

Author Response to Manuscript "Undercatch corrected gridded precipitation data to improve hydrological modeling in high-alpine orography"

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General Remarks

We sincerely want to thank the editor and both referees for their time and review. We appreciate your comments regarding our scientific honesty and thorough hydrological validation and are happy that the scope of our work is considered suitable for HESS. However, we understand that our manuscript requires more refinement to be presentable in the journal and are
5 thankful for all the comments - both major and minor - as they will greatly improve our study. In the following document we have listed the original referee comments in gray, whereas our responses to all of them are stated in black font. We have extensively reworked the methods sections regarding the weather stations, conclusions and interpretations as well as the reference management. We further added a Figure in the supplement aimed to quantify information loss when switching
10 that we could suffice the referee comments and that our manuscript can move forward to publication in HESS.

Sincerely,

Caroline Ehrendorfer and Philipp Maier (on behalf of the whole author team)

15 **Remarks of Referee 1:**

This manuscript presents a methodology to derive spatially continuous correction factors intended to account for wind-induced precipitation undercatch. The authors first apply established undercatch correction equations to hourly precipitation measurements at surface stations and then aggregate corrected and uncorrected precipitation totals at the monthly scale to derive station-based monthly correction factors (CFs). These CFs are subsequently interpolated over Austria using generalized additive models (GAMs) with static spatial covariates describing terrain and wind exposure. The resulting gridded CF fields are then used to adjust an existing gridded precipitation product. The impact of this correction is evaluated through snow and hydrological modelling. The authors show that, for two catchments investigated in detail, increasing precipitation at the appropriate time of year improves simulated snowmelt dynamics and leads to better runoff simulations compared with the raw precipitation product. Across a larger set of Austrian catchments, the corrected forcing also improves some performance metrics, although the benefits are not systematic. Overall, although the level of methodological novelty is moderate, I found the manuscript clear, well written, and scientifically honest about the strengths and limitations of the proposed approach. I also appreciated the authors effort to evaluate the correction framework from multiple perspectives. I nevertheless have several comments that I believe should be addressed before publication.

General comments

30 – A central assumption of the study is the aggregation of hourly undercatch-corrected precipitation into monthly station-wise correction factors, which are then spatially modelled. Because undercatch is fundamentally an event-scale process that depends on wind conditions, precipitation phase, and likely also precipitation intensity, this monthly aggregation is a strong structural simplification. Did you look at the information lost when moving from hourly corrections to monthly multiplicative factors? For example, a 10mm event could occur with and without high wind conditions but the proposed approach will apply the same CF if the event occurs at the same elevation/month.

This is also a comment addressed by referee 2. We therefore decided to include an additional Figure in the supplement comparing the hourly corrections to the climatological monthly factors at selected stations across different time scales and included the following lines in the manuscript:

"Monthly correction factors are then obtained by relating pr_{corr} to the uncorrected precipitation pr , resulting in one climatological correction factor per station and month. While this aggregation leads to some loss of information and may oversimplify individual events, it does not introduce a systematic bias for climatological time frames, which are the focus of this study (see Figure S4 in the Supplement) and is easily transferable for future studies."

The supplement reads as follows:

"Figure S1 compares the transfer-function undercatch correction (TF) proposed by Kochendorfer et al. (2017) with our climatological factors (CFs), which were derived using the GAM methodology, across different time scales. It shows that individual events differ in their estimated precipitation correction: the event in Fig. S1c occurred under lower wind

speeds than the CFs imply, whereas the event in Fig. S1d is underestimated. Note that the same event is displayed in panel a and d at two nearby stations, one located on the valley floor and the other on the summit, for which substantially different corrections are suggested.

50

At the scale of monthly sums, differences between the TF-based and CF-based correction already begin to decrease. On an annual timescale, no systematic bias is evident, and deviation in precipitation sums relative to the TF-corrected values remain in the lower single-digit percentage range or below. Since the transfer function includes a wind-speed cutoff (Kochendorfer et al., 2017), and no systematic bias is visible, we conclude that extreme events are not over-represented."

