

## Answers to the reviewer 2 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 sorrowfully 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.

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#### 1 General comments

##### General state of the manuscript

Generally, the research results in the manuscript are well presented, however, the overall structure as well as the overall "state" of the manuscript should be improved. For example, (i) citations are frequently not correctly used (e.g., double brackets, missing references, incorrect citation style), (ii) references to Sections and Figures are frequently missing, (iii) the presentation of Figures could be improved, and (iv) the Appendix should be substantially shortened to only include the additional information that is absolutely necessary for the manuscript (see also General Comment 4). Moreover, please check if all abbreviations are correctly introduced when first mentioned (e.g. "T" for temperature is not introduced, l. 31, p. 2). Thus, while I find the content of the manuscript interesting, the manuscript needs further polishing prior to publication.

*We apologize for the numerous typos. Citations are now correctly done, abbreviations and references to sections and figures were checked. Figures were improved, the appendix were transformed to a supplement and both the manuscript and the supplement were shortened.*

#### 2 Structure

1 The overall structure of the manuscript with introduction, methods, results, etc. is good, however, in my opinion the Results Section is missing a coherent storyline. I would suggest to re-structure and

streamline this section with a strong focus on relevant synoptic features and important differences/agreements between observations and the analysis. Some specific suggestions for potential improvements are listed below. Generally, the manuscript is (unnecessarily) long (in total more than 50 pages), and focusing on a coherent storyline will likely help to shorten the manuscript and convey the key results in a concise way.

*The structure of the manuscript was revised and modified to improve the streamline and to shorten the paper .*

### 3 Figures

Many figures contain a large number of panels and show the results from the analysis and the observations. In my opinion, the figures should be optimized (i) by minimizing white space between panels and (ii) by showing the result from the analysis or observation and directly the difference between both as sub-panels. This would help to combine the synoptic conditions and associated errors and remove redundancies when analysis and observations are very similar. It also emphasizes differences between observations and analysis. The manuscript includes a relatively large number of figures. I believe that the figure number could be reduced by carefully selecting the relevant ones and combining figures.

*Figures were improved and their number in the manuscript was reduced to 12 and some of them were moved to the supplement.*

*Figures of the differences between the observation and the model are useful for the T and wind speed analysis (see Fig. 6). They are however more difficult to interpret in the case of monthly medians of wind direction. Moreover, the analysis of the wind direction differences between MEE and MER is complex due to the bending of the valley between both sites. We estimated then that the present figures are more adapted to the wind analysis.*

### 4 Appendix

The appendix includes too many figures. I would ask the authors to carefully select only those of primary relevance for the manuscript. Moreover, similarly to General Comment 3, the information content of many figures can probably be condensed to fewer figures. E.g., Figures 1, B1, B2, and G1 all show a map of the measurements sites. I suggest their content can be summarized in 1-2 panels.

*Some figures of the supplements were also removed or condensed (e.g. Fig. 1, B1, B2 and G1 about the topography) . Other figures were moved from the paper to the supplements.*

### 5 Consistency

I would ask the authors to double-check the consistency of used abbreviations and naming conventions. E.g., it is explicitly stated that data are presented with instrumentation/site, however, this is often not consistently applied (in particular in Results Section). Moreover, several different data sets and sites are compared with each other. When overestimations / underestimations are mentioned, please check that it is specified which data/site are compared.

*A scrupulously proofreading of the second version of the manuscript hopefully corrected all the mentioned typos, including also reference to figures and sections and full and complete references.*

## 2 Specific comments

### 1. Introduction

The introduction is well written, cites relevant literature, and the goals of the study are clearly presented. As a minor adjustment, I would suggest to remove the sub-sections in the introduction.

*The subsections were removed and, as requested by the first referee, the introduction was shortened without removing important notions relevant for the study.*

### 2. Methods

In the results, bias and errors of the analysis comparison to MWR are shown. How strongly does this result depend on the quality of trained retrieval algorithm? Is it possible that the MWR measurements itself are biased? I would appreciate if the authors could comment on the error magnitude of MWR-retrieved temperature profiles and relate this to the shown bias and error magnitudes compared to the analysis.

