Answers to the reviewer 1 comments on

"Comparison of temperature and wind between ground-based remote sensing observations and NWP model profiles in complex topography: the Meiringen campaign"

First of all, we would like to thank the reviewer for the valuable, in-depth comments to our manuscript. We would also like to apologize for the numerous typos, wrong links to figures and not complete references that have sorrily lengthened the review process. As supposed, the manuscript was written very rapidly and the latex implementation to the AMT formal leads to unexpected problems. Second, the appendix was really designed as a supplement but just not submitted in a separate file. The revised version produces now two distinct files. Finally, according to both reviewers' request, the manuscript was largely shortened (35 pages instead of 42) and contains now only 12 figures.

The answers to the comments and questions are written in italic thereafter. When modifications of the manuscript are cited, the numbering of the figures correspond to the one of the new manuscript. The explanations themselves cite the numbering of the figures in the submitted manuscript in accordance to the lines' numbers of the comments.

Reviewer 1:

General comments

While the research framework is well-designed and current literature well referenced, the manuscript is apparently written too quickly (missing references, typos, erroneous labels in figures, etc.) and has not yet reached the minimum quality standard for publication. Notably, the manuscript is very long (53 pages, too many considering that it is not a review paper) and more synthesis is required. Indeed, there is a lot of interesting scientific material, but too much detail is to the detriment of the overall focus of the paper and the reader's attention.

I suggest that the authors revise the form of the manuscript and try to shorten it. The (10!) appendices could be transformed into a Supplement, where also a part of the text could be moved. More suggestions are provided hereafter.

We apologize for the numerous typos in the first version of the manuscript and hope to have corrected all of them. We revised and reorganized the manuscript in order to synthesize the results and discussion sections and to shorten the text. The appendix was also shortened and transformed into a supplement.

Specific comments

- Title: temperature and wind could be cited in the title, as they represent useful keywords

  This is a very good suggestion. The title was modified to "Comparison of temperature and wind between ground-based remote sensing observations and NWP model profiles in complex topography: the Meiringen campaign"

- Introduction: multiple sub-sections in the introduction are a bit unusual. I would suggest that the authors shorten this section and limit the information to the most relevant topics for the paper

  The sub-sections were removed and the introduction was shortened.

- Sect. 1.2: here the authors should better highlight that the analysis of the model "performances" does not focus on the forecast skills, but on the ability by the model to represent the average general patterns. I also suggest that they replace the word "climatology" (which would need a much longer period) with "statistical analysis" or "average weather patterns" or a similar expression

  It is now specified that the model was only used in analysis mode for monthly averages and some specific events.

  The word climatology is no more used and replaced by seasonality in the titles and by monthly median/average in the text.

- Sect. 2: the authors could move the description of the site (now Sect. 2.2) and the weather situation (now Sect. 2.4) at the beginning to allow the reader to better follow the description of the instruments. I would suggest that the numerical (now Sect. 2.1) and experimental (Sect. 2.3) tools could be brought closer (as 3rd and 4th subsections).

  The experimental section was reshaped with first the description of the station, then the description of the model and finally of the used instruments. The weather situation during the campaign was also shortened and moved as an introducing paragraph at the beginning of the result section.

- Sect. 2.2, l. 134: if these are important geographical features, please include a figure with these references

  The Gadmertal is not an important feature and is not further mentioned in the paper. It is visible on Fig. 1.a but not explicitly mentioned to avoid overloading of the map.