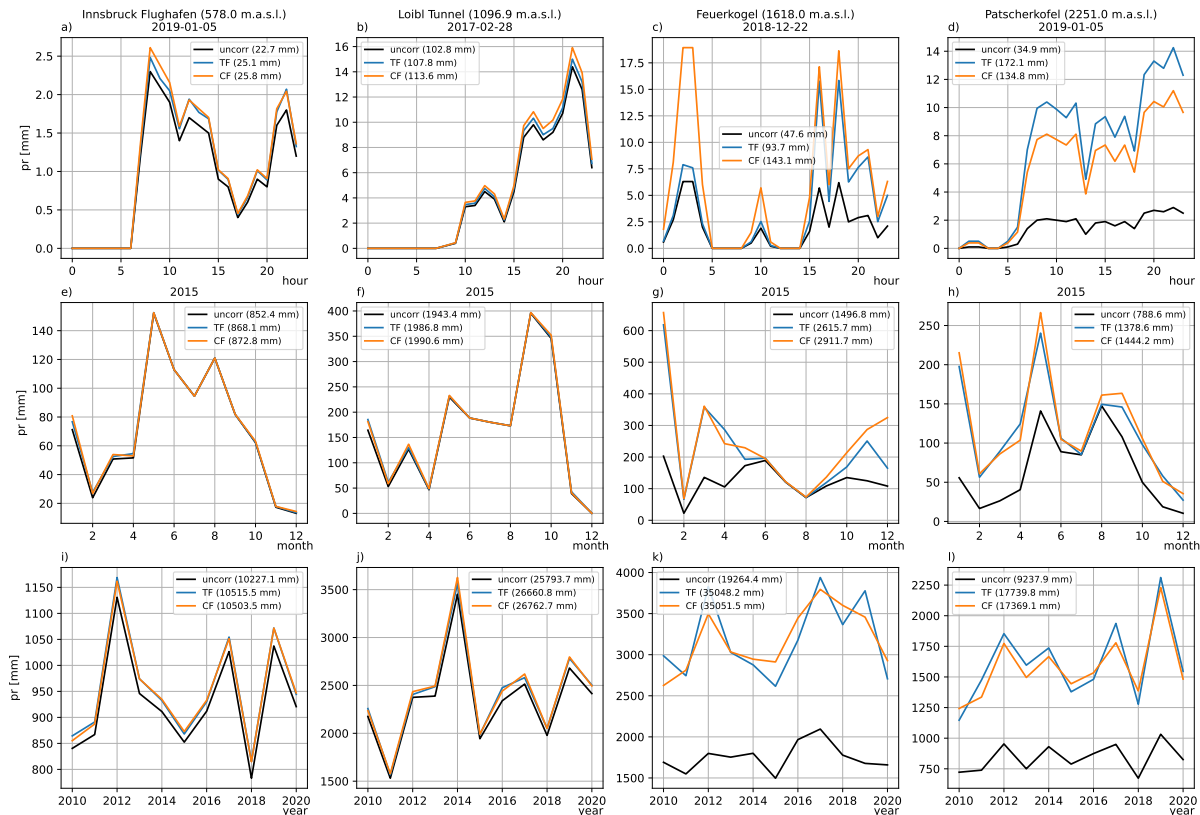


Figure 1. Comparison of station precipitation in mm across different timescales for a-d) hourly, e-h) monthly and i-l) yearly sums. Shown is the uncorrected station (uncorr, black), transfer-function (TF, blue, Kochendorfer et al., 2017) and climatological GAM-factor (CF, orange) corrected precipitation. Individual columns refer to different stations, whereas the elevation of the stations increases each column. Precipitation sums are given in the legend.

- Did you consider fitting a more parsimonious model in which seasonality is represented directly, for example by including month through sine/cosine terms, rather than fitting a separate GAM for each month? This would reduce model complexity and may help regularize the seasonal structure.

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We did indeed try that, utilizing different GAM structures for incorporating the month, but ended up with worse performance scores. We therefore judged that the movement of the snowline, together with potentially different climatological conditions in each of the months, may be too difficult for one single GAM to capture and decided to go with monthly GAM setups, which of course is more convoluted in presentation, but yields better results. Naturally, other models like random forests would even enhance performance, and potentially would be more equipped to unify all months in a single model. Using such machine learning models would come with the price of physical interpretability, which is why we refrained from implementing them in this study.

- The strongest corrections appear to occur in high-elevation regions, which are also the region with the least dense observation network. Do you think your GAM models could extrapolate more strongly there? Could you address this point in the discussion more explicitly?

We added the following paragraph to the GAM discussion and hope that together with the improvements in the description of Figure 7 we can resolve this comment. The snow-hydrological validation in the two high-alpine case study catchments puts further emphasis on the GAMs performance at high elevations. "Figure 7 and S2 further illustrate that capping the GAM-predicted undercatch factors at the observed station factors (both shown in Table 1) is a crucial step to prevent excessive extrapolation at high elevations. This is particularly important given the sparse station network at high elevations where the GAMs must extrapolate beyond well-constrained training data."