*The MWR bias and errors and their potential effects on the comparison between the model and the observations are already described in sections 2.3.2, 3.1.2 and 4.3.1. We concluded that (sect 4.3.1) "The near overall negative bias can mainly be explained by two factors: first, the MWR is susceptible of errors especially for higher altitudes with RMSE between 1 and 1.5 °C (Liu et al., 2022) and, second, the 620 MWR/MEE has been trained with profiles from Payerne, so that the difference in altitude between both stations (+100 m) and in atmospheric conditions could induce a larger RMSE or even a bias in the MWR measurements. Despite these uncertainties, the T differences up to -3 °C are probably a clear underestimation of KENDA-1 Ts."*

*A closer estimate e.g. of the error induced by the training with the sounding of Payerne would necessitate a complete study including sounding at Meiringen at different periods of the year and with different weather type patterns. As stated in the first version of the manuscript, we were able to perform only three soundings and, sorrily, the weather conditions during that day (constant high-altitude inversion) did not allow to draw a preliminary conclusion regarding the differences between T profile over PAY and over MEE. Moreover, the MWR was not yet measuring by that time. Several reasons impede further RS observations at MEE during the campaign.*

3. I appreciate the 3D map (Fig. 1), however, it would suggest to use the "northing". Moreover, it is very similar to Fig. B1, although B1 contains some added useful information. I would suggest to replace Fig. 1 by Fig. B1, and also include some information from Fig. G1 (specifically, I would find it a lot easier if wind arrows would depict the median wind direction instead of coloring the stations accordingly).

*The corrected manuscript contains only one figure with all necessary information and the northing view. The colors of the stations on Fig. G1 did not correspond to wind directions but were just added to allow an easier description in the text. The stations are now depicted on Fig. 1 that only concern topography without any wind information. We hope that the colors will no more cause misinterpretation in the corrected manuscript.*

4. The different instrumentation and sites are well described. Due to different durations of employment I would appreciate an overview table of instrumentation, available data, sites, and the measurement period.

*The required table is introduced in the supplement in order not to lengthen the manuscript.*

5. l. 86: COSMO-1E: Please introduce this abbreviation.

*As requested by the first referee, the introduction was shortened so that the abbreviation "COSMO-1E" only appears in the experimental section, where it is introduced.*

6. l. 136 "Five km before the lake": I would suggest to write "Five kilometers".

*Done*

7. l. 186: "perpendicular to the valley (not used in this study)": To streamline the manuscript I would suggest to only describe the measurement setup that was actually applied during the campaign.

*This is a wise advice. We suppressed the description of the not used scanning mode.*

8. l. 197: "These precipitations arrived in form of snow": I'm not sure if precipitation is commonly used in plural.

*Changed in singular*

9. Section 2.4

I appreciate a description of the weather during the campaign, although I do not fully understand why the authors focus on precipitation, snow, and sunshine duration while the focus of the study is on wind and temperature profiles and circulation features. I think this section could more strongly focus on the relevant aspects for the analysis.

*The description of the weather during the campaign was largely shortened. First the T features are summarized. Then a sentence explains the importance of snow cover and precipitation before to summarize the precipitation patterns during the campaign. Finally, a sentence was also added to inform that the wind features during the campaign are described in the results section. As suggested the explanation of the weather situation no more constitutes an individual section but was added as a heading to the results' section: "During the campaign, the mean T was ~1°C below the 1991-2000 norm in December and January but clearly above the norm (1.5 to 2.5°C) in February, March and from May to August. Three heat waves occurred, the first one lasting 6 days in mid-June, the second lasting 4 days around mid-July and the third one reached Switzerland at the beginning of August. Snow cover and precipitation are important parameters since the surface albedo and the soil moisture affect the development of cold pool with T inversion, subsidence, the atmospheric boundary layer development and consequently thermal valley winds. Only 60% of the precipitation of the 1991-2000 norm were observed in November, but 120% in December. Snow covers the valley's floor from the end of November to mid-December. Heavy precipitation reduced then the snow cover to less than 15 cm until the end of the winter. Strong precipitation deficits happened in January and especially in March (35 and 15 mm). March experienced frequent foehn events (95 hr determined from the MeteoSwiss foehn index (Dürr, 2008)). Precipitation from May to August was 50% or less compared to the norm, except for June (96%). The full evolution of T, precipitation and sunshine duration is aggregated in the Supplement (Tab. S2 and Fig. S3) and the wind features are fully described in the results section."*

10. Section 2.5

I believe that this section is not necessary as a separate section, but the information should be included in the previous paragraphs, e.g., where the sites, instrumentation, KENDA, etc. are described, respectively.