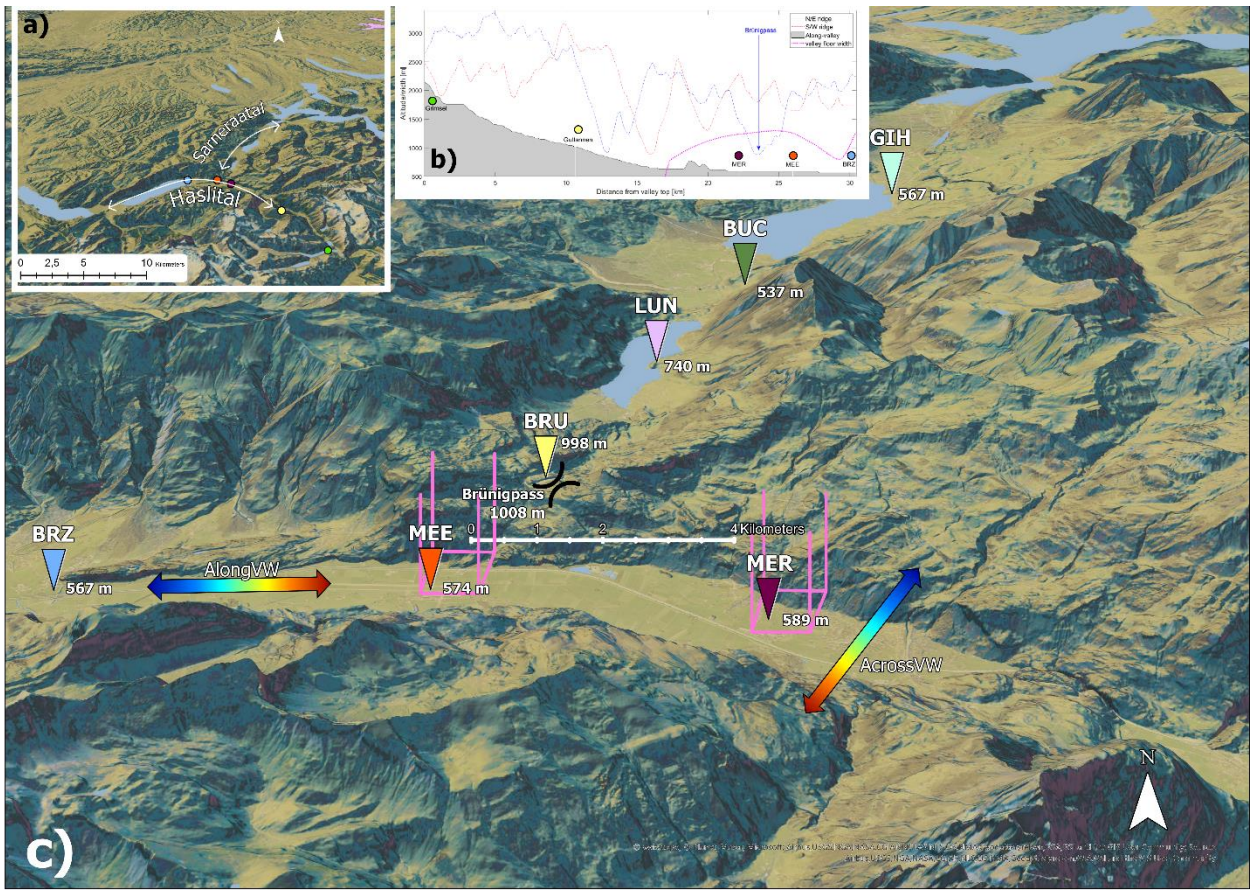
- l. 149-153: is the altitude bias depicted in Fig. 2 only due to inclusion of the slopes in the model cells (as written at l. 149-150) or is this bias due to different DEMs (KENDA-1 and the 25 m elevation model)? In the first case, I cannot explain why the average bias in some cells is far from zero. As a further note, this paragraph could be moved in the model description

Fig. 2 represents the difference between the cell mean altitude and the 25 m digital elevation model at a 25 m resolution. L. 149-153 just mean that cells containing both valley floor and slope have a more important bias than cells situated only in the valley floor. The text was modified: "Data from the two grid cells containing the MER and MEE stations were used. Both cells include part of the valley's north slope, inducing differences of 109 m and 130 m between the real topography and the model's terrain, respectively. The modeled valley floor is globally raised by a hundred meters (see Supplement, Fig. S2), whereas the ridges and the Brünig Pass are lowered with respect to their real altitudes. The altitude difference between the valley floor and the crests is thus reduced of several hundred meters and, in particularly, the Brünig Pass is only 200 m higher than the valley floor."

It was also moved at the end of the model section. Moreover, in order to shorten the manuscript, Fig. 2 was moved to the supplement.

- Fig. 1: a map over a wider area, such as the one in Fig. B1 should be reported in Fig. 1 (e.g., as a subfigure) to help the reader better understand the geography of the valley

Fig. 1 was modified in order to present a broader view of the geography in the region of the Haslital and comprises now a) a map over a wider aera comprising the complete Haslital, both lakes of Brienz and Thun and the Sarneraatal, b) a view of the terrain elevation along the Haslital and the ridges heights and the valley flood width and c) a detailed view of the Haslital and the Sarneraatal including all used stations and the used KENDA-1 cells.



- Sect. 2.3.3: can you shortly explain what the MWR "training" implies?

  The retrieval algorithm employs simulated brightness temperatures at specified frequencies and elevation angles, which are obtained through radiative transfer calculations using Payerne radiosonde data. A multi-linear regression is conducted, establishing a relationship between the forward-modeled brightness temperatures and the atmospheric temperature and humidity measured by the radiosonde at a defined height level. This training was performed by the manufacturer RPG and is described in Lohnert and Maier (2012) based on Crewell and Lohnert (2007) methodology.

  https://amt.copernicus.org/articles/5/1121/2012/amt-5-1121-2012.pdf

  https://ieeexplore.ieee.org/abstract/document/4261043

  S. Crewell and U. Lohnert, "Accuracy of Boundary Layer Temperature Profiles Retrieved With Multifrequency Multiangle Microwave Radiometry," in IEEE Transactions on Geoscience and Remote Sensing, vol. 45, no. 7, pp. 2195-2201, July 2007, doi: 10.1109/TGRS.2006.888434.

