Answers to the reviewer 2 technical corrections on "Comparison of temperature and wind between ground-based remote sensing observations and NWP model profiles in complex topography: the Meiringen campaign"

We thank the reviewer for the hopefully last technical corrections to our manuscript.

Technical corrections:

- L. 43 Subscale processes → Do the authors mean subgrid-scale processes?
  Yes, this was corrected
- l. 136 Kilometre-scale Ensemble Data Assimilation --> Please replace by 'KENDA' as it is already introduced
  Done
- Section 3.1.3 There seems to be a latex error. Please correct.
  Corrected
- l. 445 at GIH and MER (10.a) --> at GIH and MER (Fig. 10.a)
  Done
- l. 449 10.b shows the difference between ... --> Figure 10.b shows the difference between ...
  Done
- l. 529 As described in 3.3 --> As described in Section 3.3
  Done
- l. 681
  According to (Schmidli et al., 2018) -> Please remove brackets
  Done at l. 690
- l. 710
  This diurnal flow pattern develop --> This diurnal flow pattern develops
  Done
- l. 713
  simultaneous for the entire the profile --> Please remove the second 'the'
  Done
- Figure 7.
  The lower axis label is cut off.
  Done

Answers to the reviewer 3 comments on "Comparison of temperature and wind between ground-based remote sensing observations and NWP model profiles in complex topography: the Meiringen campaign"

We thank the reviewer for the in-depth comments to our manuscript.

The answers to the comments and questions are written in italic thereafter. The explanations of this document cite the numbering of the figures in the first revision in accordance with the lines' numbers of the comments.

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 all still areas that need improvement in my opinion, especially related to conciseness and preciseness.

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 use throughout the manuscript. Sometimes a.m. is used. For all heights which are with respect to mean sea level 1 '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.

The modifications of l. 511-514 were done and adaptation to of the manuscript to more precise and concise language have been done throughout the results section leading to a reduction of ~10% of the text. The authors think that a further shortening of the manuscript would be at the detriment of a correct description of the complexity of the described phenomena.

Concerning the time and altitude units, the authors chose the option to mention at the beginning of Sect. 2 that times are in UTC and, if not specified, altitude in m a.s.l. In our opinion, these options make the text lighter, easier to read and allowed by AMT. The authors also paid further attention to correct details such as formatting or mis-spelling.

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'.

Done

• l. 55: Slope winds are driven by a horizontal temperature gradient between the air adjacent to the slope and the free valley atmosphere'

The authors checked several reference papers and books on slope winds. If buoyancy generated by temperature gradients are systematically mentioned as driver for slope winds, the direct mention of the direction of the T gradient is almost never mentioned. Convection processes and gravity linked to higher air density (namely corresponding to vertical processes) are often mentioned for anabatic and katabatic winds, respectively. The sentence at line 55 was then modified to solely mention "by temperature gradients" without specifying the direction of the gradients.

• 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.
Wind intensity was replaced by wind speed.

The reversal from up-valley to down-valley winds is now correctly attributed to T and P gradients:" In the evening, as soon as the surface radiative balance becomes negative, the cold air forming at the surface moves down the slope and converges in the valley floor. After the reversal of the along-valley T and pressure gradients, the flow direction shifts from up-valley to down-valley winds."

• 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?

The sentence was modified: "However, the spatiotemporal heterogeneity of T in complex terrain is challenging for NWP models and the use of REM observations is a solution to evaluate the models and improve them by the assimilation of observed profiles."

• 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.

Done

• l. 109: 'a.s.l.' is already used in l. 94.

Yes, the use of 'a.s.l.' in l. 94 is necessary to estimate the altitude of the topographic features at the site of the campaign and the mention at l. 109 allows to specify the rule applied throughout the paper. The abbreviation a.s.l. is now introduced at l. 94.

• l. 130: 574 m?

Thanks, the altitude of MEE is 589 m but the one of MEE is 574m. This is now corrected.

• 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.

Yes, the 2dx filter has already been applied to the surface topography represented in Fig. S3 and this is exactly the terrain used by KENDA-1/COSMO-1 model.

• l. 180: SMN was already introduced.

Done

• l. 183: Where is FRU? Not included in Fig. 1. How is the cloud amount estimation done at FRU?

The coordinates of FRU are given in the manuscript. It is now further mentioned that FRU is situated south of Lake of Thun. The cloud amount is estimated by measurements of longwave downward radiation, temperature and relative humidity with a time resolution of 10 min (Automatic Partial Cloud Amount Detection Algorithm, APCADA, Dürr and Philipona, 2004).