Specific comments

Major comments

- Line 238: Are you comfortable with the level of independence between calibration, methodological development, and detailed hydrological evaluation? Please clarify this point.

Thank you for the valid comment. We are confident in the level of independence between the undercatch correction development, calibration, and hydrological evaluation, though we acknowledge that the degree of independence differs between the case study and Austria-wide contexts. The undercatch correction was developed independently of runoff. For the case study regions, the two model components that could otherwise absorb a precipitation deficit (glacier melt, actual evapotranspiration (ETA)) were both manually constrained to physically realistic ranges (based on literature and expert knowledge) rather than left free to compensate. Therefore, when the water balance closes, it is not an artifact of unconstrained calibration absorbing the correction, and we interpret it as meaningful evidence for the undercatch correction. The Alpine3D simulation adds further independent, process-based support from outside the conceptual modeling framework. For the Austrian-wide model, ETA is more uncertain at the regional scale. It is not directly validated, and the domain-wide calibration does not constrain it, meaning it may partially absorb water balance errors. We therefore did not simply attribute Austrian-wide runoff biases to the undercatch correction. Instead, we systematically examined the spatial pattern of residual biases and attributed them to identifiable causes (e.g. runoff overestimation in southern Austria is potentially linked to regional biases in the base precipitation dataset; overestimation in (north)eastern

90 lowlands is attributed to the calibration's inability to capture flashy, high-ETA regimes; underestimation in central karstic regions due to potential catchment boundary misalignment). We believe this regionally differentiated attribution demonstrates that we did not uncritically conflate model bias with undercatch signal, and that the Austria-wide evaluation adds interpretive value beyond a simple performance metric.

95 – Fig 5: It would be useful to add, as an additional subplot to Fig. 5, a map of station-based undercatch correction factors for one representative winter month (for example December or January). This would help the reader visually assess whether there is an obvious spatial structure in the station values before interpolation, even if, as the authors later note, spatial structure is not the only driver of the correction factors.

We added an additional subplot (Fig. 2b in this document) for the undercatch correction factors in January, modified the caption and flow text to accompany these changes. We further added this discussion:

100 "Further, as Fig. 5b suggests, no clear spatial structure is apparent in the undercatch factor distribution besides the tendency of increasing undercatch towards Austria's west, where highly-elevated stations are located."

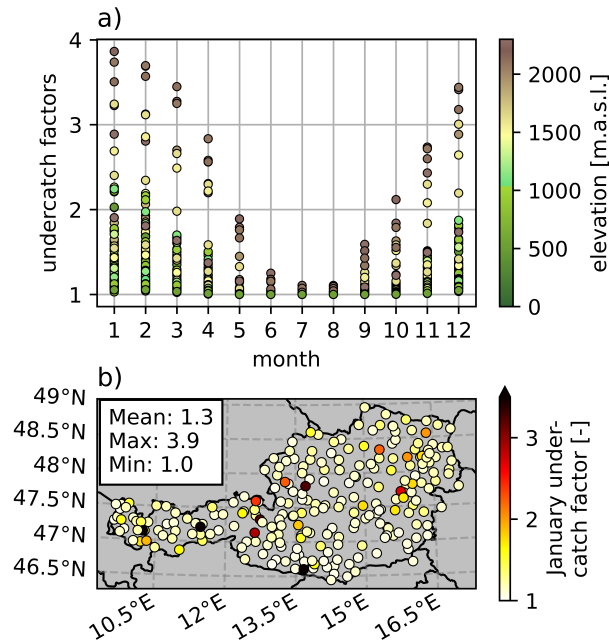


Figure 2. a) Station-derived monthly undercatch factors. The color-coding refers to the station elevation of the data entries in [m.a.s.l.], where green colors show stations with low and brown and yellow colors stations with high elevation. b) Spatial distribution of January undercatch factors (color-coded) across Austria.

– Fig 7 and Lines 299 to 301: from the figures it is hard to state that the error “increases with elevation” is sufficiently well supported. Above 1500 m, the number of stations becomes limited, and a range of exposure values is still represented. Plus, the stations with the largest relative errors do not seem to correspond clearly to the highest positive exposure values.