*The subsection 2.5 was deleted and the related information dispatched at the beginning of the section or in the subsections.*

11. I. 590: "Therefore, this study does not allow to make prediction of model performance for forecasting." Can the authors please elaborate on this, I do not fully follow the reasoning and relation to forecasts here.

*We simply wanted to highlight first the potential artifacts and bias bounded to the used of monthly medians, second the focus of this analysis on climatology and not forecasting skills of the COSMO-1E. In that sense, a good accordance of median values between the model and the observations does not allow any predictions of COSMO-1E performance as a forecast model. The analysis of special cases such as foehn events underlines this point. Following your remark and a request of the other reviewer, this § was shorten: "Finally, this study is based on monthly median values, so that the averaging artifacts has 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-1E."*

## 12. Results

I would suggest to streamline the Results Section (see also General Comments above).

13. I would suggest to use more informative titles in the Results Section (e.g. "3.1.1 Climatology" is only based on observations, which cannot be deduced from the title).

*The titles were modified to include all necessary information in order to correctly describe the section content.*

14. The overall section structure could be improved. It is not very intuitive to show (i) temperature, (ii) wind, and (iii) Foehn (with wind and temperature), while other atmospheric features were explicitly discussed in (i) and (ii). I would suggest to define a storyline to follow in the Results Section and focus on the key results.

*The structure of the result section was largely modified to have a better storyline. For example, the section on wind comprises now*

*3.2.1 Seasonality of wind profiles at MEE*

*3.2.2 Along valley winds*

*3.2.3 Cross valley winds*

*The foehn is however a very specific meteorological event that is particularly difficult to model. We prefer then to keep the analysis of foehn event as a separated subsection comprising both the T and wind analysis during foehn events.*

15. Generally I would be careful with the word "climatology" as here only a few months and not several years of data are analysed.

*We do agree that the use of "climatology" for a 10 months analysis is partly usurped, so that it was removed in the entire manuscript and replace by monthly values/medians/averages or by seasonality.*

#### 16. Section 3.1.1

I would suggest to streamline this section and focus on relevant features. E.g., the presence of a diurnal cycle and temperature increase after sunset are expected features and their description could be streamlined.

*The structure of section 3.1 was modified and comprise now only three subsections (Seasonality of T profiles at MEE, Surface T comparison and Surface T inversions) and it was also shortened. I hope that these modifications improve the reading and understanding of the results.*

17. l. 230f: Please correct units: temporal gradient of 5C/?.

*Done: it is °C/km*

18. l. 235: I struggle to see the daytime temperature inversions. Could these features be outlined in the figures (e.g., through contours)?

*Daytime T inversions occurs in winter but this results is not visible on Fig. 3a but on Fig. 7a. The sentence was then deleted so that the analysis of surface T inversions is only described in the related section.*

#### 19. Section 3.1.2

Which differences are analysed in this section? Is it hourly values?

*Yes, Figs. 4 and 5 are done with hourly averages from the whole campaign. It is now specified in the figures' caption.*

20. l. 255 f: "The difference in the effect of the ELR correction" Which difference? At both stations RMSE increases? Please clarify.

*As also stated by the first referee, the difference is not obvious. This sentence was deleted.*

#### 21. Section 3.1.3

Would it make sense to place this sub-section before Section 3.1.2 as profiles have already been described Section 3.1.1 (Fig. 6 fits better to Fig. 3)? Is Fig. 3b required? If I'm not mistaken it is not referenced in the text. The comparison of temperature profiles and respective KENDA biases assumes the MWR retrieval does not include a bias. Given some uncertainty in the retrieval algorithm, could the authors comment on this issue (see also comment above)?

*Yes, it makes sense and sections 3.1.1 and 3.1.3 are now merged into one single section. A new figure (Fig. 2 in the corrected manuscript) comprises Fig. 3a and Fig. 6.*

#### 22. Section 3.1.4

l. 308 f: "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)." Both figures compare different data sets, i.e. a direct comparison of temperature differences at different heights is difficult. Moreover, I struggle to see temperature overestimations at the lowest level in MWR-KENDA comparisons in Fig. 5. Can the authors please clarify?

