Review for

"Analysing CMIP5 EURO-CORDEX models in their ability to produce south foehn and the resulting climate change impact on frequency and spatial extent over western Austria"

Synthesis:

The present study addresses a relevant topic, namely potential changes in the occurrence of foehn events under a changing climate, thereby focusing on western Austria in the Alps. Following a methodology similar to that of a previous study, the authors employ six XGBoost models to indirectly address this question. These models are trained using ERA5 variables in order to predict the occurrence of both localized and widespread foehn events across three subregions, with station-based foehn detection data providing the training labels. Subsequently, the machine-learning models are applied to CMIP5-CORDEX climate simulations for present-day and future climate scenarios. The authors find a shift in the seasonality of foehn events under future climate, with foehn becoming more frequent in spring and less frequent in autumn.

The manuscript tackles a relevant research gap and chooses a suitable methodological approach. The study would therefore be a valuable addition to the existing foehn literature, particularly given the limited understanding of how the frequency of foehn events may be influenced by global warming. However, there is a need for a more detailed explanation of the training and validation process of the XGBoost models, as highlighted in the first major comment. Additionally, there is room for refinement in both the structure and content of the manuscript: currently, the methods section is lengthy in contrast to the relatively concise and descriptive results section. Expanding the discussion of the results, as suggested in the second major comment, would significantly enhance the manuscript. Furthermore, there are many minor comments to be addressed that would further improve readability, consistency, and the overall quality of the manuscript. For these reasons, I recommend that the authors address these concerns if the manuscript is to be considered for publication in WCD.

Major comments:

1. **Approach to training and validation:** According to the reviewers' understanding, the authors opted not to partition the ERA5 and the OFC data into distinct training and test sets. Typically, machine learning models are trained on a subset of the data, while another subset remains unseen for the model and is reserved for testing and performance evaluation (see, e.g., Mony et al., 2021). Consequently, comparing performance metrics with other studies might be somewhat misleading, as the evaluation relies on the time period used for model training. I understand that the authors conduct an indirect performance evaluation by comparing the seasonal foehn frequency derived from OFC data in the time period of 2011-2021 to that of ERA5 and EURO-CORDEX data derived from the period 1991-2020. Nevertheless, I am curious how the authors

intend to demonstrate that the current approach does not lead to overfitting and consequently render the results less generalizable, particularly considering the differences we see between OFC and ERA5, but especially between OFC and EURO-CORDEX models in Figure 5. The temporal mismatch of the time period where the OFC data is available (2011-2021) compared to the historical time period used for indirect validation (1991-2020) makes it even more challenging to judge whether the XGBoost models have been successfully trained and applied to the present-day EURO-CORDEX simulations. Please comment on these aspects and discuss them in the manuscript.

2. The authors have made a clear distinction between the descriptive results and their interpretation, which is a valid option. However, I believe that **a more extensive discussion of the results is necessary, extending beyond what is currently presented in the conclusions.** Whether this extended discussion is incorporated at the end of each individual results section, integrated within more comprehensive conclusions, or presented as a distinct discussion section is up to the authors. However, in my opinion, such an addition would create a more equal balance between the actual results and discussion within the paper, especially compared to the current methods section. I give several suggestions for additional interpretation and discussion of the results below (further suggestions are provided in the minor comments as well):

    a) ***Interpret and discuss the seasonal foehn occurrence in the three regions for the present-day period according to OFC***: Figure 3 presents intriguing results and could be discussed in more detail. Is this the first "climatology" of foehn occurrence in Vorarlberg and Tiroler Oberland? The authors could highlight this more explicitly if no such prior publication exists. Additionally, several open questions arise when looking at the differences in the seasonal cycle of foehn between the three regions: Why does Vorarlberg feature substantially less foehn days during most months compared to the Tirol regions? Why do widespread events appear to be more prevalent in Tiroler Unterland compared to the other regions? Considering the size of the study area, one would anticipate that foehn events occur under similar synoptic weather conditions. The authors could validate this assumption by computing ERA5 composites for the mean synoptic situation during foehn days in 2011-2021 across the three regions.