  The manuscript was also modified: "During the Meiringen campaign, the retrieval of Payerne was used (Lohnert and Maier, 2012). This retrieval uses Payerne's radiosonde data to perform the multi-linear regression leading to potential further uncertainties."

- Sect. 2.3.4: is the vertical component of the wind velocity used anywhere in the study? Also, there is no mention to a blind (low) zone in the DWL measurements, however the DWL plots start higher than ground level. For the same reason, I guess that the T inversion is not detectable from the wind fields (DWL)? Is it possible to detect any turbulent mixing phenomenon (e.g., development of PBL) at the bottom of the valley from DWL, overlapping to the slope/valley circulation?

  The vertical component of the wind velocity is used to calculate the horizontal wind speed with the DBS algorithm. The blind zone is of 200 m and the manuscript specify the limits of the DWL measurements from 200 to 12000 m a.g.l. The altitude of the DWL first level is now mentioned explicitly and correspond to the altitude of MEE stations (574 m) plus the depth of the blind zone (200 m).

  The DWL derives wind direction and speed from aerosol backscattering observations. It is consequently not able to measure T profiles and to detect T inversions.

  The development of the PBL can be estimate not only by the DWL but also by the MWR and the ceilometer. The automatic PBLH detection from the DWL was tested at Payerne, compared with other detection methods and found not to be completely reliable.We did a first analysis of the PBL height from the MWR data at Meiringen leading to interesting results that still have to be compared with the Ceilometer PBL estimates. Even if the inclusion of these analysis could bring further explanatory variables, we estimate that it would lengthen the manuscript so that we did not include these preliminary results in the paper.

  The observation of the overlapping to the slope/valley circulation needs a much more comprehensive setup. A good example is given by the CROSSIN campaign (https://journals.ametsoc.org/view/journals/bams/102/1/BAMS-D-19-0283.1.xml) in the Inn Valley using three DWL performing synchronized continuous coplanar RHI scans. This setup

allowed the retrieval of the two-dimensional cross-valley wind vectors in the scanning plane. The setup during the Meiringen campaign does not allow such a sophisticated analysis.

- Sect. 3: it may be a matter of taste, but as a reader I would be more comfortable if the results were split between "Climatology" and "Case studies" rather than "Temperature" and "Wind" (each one with analyses on both the long- and short-term). That would first provide an overview on how the model performs, then the focus could be on specific episodes

  The paper could also have been organized as proposed. Anyhow, the case studies include one subject only related to T (surface based T inversion), one subject only related to wind (heterogeneity of wind along the Haslital valley) and finally the special case of foehn events where both the T and the wind compounds are important. We prefer then to keep the present structure and we hope that the present modifications have improved the manuscript and make the reading easier.

- Fig. 3: what is the reason for the "erosion" in the temperature field below about 1000 m in May and June at the end of the day?

  We do not observe a peculiar erosion in the T field below 1000 m in May and June at the end of the day. Anyhow, the maximum of the color scales is adapted for each month (15°C in April, 20°C in May and 24°C in June) leading to shift, e.g., of the yellow color from 8°C in April to 14°C in May and 16°C in June and perhaps giving the impression of a T erosion.

- l. 259: it is stated that the environmental lapse rate correction is "not precise in specific cases". However, it is not even precise on average. More generally, I would remove the whole paragraph at lines 251-264 and just mention that tests using a fixed lapse rate determined that horizontal/vertical distances between the station and the KENDA-1 cell are not the reason of the observed T discrepancies

  We do agree that tests with the environmental lapse rate correction were important to reject the hypothesis of large differences due to the horizontal and vertical distances, and that they are not very relevant for the rest of the study. Following your proposition, the text was largely shortened:

  "To check if the altitude differences between the station and KENDA-1/MER-MEE first levels could explain the T differences with SMN/MER, a standard T correction with a mean environmental lapse rate (-6.5 °C/km (Lute and Abatzoglou, 2021)) close to the mean measured MWR/MEE lapse rate (-4.59 °C/km between 590 and 740 m) was applied to the modeled profiles. Considering the remaining T differences after the correction (grey in Fig 5.b and 5.c), we conclude that the horizontal and vertical distances between the SMN/MER station and the first level of KENDA-1/MEE are not the main causes of discrepancies in ground T estimation."

- l. 264-265: do these differences present a seasonal cycle? Is there a figure similar to Fig. 5 for each month?

  No such figures for each month were done. Anyhow it was shown that T overestimation by KENDA-1 is the largest at low T (< 5°C) and T underestimation the greatest at T>20°C. There is then probably a clear seasonality that follows the climatology of T as well as the T inversion seasonal

cycle (see sect 3.1.3). We do not think that this information would enhance the quality of the paper so that we will not incorporate this potentially new figure in the revised manuscript.

- l. 304: could the difference in the frequencies of T inversion between MWR and ground station be an effect of the surface, i.e. due to the fact that DWL measurements are in the free atmosphere and the station is on the ground?