105 Although, the number of stations is clearly less at higher elevations, we do think the underlying data supports this argument. We tried to clarify the statement further by addressing it explicitly in the flow text:

"Figure 7 shows the relative monthly GAM error, averaged over all months for each station. The error generally increases with elevation (with a Pearson correlation coefficient of 0.61), as does the undercatch uncertainty (Pierre et al., 2019). The relative error for the 18 stations above 1500 m averages to 0.15 as compared to the average error of 0.02 for the
110 stations below this elevation."

We additionally weakened the statement regarding the exposure factors to:

"The highest relative errors tend to occur at stations with positive exposure values,..."

– Fig 9: I appreciated this figure, but I found the discussion too brief. Are these values expected or not? It would help the reader to extend the discussion to understand whether these values are plausible, surprising, or consistent with prior
115 knowledge.

We reworked the whole section discussing Fig. 9 to:

"Figure 9 shows the local precipitation lapse rates of the undercatch corrected precipitation climatology on a 9 km x 9 km basis in % per 100 m elevation gain. These lapse rates serve as a plausibility check of the corrected precipitation fields. Grid cells shown in white were masked because the local elevation range was insufficient for a meaningful calculation (less than 500 m), or because fewer than half of the 81 grid cells within the window contained valid data. At the highest
120 elevations, lapse rates plateau and locally decrease (yellow colors), which is a direct consequence of the exposed terrain penalty. The mean precipitation increase across Austrian stations is approximately 33 mm per 100 m, corresponding to around 3 % per 100 m when normalized by the mean precipitation of our study domain (Fig. 8a). This is slightly lower than the mean lapse rate of 4.0 % per 100 m shown in Fig. 9, but remains within a plausible range when taking into
125 account the strong corrections in the alpine regions. Since precipitation lapse rates are spatially diverse and vary across the Alpine region due to slope and shielding effects modifying the precipitation-elevation relationship (Frei and Schär, 1998; Dura et al., 2024), individual grid cells are difficult to verify directly. Enhanced lapse rates in regions of strong orographic lifting north of the Alpine main ridge, including the area where the maximum value occurs, appear plausible. However, the high values found in the comparatively dry deep valleys of western Austria, ranging between 7 and 10 %
130 per 100 m, may indicate local overestimation. This interpretation is supported by comparing these values with the lapse rates between the stations Innsbruck Flughafen and Patscherkofel (see Fig. S4 in the Supplement), which are located in close proximity to each other at a valley floor and a mountain crest. Based on transfer-function undercatch-corrected mean annual precipitation, this station pair yields an average annual lapse rate of 4.1 % per 100 m, considerably lower than the highest local values in the gridded field."

- 135 – Section 3.4: why starting with the hydrology and not the snow modelling in the description of the results? Following the modelling chain illustrated in Fig. 4, I would have expected the reverse order. Since undercatch corrections should primarily affect solid precipitation and snow accumulation first, and runoff only afterward through melt, presenting snow-related impacts before hydrological impacts would seem more natural

The idea was to start with the overall annual water balance and then look at the individual processes in more detail.

- 140 Figure 4 displays the COSERO model structure (including the snow module). But the snowpack evaluation was carried out with the physically-based snowpack model Alpine3D, not with COSERO - this is only used for runoff validation. We therefore propose to leave the results in the existing order.

- Line 373-... : why using snow depth and not snow water equivalent for the evaluation? The SWE is generally more directly relevant for hydrology and, in many contexts, more informative for assessing the impact of precipitation corrections on water storage.

145 While Alpine3D models both SWE and snow depth, we only have a direct, independent satellite observation for the latter. While the snow depth observation could be converted to SWE, this would require a density assumption and thus introduce additional uncertainty. Snow depth is sufficient to assess whether the precipitation correction improves snowpack accumulation patterns, which is the primary evaluation goal here.

- 150 – Line 390... : I found the evaluation on the glacier very interesting.

Thank you! A lot of work has gone into this.

- Line 524-525: Could you add a reference or two on the “lagrangian precipitation frameworks with high resolution framework” ?

We modified this statement and included references:

- 155 "... for example through advanced modeling approaches that couple Lagrangian models (e.g. Bakels et al.,2024) utilizing moisture and precipitation frameworks (e.g. Sodemann et al.,2008) with high-resolution snowpack simulations,..."

- Conclusion:

- Which parts of the framework can or cannot be generalized to other mountain regions (data availability, network metadata, etc.) ?