    Local effects, rather than synoptic differences, are probably responsible for the above-mentioned differences between the three regions. The authors mention the north-south valley axis of the Wipp Valley as a factor favoring foehn breakthrough (L. 38 and L. 302). Are none of the other stations located in a valley that is also north-south oriented? It is worth noting that several other factors influence the foehn climatology at specific stations. Foehn is more likely to occur at stations that are closer to the main Alpine crest, both horizontally and vertically (e.g., Gutermann et al., 2012). Additionally, a deep incision in the crest, such as the Brenner gap, favors foehn penetration, especially during shallow foehn episodes that are primarily driven by the overflow of colder air from the southern to the northern side (e.g., Jansing et al., 2022). Another crucial factor to consider is a station's tendency to cold-air pooling. During the colder months, the foehn flow at lowerelevation stations in valleys often fails to reach the ground due to the stable valley air mass hindering its breakthrough (e.g., Drobinski et al., 2007; Haid et al., 2022). How strong is this effect at the different stations across the three regions? I am particularly surprised by the higher occurrence of widespread events in the Tiroler Unterland compared to other regions, as I had previously assumed that stations in the Inn Valley (such as Innsbruck, Jenbach, and Kufstein) are prone to cold-air pooling, especially in autumn and winter. Moreover, it is worth noting that local cold-air pools can pose challenges for the accurate prediction of foehn occurrence by the XGBoost models, as foehn-prone conditions might not lead to foehn breakthrough under such circumstances. I encourage a discussion of these factors in relation to the seasonal foehn occurrence diagnosed from OFC data, as well as their implications for the XGBoost predictions.

b) ***Provide a more comprehensive description and interpretation of the training results***: The authors might consider incorporating metrics such as the correct alarm ratio and probability of detection (as suggested in minor comment 16), as the accuracy metric may present an overly optimistic view on the model performance given the imbalanced dataset. This would enable a more precise evaluation of the performance discrepancies among the XGBoost models. For instance, the correct alarm ratio for Vorarlberg loc. / wide., as well as the probability of detection and the correct alarm ratio for Tiroler Unterland loc. / wide. seem to be lower compared to other metrics.

   Moreover, the authors could consider showing the importance of additional features, or at least indicate whether they hold nearly equal importance or are considerably less significant compared to the most important feature (Table 4). Additionally, Figure 5 could be expanded to multiple panels to illustrate the results for all three regions and both categories (localized/widespread). This represents one of the main results of the study. Presently, with the existing Figure 5, readers are unable to discern how ERA5, the weighted, and the unweighted EURO-CORDEX models capture the seasonality across different regions and categories. The manuscript would benefit significantly from including these results and discussing them within the text.

c) ***Revisit the description and interpretation of the XGBoost models applied to ERA5 and the EURO-CORDEX models for the historical period*:** Unfortunately, there is a temporal discrepancy between the time periods depicted for the training data (2011-2021) based on OFC and the predictions from the XGBoost models (1991-2020). How does the ERA5 seasonality look like when only considering the training period from 2011-2021? The same question is also valid for the XGBoost predictions using EURO-CORDEX data. This comment is also related to comment a) further above.

Minor comments:

1. The title ("… produce south foehn …") is somewhat misleading in the sense that the EURO-CORDEX models do not actually simulate foehn flows that resemble actual foehn flow

characteristics, since their resolution, as the authors correctly state in their manuscript, is too coarse to resolve individual foehn valleys. These models only represent the synoptic (and mesoscale) conditions that are typically associated with the occurrence of foehn flows in the Alps. Consider rephrasing the title of the manuscript.