  This is a very interesting but very complex question. Differences due to the direct influence of the ground could relate to:

  - *The presence of a shallow fog layer that would affect both ground stations without extending to the middle of the valley. Fog usually forms lakes in the valley so that the influence of fog is probably not a reliable potential explanation.*
  - *The moisture state of the ground could also influence T inversions since water is a very good IR emitter. A complete radiative budget should be perfomed in order to make some reliable assumptions.*
  - *Snow has, on the contrary, very good insolate properties. Different snow coverage as a function of altitude would lead to larger daytime heating and nighttime cooling in the valley floor than at higher altitude. Such cases are however not relevant during very cold days as well as during late spring and summer.*
  - *The more simple explanation for large differences between the temperatures at the ground and in the free atmosphere would be nighttime down slope winds that would cool only a few tens of meters above ground.*

  All these explanations are, however, quite speculative and cannot be solved in the framework of the Meiringen campaign.

- l. 309: in March and April, the differences between MWR and KENDA-1 are not "just above" the T inversion

  The sentence at L 309 ("All this leads to both an important overestimation of the T at ground level (Fig. 5) and a slight underestimation of the T just above the T inversion (Fig. 6).") aims to underline that KENDA-1 overestimates T at ground level (Fig. 5 is the comparison between KENDA-1/MER and SMN/MER) but slightly underestimates T above the T inversion, as can be seen in the profiles of Fig. 6. This behavior is obvious if individual T profiles are analyzed (Fig. S20 et S21). Anyhow we do agree that the T underestimation by KENDA-1 is not restricted to the altitudes just above the T inversion but to the greatest part of the profiles between the first MWR level (625 m) and 1500 m. To avoid any confusion, the manuscript was modified: "The missed T inversions by KENDA-1/MEE leads to both its important overestimation of the T at ground level (Fig. 4) and its slight T underestimation between ~850-1200 m (see Supplement Fig. S5 for detailed examples)."

- Sect. 3.1.5: this section is very short and maybe not too relevant. Can it be removed?

  Since this result is effectively not relevant for the rest of the study, it was removed.

- Sects. 3.2.1, l. 341-342: this classification method sounds a bit naive, and some similarity between the w<20 km/h and w>20 km/h diagrams (Fig. 8) are visible, i.e. no clear boundaries are found between synoptic and thermal circulations. Could the authors further elaborate on that?

We do agree that this classification method relay on the arbitrary threshold of 20 km/h to separate synoptic winds from thermal valley winds. A separation between wind speeds smaller 10 km/h, wind speeds comprised between 10 and 20 km/h and wind speeds higher than 20 km/h clearly showed that valley winds are already observed for speed < 10 km/h but are much better defined if speed until 20 km/h are considered, whereas different clear features are visible for speed > 20 km/h. The maximum up valley speed measured in the alps (30-35 km/h see Schmid et al., 2020, Adler et al., 2021 and Giovannini et al., 2017)) in case of clear sky conditions leads to lower monthly median speeds for all weather conditions, i.e. no selection of days favoring thermal valley winds. The foehn have usually high wind speeds reaching 70-140 km/h (https://www.meteoswiss.admin.ch/weather/weather-and-climate-from-a-to-z/foehn.html) and Bise events also have speed > 40-50 km/h over the Swiss plateau. The westerlies are usually less strong and they can be observed in the categories with speed < 20 km/h. However, the main purpose was to exclude strong foehn events from thermal winds. This goal is reached since thermal valley winds can be observed in March even if this month is the most strongly affected by foehn events.

- l. 396-397: is the difference between 3.5 and 4 hours significant (and relevant)? Also, is the +/-1h offset described at l. 399-400 significant?

  We do agree that these differences are not significant. The whole paragraph was modified:" Fig. 7.a shows the diurnal and seasonal cycles of the along valley wind speed at SMN/MER during the campaign. The occurrence of along valley winds is confirmed by the diurnal cycle in November and from February to August. A 3-4 hours delay between sunrise and the onset of up valley winds (> 10 km/h) is observed. February shows some early up valley wind, but their origin is more linked to synoptic flow intrusions. The transition to down valley winds occurs 1 hours before sunset in March and June and around sunset otherwise."

- l. 453: the wind is defined as "Up valley", but has E/SE direction

  This is a typo. The right time of up-valley wind measured by DWL/MEE is on the 10 of July at 13:00-14:00, namely the second reported time in the sentence. The sentence was corrected: "At the DWL first level (200 m a.g.l.), up valley wind is only measured in DWL/MEE on the 10 July at 13.00-14:00 (Fig. 10.a, color bar). The wind direction switches thereafter to N and the wind speed increase gradually to reach 40 km/h at 20:00."

- l. 457: "only observed between 1300 and 2000 m", but it looks like there is a positive along-valley at 800 m in Fig. 10

  Fig. 10 refers to the monthly median wind speed whereas section 3.2.4 analyzes three clear-sky days in July 2022. The result described in L. 457 is not shown since we did not report another figure with complete DWL profiles for these selected days. We mentioned now in the manuscript that this result is not presented.