• l. 185-186: Are only wind observations used at BRU? What about temperature? What is 'similar temporal resolution'? Order of minutes or hours?

No. Lines 179-181 mentioned that wind observations are also performed at all SMN stations including MER, BRZ and GIH and l. 185-186 clearly mention wind observations from FEDRO also at LUN und BUC with similar temporal resolution, namely hourly observations.

- l. 199: Please add information that line of sight of about 10 km is in downvalley direction.
  Done

- l. 228: What is 'end of winter'? Was there snow on the ground after mid-December?
  There was no further snow cover higher than 15 cm (that corresponds to a homogeneous snow coverge) after mid-December at MER. "end of the winter" was then replaced by "end of the spring" to be more precise.

- l. 230: I would appreciate if a brief description of how the foehn index works was included.
  The main prerequisite for the occurrence of foehn on the northern slopes of the Alps is a southerly wind on the main Alpine ridge, which is measured at the Gütsch station, Andermatt, (GUE). Conversely, on the southern slope of the Alps, the wind on the main Alpine ridge must come from the north for foehn to occur. The foehn index is calculated individually for each station using different measurements. There are fixed threshold values for each parameter. These enable the index to be objectively calculated for each station, depending on where it is located and how prone it is to foehn. The parameters used (measured every ten minutes) are average speed, wind gusts, wind direction, relative humidity and potential temperature. The potential temperature is a hypothetical value that the air would have if it were measured at sea level. It is calculated from the temperature and the air pressure.
  A complete description on how the foehn index is calculated in the Alpine valleys can be found at https://www.meteoswiss.admin.ch/dam/jcr:3ed2aec8-0901-417a-acc3-8be11cce440a/Foehnindex_Arbeitsbericht_223_Automatisiertes_Verfahren_zur_Bestimmung_von_Foehn_in_Alpentaelern_de.pdf
  Since a detailed description is quite long, the authors prefer not to include it in the manuscript.

- l. 250: Radio soundings are not mentioned before. Where and when were they performed? What cold bias?
  The radio-sounding is already mentioned in Sect. 2.2 since it is assimilated by KENDA-1. In Switzerland, radio-sounding are only performed operationally at Payerne on the Swiss plateau. Since a MWR is also available at PAY, comparison between RS allows evaluating the MWR performance. The first version of the manuscript had a small section on three radio-sounding performed in November 2023 at MEE, but is was suppressed to shorten the manuscript.

- l. 257: At what height are the statistics computed?
  The heights used for the statistics of Fig. 3 are described in the figure caption: "The lowest level corresponds to 576 m for SMN/MER, 625 m for MWR/MEE and 705 m for KENDA-1/MER and 739 m for KENDA-1/MER."

- 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.
  The word' amplitude' was replaced by 'strength'.
  The answer to the previous comments were perhaps misleading. Fig. 2 a presents a T gradient (a temperature difference divided by an altitude difference) whereas Fig. 5 present the frequency of T inversion computed from the T difference and the strength of the T inversion given as the T difference. The measurement campaign set-up involves different lowest level altitudes for the ground observations, the REM observations and the model. The use of gradients does not allow to get ride of these differences, because the gradients will be computed on difference height differences. KENDA-1 has the greatest height

difference of the lowest level that corresponds effectively to the presence of the ground (associated with its physical effect) at this altitude. We can see from Fig. S6 that KENDA-1 T profiles differs from MWR profiles from the lowest level up to ~1500 m in case of missed T inversion. The underestimated strength of the T inversion modeled by KENDA-1 would appears smaller on a figure with a gradient due to the division by a smaller height difference than for MWR/MEE or the pair of ground stations. However, the discussion on the differences between the different set-up cannot be avoided. A new sentence warns the reader of this difference: "The higher altitude of KENDA-1/MEE lowest level results in a lower inversion strength but explains only 30\% and 40\% of the difference with MWR/MEE and BRU-MER pair, respectively.".
In our response, we say that the potential sources of errors are described, which was done in Sect 3.1.3 but was not readable due to an overleaf error. They are however not taken into account in Fig. 5.

• 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.