Figure 5 does not directly compare MWR and KENDA-1, but compare MWR and KENDA T at the first level with SMN/MWR T observations. Fig. 5 clearly shows an T overestimation by KENDA-1 during nighttime (positive difference from 20h to 7h) where no averaged difference is found between MWR and the SMN/MWR observations (blue dashed line). We can conclude that KENDA-1 overestimates the ground T at both MEE and MER but not MWR/MEE. The KENDA-1/MEE underestimation from 850 m to ~1200-1500 m is visible in Fig. 3b and occurs mostly during nighttime, even if daytime underestimation is also present. Fig. S5 in the Supplement presents individual MWR/MEE and KENDA-1/MEE profiles allows a better understanding of the described phenomena.

23. I. 309-311: Please either elaborate on this or remove.

Since a systematic analysis of this effect was not done, we chose to remove this result.

24. I. 315 - 322 I find this very interesting and would like to see some results, as this observation rejection is linked to some of the surface temperature differences reported in the study.

We only analyzed visually a period with strong T inversions and large error in the modeled T in March 2022. As you can see from the figures below, COSMO-1E did not assimilate the SMN/MER T due to too large differences reaching up to 10°C in some cases. During the same period, the humidity at 2 m was largely underestimated during nighttime by the model. This is only a first rapid study and a complete analysis of the causes of the model deficiencies in case of T inversion in middle size and narrow valleys would be very interesting and will perhaps be the focus of a next study.

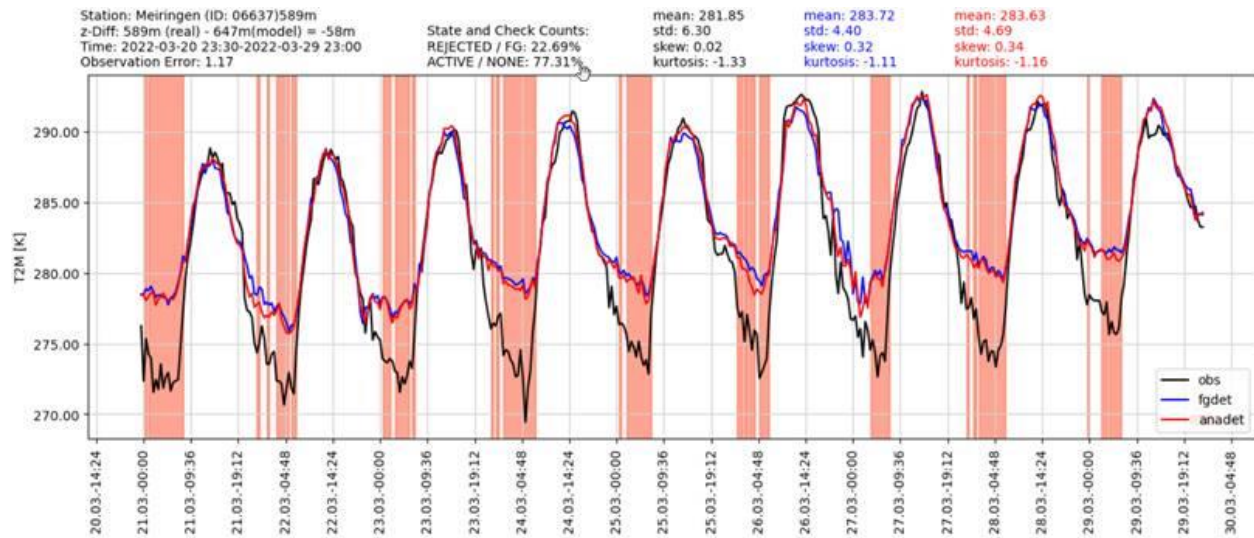


Figure 1: Ground T during the end of March 2022 with SMN/MER observations in black, COSMO-1E first guess in blue and model analysis in red. Red vertical bars denote times when the observations are rejected.

25. Section 3.1.5

Personally, this very short sub-section interrupts the storyline which strongly focuses on MWR and surface measurements. Did the authors also compare MWR profiles with the RS profile? Please note that Fig. F1 is not referenced. Please also elaborate on the influence of the RS from Payerne. Was the additional RS/MEE not assimilated? Please double-check the spelling of radiosounding.