- l. 464-470: a "3D" figure with the winds depicted as arrows would be very beneficial for the readers not familiar with the Swiss geography

The wind regime is constantly changing in the Haslital valley during these three days, so that no clear and explicit figure with the various configuration of the wind could be produced. We did a skitch of a small film with the wind flows during several days that can however not be included in such a paper.

- l. 478: "from NE", this seems to contradict Fig. 8b ("green colour" of the wind direction during the night)

  As in L 457, L 478 refers to the in-deep analysis of three days in July and not to the monthly median directions presented in Fig. 8b.

- l. 578-584: is the presence of a lake really discussed in the study? Can you better explain why the model would not be able to deal with a lake?

  This paragraph does not explicitly mention known problems of models in complex topography but aimed to describe the particularities of the Haslital valley. These peculiarities could explain differences between the results in MEE and in MER as well as different thermal wind behavior than reported in other studies as mentioned at the end of the last sentence.

- l. 596: "equally common", is this the case? The last sentence of Sect. 4.2.1 lets me think the opposite

  This sentence describes the results of Joly and Richard (2019) and not the present study, where T inversions are less frequent in summer than in winter.

- l. 602: is the "thickness" of the T inversion analysed here?

  Yes. It is now better specified in the manuscript:" The intensity, magnitude and thickness of these surface T inversions follow a similar seasonal pattern as observed in the Haslital."

- Sect. 4.3: can qualitative concepts such as "accurate results" (l. 675), "large modeling errors" (l. 677), "satisfactory" (l. 686), "very good performance" and "well modeled" (l. 722), etc. be quantified in a more precise way, i.e. based on some performance targets.

  We have reformulated the mentioned text and put the results of this study into perspective of the standard verification against radiosondes and surface observations regularly done at MeteoSwiss. This verification is done averaged over the whole domain and comprises complex and non-complex terrain.

- Conclusions should be better synthesized. So many "bullets" and detail are not common in the conclusions and do not help the reader get the overall idea of the outcomes.

  The conclusion has also been extensively reworked. Only the main conclusions remain, results of observations and about Kenda-1 performance are now grouped for T and wind and all the bullets were removed. We hope that the most important results are now easier to grasp.

Technical remarks

- Please, read the "Manuscript composition" guidelines (https://www.atmospheric-chemistry-and-physics.net/submission.html) and the LaTeX template, if necessary. Correct the bibliographic references, section/table/appendix/figure references, date formats, etc.

  See general answer to the reviewers' comments. We corrected all the mentioned types of typos.

- l. 2: why do you cite the "mesoscale" and not the local scale?

  The manuscript was modified: "Thermally driven valley winds and near-surface air temperature inversions are common over complex topography and have a significant impact on the local and mesoscale weather situation."

- l. 3: "it" or "them"? Vertical exchange is explained here, but horizontal transport should also be mentioned (also at lines 21-22)

  The manuscript was modified:

  L3: "Valley winds affect them by favoring horizontal transport and exchange between the boundary layer and the free troposphere, whereas temperature inversion concentrates pollutants in cold stable surface layers."

  L21-22:" Over mountainous areas, interactions between the terrain and the overlying atmosphere favor the horizontal and vertical transports of moisture and pollutants and consequently increase the air masses exchanges along the valleys and between the boundary layer and the free troposphere."

- l. 8: "measurement's" --> "measurement"

  Done

- l. 9: "influences"

  Done

- l. 16: "too"

  Done

- l. 22: bibliographic references can be added (e.g., anticipated from the next lines) after "troposphere"

  References to de Wekker et al, 2015 and Rotach et al., 2022, two papers describing general phenomena leading to exchanges with the troposphere in complex terrain, were added.

- l. 31: "air T" --> "air temperature (T)"

Done

- l. 51: "slope-flow-induced local subsidence", please rephrase

  The sentence was changed:" They can partially be explained by the topographic amplification factor concept (Whiteman, 1990) and local subsidence in the valley center induced by upslope-flow (Schmidli and Rotumno, 2010) leading to a faster heating of the valley than of the plain."

- l. 121: differences between Schraff et al. (2016) and the "current setup" are not clear

  We have described the main differences in the text: " Differences to the setup described in \cite{schraff_2016} include the modeling domain (central Europe covering the Alpine Arc), the grid size of 1.1km and the observation errors tuned to the MeteoSwiss setup."

- l. 132: "upper (southern)"

  Done

- l. 134: does the valley with 1.5 km wide floor continue towards NE or NW?