The plots of Fig. 2 have now the same size.
As already explained in Sect 2, the monthly averages are aggregated according to the median hourly values. An addition of the way of averaging each time they are used would make the text too heavy.
The median of the hourly T differences is presented in Fig. 2b. We tried both solutions. The difference of the monthly T median leads to larger values but with similar monthly diurnal cycles.
In this case, the sunrise and sunset time does not take the shading into account. In the first version of the manuscript, Fig. 9b presented sunrise and sunset including the shading at MER that was computed from the real topography and not from the lines in Fig. 1. The shading effects affect mostly sunset time between October and March. Sunrise time is almost not affected. We decided not to use systematically the shading to compute sunrise and sunset because 1) they are different at MER and MEE as well as at the different altitudes and 2) phenomena as T inversion of thermal winds depends not only on the shading at the station site but on the effect of solar radiation in the whole valley.
The monthly sunrise and sunset correspond to the monthly mean that is approximately similar to the median since the variation is quasi linear without any outliers.
The mean ridge height corresponds to the mean of the ridge from one km upstream to on km downstream from MEE station.

• l. 364-465: This sentence is not clear.

The sentence is not relevant and was deleted.

• l. 367: Not clear, why vertical transport is important for weakening of drainage flows.

You're right, this explanation is not relevant and was removed.

• 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.

No, the orography is not taken into account (see previous explanation on Fig. 2). The sunrise and sunset hours are the same as in Fig. S8, which however presents a complete seasonal cycle (Jan-Dec) whereas Fig. 7 presents only the months of the Meiringen Campaign.

• l. 373: 'underestimation of wind speed' is not clear. Upvalley wind is actually stronger (positive values) in KENDA. What is missing are downvalley winds.

The sentence was modified since it relates to the downvalley wind speed in November and December: "A comparison of the first level of KENDA-1/MER and SMN/MER (Fig. 7 b and a) indicates an underestimation of downvalley wind speed by KENDA-1/MER, leading to the absence of a diurnal cycle in November and December."

• 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.

The sentence was modified: "The modeled data at MER and MEE also show distinct differences, a stronger up-valley wind speed in MER, a weaker down-valley wind speed and the presence of weak up-valley wind during the entire days in winter."

• l. 382: The phrase 'onset is anticipated compared to' is not clear.

The sentence was modified: "The onset of down-valley winds near the ground happens earlier than at higher altitudes so that up-valley winds can persist until 1-3 h after sunset above 1500 m."

• l. 384-385: What about moist convection? Surely there are convective clouds and precipitation during the summer months that can affect the flow.

There is surely moist convection, a development of convective clouds and precipitations, but there is no reason why they should affect the wind direction only between 1000 and 1500 m. I would expect an influence on the whole profile of the wind direction.

• l. 392-393: How can synoptic winds lead to continuous downvalley wind underneath. Please rephrase.

The sentence was modified: "Finally, in winter, KENDA-1/MEE overestimates the influence of the synoptic winds, which leads to the presence of homogeneous up-valley winds down to 1000 m, and models continuous down-valley winds underneath."

• Fig. 9: Please add a) and b) to the figure. 'wind speed values'.

Done

• l. 422-424: Weird sentence. Please rephrase.

The sentence was modified: "The observed wind speeds during a series of clear warm days in July with low cloud coverage (Fig. 11) present a wind pattern in the Haslital, which is undetectable in the analysis based on monthly medians."

• l. 443: Figures should appear in the same order as they are mentioned.

Done

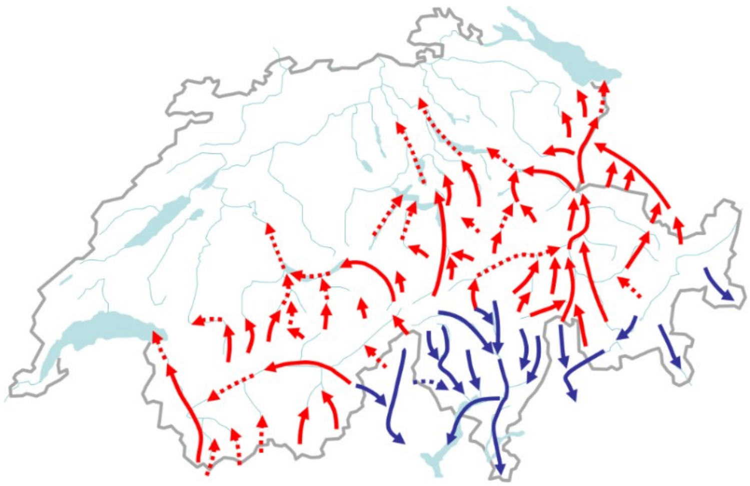
• l. 449ff: Annotation (x, y, θ) is not clear. Is GIH at the same height as BRU? If not, how is pressure at GIH used to compute potential temperature at BRU? How is the valley volume computed?

The annotation (x, y, ϑ) was replaced by (ϑ). GIH is at a lower altitude than BRU. The pressure is not measured at BRU, so that the pressure at the BRU altitude was computed from GIH ground pressure with the barometric formula. This is now mentioned in the manuscript.