*This result does not bring any relevant explanation for the T and wind results of this study. It was consequently removed. The MWR was not yet measuring in November 2021, so that the comparison was not possible. We cannot elaborate more on the influence of the RS from Payerne on KENDA-1 due to the absence of further radio-sounding measurement in complex terrain. No try to assimilate this isolated radio-sounding was done.*

#### 26. Section 3.2.1

This section appears unfinished and I think it needs some more work. The writing style with bullet points differs substantially from the style used above for temperature. Moreover, I would ask the authors to improve

*As already explained before, the entire result section was reorganized and modified, including section 3.2.1. Bullet points were removed, the redundant content with section 3.2.2 (along valley wind) was suppressed.*

Fig. 8. It is very difficult to identify relevant features in a 60 panel figure. Would it be an option to, e.g., show differences in panel b)?

*As explained as an answer to the general comments about figures, the difference of median monthly wind direction are difficult to interpret so that we will keep the present representation.*

#### 27. Section 3.2.2

This section should be removed.

*The structure of the paper was entirely revised and this title was removed.*

#### 28. Section 3.2.3

I appreciate the comparison of allong valley winds during the campaign period to the 10-year climatology, however, this interrupts the storyline. In my opinion, it would be sufficient to mention the good agreement and move the figure to the appendix.

*To shorten the paper and improve the story line, the climatology of along- alley wind at SMN/MER was moved to the supplement.*

29. Fig. 10a: Typo in axis label: "DWL/MER"

*Done*

#### 30. Section 3.2.4

This section provides a very detailed description of the circulation evolution at different sites. I would ask as the authors to streamline this section and highlight the important circulation features. Figure B1 could also be referred to for clarification.

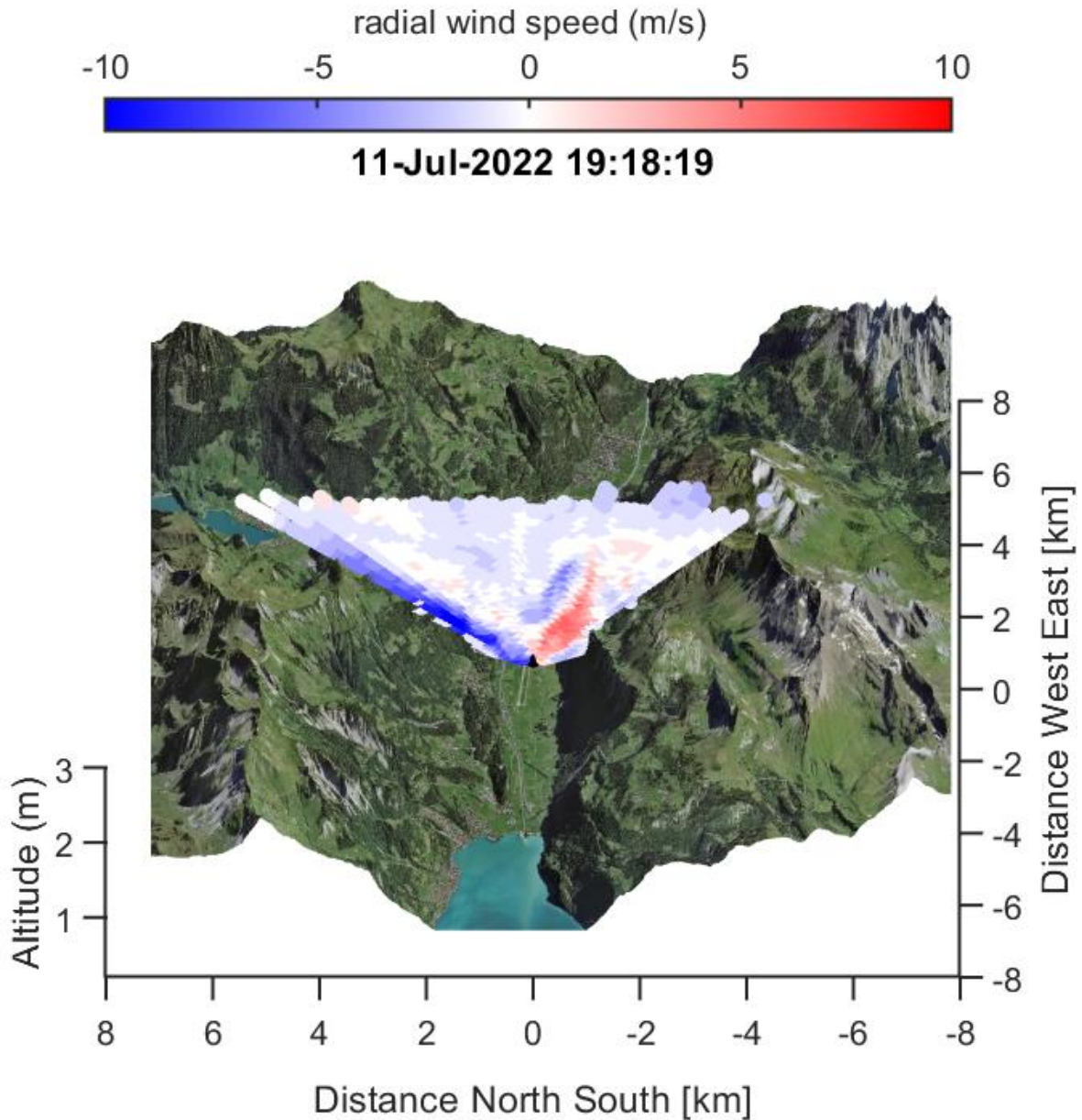
*This section was moved after the along and cross valley description and merged with results about the differences between MEE and MER. Its title is now "Heterogeneity of wind patterns in the Haslital valley". Moreover, the content was simplified and shorten. We hope that these modifications increase the manuscript readiness.*



