscript.

- Fig. 2. Please make plots in a) and b) the same size for consistency. How is the monthly diurnal cycle computed, is it mean or median? Please add. Is the difference in b) computed before or after the monthly means/medians for the observations and KENDA are computed? Do sunrise and sunset times account for orography? Are they monthly means/medians? How is the mean ridge height computed? Is this based on the lines in Fig. 1? Please explain.
- l. 364-465: This sentence is not clear.
- l. 367: Not clear, why vertical transport is important for weakening of drainage flows.
- Fig. 7: Do sunrise and sunset take orography into account? They are different from sunrise/sunset in Fig. S8. Better show the one taking orography into account.
- l. 373: 'underestimation of wind speed' is not clear. Upvalley wind is actually stronger (positive values) in KENDA. What is missing are downvalley winds.
- l. 376-377: This sentence implies the stronger presence of upvalley wind is leading to weaker downvalley wind speed, which is not physical. Please rephrase.
- l. 382: The phrase 'onset is anticipated compared to' is not clear.
- l. 384-385: What about moist convection? Surely there are convective clouds and precipitation during the summer months that can affect the flow.
- l. 392-393: How can synoptic winds lead to continuous downvalley wind underneath. Please rephrase.
- Fig. 9: Please add a) and b) to the figure. 'wind speed values'.
- l. 422-424: Weird sentence. Please rephrase.
- l. 443: Figures should appear in the same order as they are mentioned.
- l. 449ff: Annotation (x, y, θ) is not clear. Is GIH at the same height as BRU? If not, how is pressure at GIH used to compute potential temperature at BRU? How is the valley volume computed?
- l. 482: Foehn is NOT a katabatic wind. Is Haslital on northern side of Alpine ridge?
- Fig. 12a: Are sample sizes similar for each hour? With only 117 hours of foehn detected, this would mean that there are only 5-6 samples per hour. Pretty small sample size to compute distribution and to draw meaningful conclusions. This limitation should be mentioned.
- l. 509-510: This comparison is hindered by the fact that the value at SMN/MER is observed at the surface and the value at DWL/MEE at 800 m a.s.l.
- l. 529: Please formulate as hypothesis, 'can explain'.
- l. 531-532: Bise is mentioned here for the first time. How are Bise situations determined? The enhancement is is not documented.
- l. 546ff: Please explain why monthly medians are used and not temporally resolved values. Is this because of noise? The KENDA-1 analysis is used, i.e. the forecast skill is not investigated.
- l. 566ff: Several studies focused on valley wind in the Inn Valley. A classic one that would be good to include is Vergeiner and Dreiseitl (1987).
- l. 579: Where is Sion?
- l. 602ff: A study focusing specifically on the vortices in the Inn Valley is Babić et al. (2021).

- l. 626: What is 3?
- l. 637: The sentence 'Westerhuis et al.' is not clear.
- l. 655: Since the information content from the passive microwave radiometer decreases with height, the vertical resolution decreases and inversions and elevated layers are smoothed with height (e.g. Crewell and Löhnert, 2007).
- l. 670: Where is Visp? Without more detail this comparison does not make sense.
- l. 671: 'four-time shorter length of the Haslital' compared to which valley?
- l. 682-683: Differences in real-world and model valley depth certainly are also important.
- l. 697: 'from November 2021 through August 2022'.
- l. 713: Please rephrase '... simultaneous for the entire the profile.'

References

- Babić, N., Adler, B., Gohm, A., Kalthoff, N., Haid, M., Lehner, M., Ladstätter, P., and Rotach, M. W.: Cross-valley vortices in the Inn valley, Austria: Structure, evolution and governing force imbalances, *Quart. J. Roy. Meteor. Soc.*, 147, 3835–3861, doi:10.1002/qj.4159, 2021.
- Crewell, S. and Löhnert, U.: Accuracy of boundary layer temperature profiles retrieved with multifrequency multiangle microwave radiometry, *Geoscience and Remote Sensing, IEEE Transactions on*, 45, 2195–2201, doi:10.1109/TGRS.2006.888434, 2007.
- Vergeiner, I. and Dreiseitl, E.: Valley winds and slope winds - Observations and elementary thoughts, *Meteor. Atmos. Phys.*, 36, 264–286, doi:10.1007/BF01045154, 1987.