  Right, NW direction. Done

- l. 143: "provide from", check syntax

  Done : « The campaign provides in-situ observations from the automatic Swiss Measurement Network SwissMetNet (SMN) station at MER and REM observations from MEE facing the Brünig Pass. «

- l. 161: add one sentence explaining why even though some measurements (ground and remote sensing elsewhere) are assimilated into the model, a later comparison of the model to the same type of measurements still makes sense

  One of the reasons to compare observed and modeled data at MER is that the MER ground observations are only assimilated if the difference with the modeled data is inside a given threshold. Consequently, the next sentence was modified: "Anyhow, the observations considered as too far from the modeled data are rejected during the assimilation phase, so that a comparison between the observed and modeled data at MER allows making assumption on the models' skills. "

  Anyhow, even if the observations were always assimilated, it doesn't ensure to have the same observed and modeled data. Therefore a further sentence was added at the beginning of the results' section to further emphasize this point: "Even if SNM/MER surface observations are assimilated by KENDA-1, the comparison of the modeled and observed data at MER and MEE allows evaluating the impact of the assimilation at MER."

  l. 177-178: check syntax

The sentence was modified: «During the Meiringen campaign the training dataset of Payerne on the Swiss plateau was used, leading to potential further bias. «

- l. 227: can you add the mean ridge height to the plot as horizontal line?

  The mean ridge height was added to Fig. 3 and 6 that are now merged into Fig. 2.

- l. 228: it is not simple to understand where the "T rise shortly at sunset" is visible in Fig. 3

  It is principally visible in April and to a lesser extent in May, but it does not correspond to a main feature and does not lead to an interesting conclusion. We decided then to delete this sentence to shorten the paper and improve the streamline.

- l. 232: there is no figure D1b (also at l. 234). What are "standard" values?

  Yes, both positive and negative gradients refer to the same figure (D1). This was correct in the manuscript. Lute and Abatzoglou (2021) did not present standard values for evening T gradient, so that we delete this part of the sentence.

- l. 233: "(Fig. 3a) near the ground (590-1000 m) for all months..."

  Done

- l. 235: "apart" or "onwards"? Please, correct all references

  Done here and in the whole manuscript

- Fig. 3: explain the dashed vertical lines in the caption. Change the x marks to, e.g., 6h or submultiple of 24h. Is Fig. 3b mentioned anywhere in the text? Maybe it should be moved next to Fig. 6?

  As explained now in the figure caption, the dashed lines correspond to sunrise and sunset. Figure 3b was removed and replaced by Fig. 6.

- l. 251: "station altitude" instead of "real topography"

  Done

- l. 257: what does "difference in the effect" stand for?

  There is effectively not much difference in the effect of the ELR correction at both stations so that this sentence was removed.

- Fig. 4: the dashed vertical lines should be explained in the figure caption

  Yes, it's done: "The dotted and dashed lines correspond to the median and the mean, respectively."

- Fig. 5: T differences between ... and SMN/MER" (subtraction is non-commutative). Also, is "MERn" a typo?

  The use of "different" in this context if a French mistake. In French, "different" can signify "various" and has nothing to do with a subtraction. The world is not necessary and was suppressed. MERn is a typo.

- l. 279: explain why "705" m

  The two mentioned altitudes are now described: "The T profile comparisons describes the differences between the MWR/MEE retrieved T profiles and KENDA-1/MEE modelled T between the first level of the model at 705 m and the top of the good MWR measurements at 2500 m."

- l. 284: correct missing reference ("??") and similar occurrences

  All the references were checked and corrected.

- l. 289: "presents slightly"

  Done

- l. 294: "shows"

  The subject of this verb is plural, so that the third person singular cannot be applied.

- Fig. 7b: check sign of deltaT. Is it T_SMN - T_MER (>0) or the opposite (as in the main text)? Use submultiples of 24h on the time axis. Mention why there are data gaps

  Delta T refers to differences between the ground T and the minimal T of the inversion. They are therefore computed always from the same type of observations.

  Multiples of 6h are now used. There is no MWR data from November to January and in August since the MWR was installed current January and not measuring during whole August as stated in the experimental part. No data are represented in Fig. 7b (presently Fig. 5b), when no T inversions were observed.

- l. 319: "or not of the values" please check this sentence

  This sentence was modified and merged with the previous one:" During these nights, differences between the model's first guess and observations are mainly around 5 °C and can reach 10 °C in extreme cases (results not shown), so that observations are rejected due to differences exceeding the predefined threshold based on the ensemble first guess, its spread and the observation error."

- l. 326: what is a "Bise" wind?

  Bise is a cold dry northern wind in Switzerland. This is now specified in the text.

- l. 349: "1500 m", is this altitude limit so clear?

  No, this limit is not clear, moreover if we consider that the E winds can provide from both down-valley and foehn winds. The reference to the vertical extent was removed.

- l. 355: "between 1300 and 1700 m", not clear from the figure

  Yes, we do agree that the contribution from N wind is not clearly visible in this plot, so that the sentence was removed.

- Fig. 8: how do you deal with direction in calm wind conditions? Why are the white boxes (data gaps) similar for all sources? Clearly write the difference between plots b) and c) even in the figure, not only in the caption

  All the analysis of wind directions were only performed on observations with wind speeds>2 km/h to avoid overinterpretation of the direction during calm winds. This is now mentioned in the experimental section.