31. Section 3.2.5

I. 493-495 Could the authors please elaborate on this? How was the vertical velocity estimated?

*The observation of the vertical velocities is part of the DWL scanning procedure, so that these were not estimated but measured by the DWL. In order to better describe this vortex, an example of the radial winds perpendicular to the valley axis was added to the supplement.*



*Fig: DWL/MEE adial speed perpendicular to the valley axis on the 11 July 2022 at 19h18. A Vortex is clearly visible.*

32. Section 3.3

General comment: In my opinion, much of the comparison between KENDA and observations has already been described above. I would suggest to restructure to avoid repetitions and potentially include foehn events as subsections in 3.1 and 3.2.

*As already stated under question 14, we prefer to keep the foehn description as an individual section but the text was revised to avoid unnecessary repetitions.*

33. I. 505: Please define the foehn index and provide a reference.

*The foehn index is mentioned with a reference in the experimental section, so that this information will not be repeated here.*

34. I. 505: Is the subsequent analysis (e.g., Fig. 14) performed for three events only or for "all the period" with foehn? Please clarify.

*The analysis of the T modeled performance is done on all the periods with foehn, whereas the analysis of the wind model performance is done on only the three mentioned cases. The text has been modified for a better comprehension: "Foehn is in a katabatic wind bringing strong warm and dry downdraughts associated generally with clear weather. The study of the T during foehn events combines all the periods where foehn was measured at the SMN station in MER, according to the foehn index. The study on the wind is however performed on only three selected events (10-16.03.2022/19-22.03.2022/23-24.04.2022) representing foehn 117 hours during clear weather in March, while the April and June episodes presented a slightly overcast sky (50-70% of maximum global radiation)."*

35. I. 506: Which April episode? Fig. 15 shows again different time periods? Please clarify.

*There was some incoherence between the text and the figures and some further typos in the x label of Fig. 15. The three foehn episodes represented in Figs. 15, 25, 26 and 27 correspond to the events of the 10-16.3.2022, 19-22.3.2022 and 23-24.4.2022. They comprise then the April event mentioned in the text.*

36. I. 516: I would expect a better agreement of KENDA and observations if both data are taken from the same site (compared to different sites). I would appreciate if the authors could explain the reason for comparison of KENDA/MEE with SMS/MER (instead of KENDA/MEE). Such comparisons are frequently performed throughout the manuscript, and to some extent it is difficult to follow all different comparisons.

*It was not possible to install the REM instruments at MER during the campaign, so that they were put in MEE. The frequent comparisons between MEE and MER are due to this campaign setup comprising ground data with the smallest uncertainties that are measured at SMN/MER whereas the REM profiles were at MEE. The second reason is that, mostly regarding wind profiles, we found marked differences between both sites such as very different thermal valley wind diurnal cycles or wind speed during foehn events. These differences were not expected prior to the campaign but they effectively lead to complex comparison between MEE/MER, ground/profiles and observed/modeled data. Anyhow this complex setup allows having a good representation of the influence of the complex topography on the T and wind patterns.*

37. I. 521: "mettre reference fig ap 26"?

Done

38. l. 537: Does KENDA/MER really show a delay in foehn onset? To me it rather looks like a too early onset (similar to KENDA/MEE)?