2. L. 9: Similar to the first minor comment, the EURO-CORDEX models do not "produce south foehn" in the sense that these flows are resolved in the models. Instead, foehn is diagnosed using the atmospheric fields that represent the typical synoptic to mesoscale weather situations during foehn and non-foehn conditions.
3. L. 36ff: Please consider expanding this sentence. It is, in my opinion, oversimplifying the foehn research in the Alpine region, which is not limited to two field campaigns. If you are referring to the MAP field campaign, it would be more accurate to cite Mayr et al. (2007) and Drobinksi et al. (2007) to adequately represent the MAP findings concerning foehn flows. Furthermore, there has been a recent, more local-scale campaign focusing on foehn-cold-air-pool interactions (Haid et al., 2020, 2022.). Moreover, there exists a range of recent publications that focus on the representation of Alpine foehn flows in mesoscale to large-eddy simulations, aiming to understand the physical processes governing foehn flows and to improve the quality of foehn simulations (e.g., Seibert et al., 2000; Würsch and Sprenger, 2015; Miltenberger et al., 2016; Umek et al., 2021; Jansing and Sprenger, 2022; Jansing et al., 2022; Saigger and Gohm, 2022; Umek et al., 2022; Jansing et al., 2024; Tian et al., 2024). Consider incorporating this important aspect of foehn research in an additional statement and referring to selected publications.
4. L. 40: The descent of foehn in the framework of hydraulic theory does not occur due to the flow transition into critical or supercritical flow, but due to cross-Alpine density differences (buoyancy-driven, see, e.g., Mayr et al., 2007).
5. L. 41ff: Forecasting foehn using statistical methods extends further back in time. Widmer (1966) and Courvoisier and Gutermann (1971) already developed an index to predict foehn at Altdorf, Switzerland. Moreover, Dürr (2008) also developed an objective foehn classification method using station data.
6. L. 46: This is actually not true, there are by now regional climate projections with a spatial resolution down to 2.2 km (e.g., Ban et al., 2021). In these models, one could actually at least partially resolve the foehn flows and thus explicitly investigate foehn flows under future climate.
7. L. 51: Given the similarity in approach, it would be beneficial to summarize the main findings of Mony et al. (2021) in one or two sentences. This would sharpen the research addressed in the study, specifically the investigation of future foehn occurrence over Austria, a topic that has not been explored previously. For instance, it has remained uncertain whether the conclusions drawn by Mony et al. (2021) could be replicated with other climate models and applied to foehn regions in the Eastern Alps.
8. L. 59-73: A substantial paragraph of the introduction is devoted to a discussion of the advantages and disadvantages of ensemble weighting. I wonder if at least part of this discussion would be better placed in the methods section. The current structure of this paragraph overemphasizes this particular method and focuses too little on the actual aims of the study. Therefore, the author may consider restructuring this part of the introduction to better align with the overall focus of the research.
9. Fig. 2: Nice flowchart! Why not drawing an arrow from "11 years of daily training data" to "Training of two XGBoost models per region" as well?

10. Table 1: How is the model main ridge defined? Please specify this.
11. Table 1 vs. L. 97-99: Table 1 is not fully consistent with the text; for instance, the cross-Alpine potential temperature gradient is not mentioned in the text. Additionally, could the authors provide further insight into their rationale behind selecting the cross-Alpine pressure gradient within ± 1° latitude relative to the model's main ridge as a feature (only 37 grid points), compared to the inclusion of every grid point for the geopotential difference on 500 hPa (925 grid points)? The same question applies to the cross-Alpine potential temperature gradient. Moreover, the authors might consider elaborating on why they opted for the relative humidity difference with respect to the monthly mean as an additional feature, especially since it is the only variable where an anomaly is considered. It would be beneficial for readers to gain a deeper understanding of the motivation behind the selection of the feature matrix input.
12. L. 100-101: This is actually not the study domain (not the red box in Fig. 1a), but the domain considered to extract features from ERA5.
13. L. 110: Can the authors specify how many models are included in this OEKS15 selection?
14. L. 117ff: In my opinion, this paragraph could be shifted to the beginning to the next paragraph, as it is directly related to the OFC used to generate the training labels. If done so, I would also suggest to rename section 2.1, e.g., to “2.1 Reanalysis data and climate projections”.
15. L. 141-142: Judging from Fig. 4b, Vorarlberg does not feature a less dense network of stations, but simply is the smaller region.
16. L. 179-182: As the authors correctly point out, the accuracy (acc) is not a suitable metric for a highly unbalanced dataset like the one presented here. They could therefore consider to also evaluate the correct alarm ratio (TP / (TP + FA)) and the probability of detection (TP / (TP + ME)), which would give the reader a more comprehensive picture of the model performance on the training dataset.
17. L. 199ff / Eqs. 5-7: As the authors compare two different time periods (1991-2020 vs. 2011-2021), this correction is only valid if the annual frequency, the seasonality and the inter-annual variability in the period 1991-2010 do not differ too much from the period 2011-2021. Unfortunately, this cannot be tested with the training data used in this study. It should be noted that years with an unusually high or low foehn frequency often co-occur over several years (e.g., Richner et al., 2014). Did the authors try a correction where they calculated the biases using only the period 2011-2020 (i.e., N = 10 instead of N = 30)?
18. L. 205ff: Why is the seasonality bias ($b_{m,seasonality}$) either added or subtracted from the monthly foehn events before computing the standard deviation?
19. Section 2.4: This section is rather lengthy, describing the methodological procedure of weighting of the different simulations. It therefore appears imbalanced, especially compared to the relatively short results section. The authors might consider shifting part of this explanation into an appendix (e.g., appendix D).
20. L. 235, L. 248, L. 256, L. 274: The section titles reflect the methodology used rather than the results they present. Why not rename them, for example, to “3.1 Present-day foehn climatology based on station data and OFC“, “3.2 Model performance for present-day climate“ (merge 3.2 and 3.3 as 3.2 is very short anyway), “3.3 Foehn occurrence under future climate“? These are just suggestions, but consider renaming the sections to make them more intuitive.
21. Figure 3: The caption explains that the uncertainty bars show the standard deviation over the 11 years. In my opinion, it would be more intuitive to adjust the ranges to the minimum and

maximum, or to the 10th and 90th percentiles. The current visualization is misleading as there are certainly not less than 0 foehn days in a given month.