  White boxes are similar in observations and KENDA-1 data because the modeled data were restricted to time with observations to allow a comparison between the measurements and the model. The indication of the type of data and the site are now also given in the figure.

- Fig. 9: explain what positive and negative speeds mean in the caption. Clearly state what interpolation/smoothing technique was used in the contour plots

  Positive speeds correspond to up valley wind as specified in the color of the arrows in Fig. 1. This information is now given under 3.2.2 in the manuscript: "For this analysis, the positive speeds correspond to up valley wind (see Fig. 1 and Fig 1B) and to northern wind from the Brünig Pass (see Fig. 1B) for along and across valley winds, respectively."

  The method to interpolate is now clearly station in the figure caption: "a) Diurnal cycle of the hourly T inversion frequency between T at SMN/MER (589 m) and FEDRO/BRU (998 m) ground stations, at the lowest level (640 and 705 m, respectively) and 1000 m of MWR/MEE and KENDA-1/MEE profiles. The 1_D measured values were interpolated using a linear interpolation with 10 m spaced vectors. b) Mean ΔT for the time where an inversion is detected. Sunrise and sunset are represented by dotted lines."

- l. 403-404: is 800 m "ground level"?

  800 m corresponds to the DWL first level. 800 m was however falsely reported since the first level is 200 a.g.l. corresponding to 775 m in MEE. This altitude was modified in the whole manuscript.

- l. 405: "reduced" compared to what?

  The reduced wind speeds are compared to the ground SMN/MER observations. This is now stated in the manuscript:" The onset of the up valley winds occurs with the same delay to sunrise (~4 h)

during the summer months but their speed is of reduced maximum amplitude (10-15 km/h) than at SMN/MER"

- l. 408: "occurred"

  Done

- l. 416: "ridge height"

  Done

- Fig. 10: y label should be "DWL/MEE" instead of "DWL/MER"

  Done

- l. 453: "reported at 200 m", explain why in the instrument description

  The fact that the DWL first level is at 200 m a.g.l. is now explained in the experimental section. The up valley wind is only measured in DWL/MEE on the 10 July not because of an instrumental problem, but because other types of winds are measured during the rest of the chosen period.

- Fig. 11: add "FEB"

  Done

- l. 467-470: unclear, please rephrase. Maybe clearly state that MER is more sheltered from the N wind than MEE? Can BRZ be influenced by katabatic nighttime winds from the slope?

  The sentence was simplified to make a clear causality between the wind observed at the Brünig Pass and its influence at MEE. The next sentences were also slightly modified to underline that the wind from the Brünig pass largely influence the diurnal cycle of the wind direction at BRZ but only slightly at MER:" These winds from the Brünig Pass explains first the N wind observed in MEE during the afternoon, the early evening and even sometimes in the morning (e.g. on the 11 of July). Second, they also strongly influence the diurnal cycle at BRZ leading to the onset of down valley winds in the early afternoon or even by suppressing up valley winds (July, 11). Finally, their influence at MER is however weak with only a slight shift of the wind direction towards N in the late afternoon."

  The winds from the Brünig appear already in the morning during these clear and warm days in July. Since a complete T analysis at all the ground stations were not performed, these down slope winds are not necessarily katabatic, but could be qualified of drainage wind following the AMS glossary of Meterology. But a complete analysis of the causes of the diurnal cycle of the down slope winds from the Brünig is beyond the scope of this paper.

- Fig. 12: is BRZ really representative of thermal circulation? Also, use correct colours in the legend of Fig. 12b. Add the indication "(orange)" after "sunshine amount" in the caption

Fig. 12 and the related section now entitled "Heterogenity of wind patterns in the Haslital valley" aims to highlight that the standard thermal valley wind pattern is not spatially homogeneous even at distances of some km. In that sense, BRZ is not representative of a standard thermal valley circulation (similarly to MER) but its wind pattern is anyhow produced by thermal effect in the complex topography of the Haslital valley.

The colors of the legend of Fig 12 b were corrected and the indication that the sunshine amount is given in orange was added.

- l. 486-487: I cannot fully understand the meaning of this sentence

  This sentence notes that the KENDA-1/MEE grid cell comprises part of the slope leading to the Brünig Pass and should consequently monitor slope winds better than the DWL/MEE situated in the middle of the valley. Anyhow this was already mentioned at several places in the manuscript and brings no important information here. The sentence was then deleted to shorten the manuscript.

- l. 490: "up to 1500 m during all other months", not so clear in summer. The N component up to 2500 m is difficult to separate

  Yes, this sentence aims to describe only the strongest N winds with speed > 20 km/h. The manuscript was modified to highlight that N wind can also extend up to 2500 m, but with a weaker speed.