The valley volume is computed from the integral of the difference between the ground and the mean ridge height for each pixel of the topography. It is specified that the volume is an approximation.

• l. 482: Foehn is NOT a katabatic wind. Is Haslital on northern side of Alpine ridge?

Yes, foehn is not a katabatic wind. The Haslital is influenced by South foehn and is then considered to be on the northern side of Alpine ridge (see next figure for the influence of south and north foehn, from https://www.meteoswiss.admin.ch/weather/weather-and-climate-from-a-to-z/foehn.html). The sentence was consequently modified: "South alpine foehn is a strong wind that brings warm and dry down-valley wind and leads to clear weather conditions on the northern side of the Alpine ridge."



Typical valleys of Switzerland with southerly foehn (red) and northerly foehn (blue). The areas in which foehn is rarely observed are marked with a dashed line. (© MeteoSwiss)

• 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.

The numbers n at the top of each figure correspond to the number of cases in each category. Fig. 12a has effectively only 3-5 cases per hour whereas Fig. 12b has between 13 and 31 cases per wind speed category. It is now specified in the figure caption that "The limited number of cases per hour in a) involves a higher uncertainty in the results.".

• 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.

Yes, but the SMN/MER is at 574 m a.s.l., namely 200 m below the DWL/MEE first level at 775 m a.s.l. It seems that the different sites (4 km in horizontal distance and 30° difference in the orientation of the valley) is as important as the height difference. We think that the differences in height, horizontal distance and terrain between MEE and MER are sufficiently mentioned in the paper, so that it is not needed to repeat it here. Moreover the next § describing KENDA-1 results mentions these differences.

• l. 529: Please formulate as hypothesis, 'can explain'.

Done

• l. 531-532: Bise is mentioned here for the first time. How are Bise situations determined? The enhancement is not documented.

Bise situation are determined by the speed of the north-, northeast- or east-wind over the northern Alpine foreland which normally occurs in the presence of northeast-southwest 850 hPa pressure gradient in the order of 1 hPa/100 km.
Wanner and Furger, Meteorology and Atmospheric Physics, 43(1):105-115, DOI:10.1007/BF01028113).
The sentence was modified :" This phenomenon can be enhanced in case of Bise situation, a N-NE synoptic winds that occurred on 35 days in the January-August 2022 period."

• 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.

As explained in the next sentence, this analysis focused on climatology so that monthly medians were principally used. Temporally resolved values are still used 1) for case studies such as the foehn events or the peculiar wind pattern during hot summer days and 2) statistical analysis such as the diurnal cycle of T differences (Fig. 3-5).

• 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).

It is now specified that only studies in the Alps and using REM instruments are cited in Sect. 4.2.2 since the main feature of this paper is the use of REM technology in a medium size alpine valley. Since the referee find important the citation of this paper, it is now cited in the introduction (l. 65 of the new manuscript).

• l. 579: Where is Sion?

Sion is in the Rhone valley. Since the precise location of the campaign is not cited, we removed the mention of Sion and replace it with "the Rhone valley".

• l. 602ff: A study focusing specifically on the vortices in the Inn Valley is Babi´c et al. (2021).

Yes, I know quite well the excellent publication of Babic et all, 2021. The measurement program during the CROSSINN campaign allowed to evidence cross-valley vortex. The modest set-up in Meiringen does not allow such a detailed analysis of the circulation in the Haslital. Babic's paper is not cited since its results cannot be directly compared with the results presented in this paper.

• l. 626: What is 3?

The mention of "Sect." was accidently omitted. It is now corrected.

• l. 637: The sentence 'Westerhuis et al. ....' is not clear.

The sentence was merged in the next one: "Westerhuis et al., 2021} showed, that, in complex topography, numerical artifacts may originate from the intersection between T inversions and the surface of the vertical grid used by the model."

• 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 L¨ohnert, 2007).

Yes, this is right and the paper of Crewell and Löhnert, 2007 is now cited.

• l. 670: Where is Visp? Without more detail this comparison does not make sense.

The manuscript already specify that Visp is in the Rhone valley. The exact coordinates are now given.

• l. 671: 'four-time shorter length of the Haslital' compared to which valley?

It is now specified that the comparison is done with the Rhone valley.

• l. 682-683: Differences in real-word and model valley depth certainly are also important.

Yes, the authors completely agree with this statement.

• l. 697: 'from November 2021 through August 2022'.

Done

• l. 713: Please rephrase '... simultaneous for the entire the profile.'

Done

References

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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.