22. Figure 3: I suggest to show the average localized foehn days in a third panel and the temporal evolution (i.e., the inter-annual variability) in a fourth panel. This would allow the reader to get a more complete picture of the contrasting behavior of the three regions (e.g., Tiroler Unterland has less localized but more widespread events than Tiroler Oberland – really surprising to me!)
23. L. 239-242: The text is not consistent with the figure. The figure shows that Tiroler Unterland has the highest overall annual foehn frequency and a secondary maximum in autumn due to widespread events. In the text, however, the authors claim that the Tiroler Oberland has the highest annual foehn frequency (17.9%) and the highest number of widespread events. Which is correct?
24. L. 281 and L. 284: Please be careful when talking about "significant trends", especially in the context of statistical trend analysis. Did the authors test the trends for significance using a statistical method?
25. Figure 7: Given the different trends for localized and widespread events in Figure 6, it would be interesting to see Figure 7 separately for localized and widespread events. If the authors feel that this is not appropriate in the main text, they could include it in the appendix (or a supplement).
26. L. 298-303: Here the authors could discuss the other possible explanations for the regional differences (see major comment 2a).
27. L. 307-308: Consider comparing the correct alarm ratio and the probability of detection with Mony et al. (2021). Note that the comparison is somewhat misleading, as the authors did not apply the XGBoost models to a test dataset that the models had not seen before (see also major comment 1 and major comment 2b).
28. L. 327-330: How can a cold bias in the Alpine region, or a negative mean sea-level pressure bias, cause the biases in the annual foehn occurrence? I suggest that these models must either be biased in the north-south *pressure gradients* during foehn, or have less synoptic conditions conducive to foehn formation, which would be counterintuitive if they reproduce the frequency of extratropical cyclones over Europe.
29. L. 341-344: As there are now two studies claiming an increase in the occurrence of south foehn in spring and a decrease in late summer/early autumn, it would be very interesting to hear from the authors some possible hypotheses as to why this shift might occur under future climate. Will there be a shift in the seasonality of extratropical cyclones passing over Europe? Is the propagation and breaking of Rossby waves expected to change? I would be interested to hear some speculation from the authors in this regard. This is a relevant question for future research, as the authors rightly point out on pp. 357-358.
30. L. 345ff: I see two more caveats of the study that should be mentioned here:
    a) The XGBoost models are not able to able to fully replicate the seasonality of foehn occurrence in the historical period (perhaps due to the lack of a split between the training and test datasets; see main comment 1).
    b) The statistical approach is unable to capture local effects on foehn occurrence, such as the effect of local cold-air pools. This makes training more difficult, and potential future changes in the occurrence of cold-air pools and the effects on foehn occurrence are also not captured by the approach. The use of climate models that explicitly resolve foehn flows could mitigate this limitation.

Textual comments:

1. L. 25: I think Greenland has just one ice sheet, i.e., the “Greenland Ice Sheet”.
2. L. 26: I would be consistent and state the location for each impact, i.e., damaging rice crops *in Japan.*
3. L. 27: It should be “Baumann et al. (2001)”. Also check the order of authors with the journal guidelines.
4. L. 31: Is “FAO” the author names?
5. L. 53: “conditions for foehn” sounds a bit strange to me. Maybe “conditions associated with foehn” is better suited?
6. L. 114: “…, which absolute values were expected to be due to conditions close to the free atmosphere” → “…, as absolute values of wind speed in the free atmosphere were expected to be captured realistically”
7. L. 70-71: “…, model weighting is likely to improve predicting a model mean and smaller uncertainties, …” → This sounds strange. Maybe rephrase to “…, model weighting is likely to improve the model mean and reduce uncertainties”?
8. L. 79: “producing south foehn” → “producing synoptic to mesoscale conditions associated with south foehn”
9. L. 94: “adjusting a NWP to observations” → “assimilating observations including, amongst others, radars, satellites and radiosondes into the integrated forecast system of ECMWF”
10. L. 268: “… in the historical period, summer across all regions …” → “… in the historical period across all regions…”
11. L. 293: “eastern Alps” → “Eastern Alps” (is capitalized in other occurrences)

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