- l. 503: "Foehn is"

  Done

- l. 512: "measured wind speed"

  Done

- l. 516: "better agreement ... (not shown)", are your referring to the difference between the two red boxes in Fig. 14b? Is this difference significant?

  We do agree that this difference is not clearly significant if we refer to both Fig. 14 a and b. The sentence was removed.

- Fig. 14: explain the number row at the top of the plots. Correct the intervals on the x axis in Fig. 14b. Correct order of factors in caption (difference should be with reference to SMN/MER)

  The numbers are now explained in the figure caption: "and n is the number of cases in each of the categories". The intervals on the x axis and the legend were also corrected.

- l. 521: "(mettre...)", correct typo

  Done

- l. 549: why do you mention three different foehn types, when the selected cases are all of deep foehn?

  We mentioned them because they exist. However, since only one type of foehn was measured, this description was removed.

- Fig. 15: correct x labels for 2nd and 3rd columns

  Labels were corrected.

- l. 558-560: this sentence is a bit general. Maybe rephrase so that it can be understood that the focus is on "atmosphere over complex topography"

  This quite general sentence is now more focused on the particularities of the modelling of MoBL and was move under 4 as an introduction to the discussion section:" Complex topography, landscape heterogeneity and specific thermal wind regimes challenge the models' spatial and temporal resolutions, their performances in data assimilation and the parametrization of multi-scale processes."

- l. 560-561: isn't it the same over flat terrain?

  Yes it is also the case in heterogeneous terrain and various surface types. The two first sentences of 4.1 were modified and used as an introduction to the discussion section (see also previous remark).

- l. 569: "As such", please check syntax

  The expression was modified to "In this regard,…"

- l. 570-576: most of it has already been written. Please, shorten this part

  This paragraph was shortened as recommended: "Moreover, the model cell over MEE overlaps the slope towards the Brünig Pass, so that KENDA-1/MEE reports an average of winds from the Brünig Pass and in the Haslital. DWL/MEE, on the other hand, only observes winds in the middle of the Haslital. Consequently, the differences between the modeled T/wind averaged values and the observations cannot be only considered as model errors."

- Sect. 4: use the space here to anticipate the structure of the discussion, especially Sects. 4.2 and 4.3

  A first paragraph now summarizes the challenges of MoBL in complex terrain and describes the discussed points: "Complex topography, landscape heterogeneity and specific thermal wind regimes challenge the models' spatial and temporal resolutions, their performances in data assimilation and the parameterization of multi-scale processes. The discussion will consequently focus on three points, the specificity of the terrain around the campaign site, the comparison of the observed wind and T profiles with previous observations in the Alps and the model performances in Meiringen."

- l. 585-589: this can be shortened by writing that the forecast skills were not the focus of the paper

  The § was shorten as suggested: "Finally, this study is based on monthly median values, so that the averaging artifacts have to be considered, e.g. for the analysis of maximum wind speed, the onset time of valley wind or wind directions. In that sense, this analysis focused on climatology and not on the forecast skills of COSMO-1."

- l. 596: "pairs", do you mean at different altitudes?

  Yes, it is now specified in the text

- l. 607: "months"

  Done

- Sect. 4.2.2: too much detail. Do not repeat all results, just mention the most important similarities and differences

  The section 4.2.2 was largely shortened, mostly the second § containing the largest number of repetitions about the results in the Haslital.

- l. 684: "other study" by whom?

  This not published study was performed by one of the co-author, D. Leuenberger. It is now specified in the manuscript.

- l. 722: are winds "well modeled" despite "the onset is predicted with a larger inaccuracy" (l. 724)? Does the sentence only stand for the wind velocity?

  Both the qualification of « very good performance" and "larger inaccuracy" are overrated and have been removed since they are not based on any similar performance indicators.

- Table A1: is the last column a % of a %? Write the year the anomaly refers to in the caption

  Yes, the penultimate column is the percentage of maximum sunshine and the last column th corresponding percentage of the norm. The years considered for the climatological norm (1991-2020) are already given in the table caption.

- Figure A1: a y label for the sunshine duration is missing

  Done

- Figure B1: Explain the (black and coloured) arrows in the figure. Caption: "downloaded"

  Fig B1 was removed. The arrows are explained in Fig. 1 that takes over part of the information of Fig. B1 and B2

- Figure B2: red/blue lines are too thin

  Fig. B2 is now part of Fig. 1 and the ridges' heights are not larger.

- Appendix D, E, F, G, H, I, J: titles should precede the figures

  The appendix was converted into a supplement and no titles subsist.

- Figures E1 and E2: y axis label (and measurement unit) is missing. Explain the two lines of plots

  Fig E2 was removed. Y label and unit are now reported. The lines of the plots are already explained in the figure caption.

- Figure F1: add a legend for the purple, blue and red lines. Where are the dashed lines mentioned in the caption?

  Fig. F1 was removed.

- Figure H1: "sunrise"

  The dashed lines are now described in the figure caption.

- Figure I1: the second colour key should report "°", not "°C"

  Done