*Yes, there is also a too early foehn onset modelled by KENDA-1/MER. The word "delay" was wrongly used here as a time shift that could be positive. The manuscript was corrected: "the same too early onset of the foehn breakthrough is observed ..."*

39. l. 541-544: In particular in this section it is very difficult to see where wind speed is over-/underestimated, i.e., showing differences to DWL might be helpful.

*I think that the difficulties to see wind speed misestimation comes principally from a mistake at L540, where KENDA-1/MER is mentioned instead of KENDA-1/MEE. The text was however also improved: "During the second episode, KENDA-1/MEE models correctly the 3h delay between SMN/MER and DWL/MEE measurement (Fig. 12.b) but extends it up the ridge height contrarily to the measurements. The KENDA-1/MEE wind speeds tend to be overestimated (+15 km/h) from ground to 1100 m during the entire event and underestimated from 1100 m to the ridge's height (-30 km/h) the first hours following the breakthrough. KENDA-1/MER modelled again wind speeds up to 100 km/h with a foehn breakthrough at the same time as the SMN/MER (Fig. 12.c)."*

40. l. 545: Based on Fig. 15 (which is the main figure discussed in this paragraph), I find it difficult to follow the conclusion that the representation at MER is better than MEE. Improved visualization may help.

*We agree that KENDA-1/MER cannot be considered as better than KENDA-1/MEE. The text was modified: "To summarize, the three analyzed events exhibit some similarities but also large differences. The foehn breakthrough is often observed some hours later by DWL/MEE than by SMN/MER and not always simultaneously in the entire profile. The wind speed at the DWL/MEE first level is usually similar to the one at SMN/MER. KENDA-1 tends to model the foehn arrival and end with positive or negative time shifts at both stations. The most critical point concerns the very high KENDA-1/MER modeled speed up to 110 km/h from ground level to 1500 m that is twice faster than the DWL/MEE observation, 5 km further down in the valley."*

41. l. 554: Please elaborate on the link between wind bias, temperature bias and specific humidity bias.

*Relation between moist bias and T are obvious, since warm air is able to contain more humidity and more humid air need more energy to be heated. Anyhow there is no hypothesis about a relation between a moist bias in the model and the wind speed overestimation. This sentence was then deleted.*

42. Fig. 14: Please specify what is shown in the figure. What do the numbers on top represent? What is shown in the x-axis in b)?

*The numbers n on top are the number of cases in each category. This information was added in the figure caption. The x-axis labels corresponding to wind categories were corrected.*

43. Fig. 15: Are the dates correctly shown in all panels? I cannot find any figures for 19-22 March nor 23-24 April. Are data shown only at 11 am and 11 pm?

*No, the dates are not correct and Fig. 15 was revised.*

44. Discussion

The manuscript includes an extensive discussion of the results, which I appreciate. However, I would suggest to condense the information and streamline the Discussion Section. It may also be helpful to include a short summary and/or discussion after the respective Results Sections, respectively.

45. l. 680: The daily cycle of temperature underestimation/overestimation is not apparent in the MER observations (Fig. 6). Could the authors please elaborate on this?

*Fig. 6 allows to compare the observed and modeled profiles at MEE and not the ground T at MER, which is represented in Fig. 5. This sentence refers to the ground T comparison at MER, that presents a diurnal cycle of overestimation during nighttime due to the missed T inversion and underestimation during daytime. This cycle of the T difference is much less visible at the lowest levels of the profiles (Fig. 6), due to the difference in elevation and in site as well as to the larger uncertainties of REM instruments compared to ground observations.*

46. l. 681 f: Comparisons are made to different versions of COSMO. Please either specify the versions /differences or remove.

*COSMO-1E is the only used version in this paper since KENDA-1 is the analysis mode of COSMO-1E. The nomenclature was harmonized in the whole manuscript.*

47. Conclusions

The Conclusions provides a bullet point summary of the key results. I would suggest to formulate continuous text for the conclusions with distinct paragraphs instead of bullet points.

*Done*

48. l. 772 - 787. The important circulation features are listed here. I would appreciate a figure/sketch similar to Fig. B1 where all the identified flow features are outlined and summarized

*Certainly, but up to now we didn't have found a good way to simply sketch the diurnal cycle of the various wind compounds. We will try a representation and incorporate it to the manuscript or the supplement if we find it helpful.*