- 160 – I think it would be interesting to mention that alternative correction approaches could also be explored, for example transfer functions that depend more explicitly on snowfall intensity rather than on air temperature. One potentially relevant reference is: Impacts of Adjusting Solid Precipitation Amounts in the Canadian Precipitation Analysis System, Journal of Applied Meteorology and Climatology, 64(7), 2025.

We included your suggestions and refined the final paragraph of the conclusions as follows:

165 "The framework presented here is generalizable and can be reproduced in other mountain regions, provided that the
following requirements are met: i) comparable (high-elevation) station coverage, ii) sufficient observation quality, and
170 iii) DEM and gridded precipitation data that adequately resolve the spatial scale of the underlying orography. The
latter requirement is particularly important, as strongly smoothed DEMs can fail to represent small-scale topographic
characteristics and may affect elevation-dependent precipitation estimates in complex high-alpine terrain (Weber et al.,
2021). Where these conditions are fulfilled, we encourage the application of the proposed methodology to other mountain
regions and gridded precipitation datasets to advance the understanding of undercatch errors across diverse climatological
and topographic settings. Additionally, future work should evaluate alternative undercatch correction approaches systematically,
175 in a similar structure as proposed by Aubry et al. (2025), for example by considering correction methods that rely on
snowfall intensity rather than on air temperature. Further, greater emphasis should be placed on improving the physical
representation of undercatch. This could be achieved through advanced modeling approaches that couple Lagrangian
models (e.g. Bakels et al.,2024) and moisture and precipitation tracking frameworks (e.g. Sodemann et al.,2008) with
high-resolution snowpack simulations, as well as through enhanced experimental observations at high-elevation and
exposed sites. Such developments are essential to further reduce uncertainty in alpine precipitation estimates and to
improve the reliability of hydrological projections in a changing climate."

180 **Minor comments**

– Line 40: misplaced parenthesis **Done.**

– Line 141: Explain what patch interpolation is?

We added:

185 "..., which is an artifact-reducing, smoother, higher-order regridding method than bilinear interpolation (Jiawei Zhuang
et al., 2025)."

– Line 229: Missing the word "Fig" **Done.**

– Fig 8: could you consider having an organization of maps of (2, 2) instead of (4,1) ? **Done.**

1 Remarks of Referee 2:

190 Maier et al. used weather station data in Austria to create correction factors to adjust for the well-documented wind-induced undercatch that precipitation gauges are suffering from. They further developed a method to expand those correction factors and present a gridded data set, adjusted for undercatch. The results were validated with independent measurements from two catchments in Austria and the authors could show that a general more realistic representation of the precipitation were achieved. While the idea of developing a gridded data product with precipitation data adjusted for undercatch is not new, the development of a consistent and complex method for it and its application to a new region in this paper, justifies the publication of this paper
195 in HESS. The study appears well prepared and the detailed verification of the results at the end, and the discussion of the limitations witness of a thorough process.

I do think, however, that this thoroughness is not reflected in the presentation quality of the paper. There is a need for major improvements in the description and explanations of the underlying data and calculations which will also increase the scientific quality of this study. Please see my major comments on this. In my opinion, also the use of references is insufficient repeatedly
200 throughout the study. Some references seem cited slightly out of content, and some do not clearly difference what part is the authors' contribution and what is taken from the citation. I will give some examples at the end but encourage the authors to carefully re-check all their references and possibly adding further studies which work on similar topics in different regions before resubmitting a revised version.

Major Comments:

205 Section 2.2 – Weather Station Data Line 125ff. This entire section is missing important information.

- It is unclear to me what exactly you mean with “semi-automated” weather stations. What of them is not fully automated? Do all stations have all the parameters you mention? What about their location and elevation? Much later, in your result section line 277 you state that wind and precipitation sensors are not always co-located. Also, the elevation of the precipitation data will become an important criteria in your results – this information is clearly missing here.
- 210 – You should describe and explain what gauge-types are in use already in this section. You later mention that it is mostly tipping buckets (line 159, section 2.4.), as a reasoning for your selected CE transfer function (which then was from the paper not describing tipping buckets, see comment for section 2.4), but also name in line 211 that you want to account for the use of gauge totalizers (possibly as base for the gridded data set) without any further explanations. Please gather all the information about your station data here in this section.
- 215 – Another interesting (and missing) factor is which time periods of data are you analyzing? You mention that you require at least 10 years of data, resulting in your selection of 261 stations – but do you analyse 10 years – do all stations cover the same 10 years?

Thank you very much for the detailed comment regarding this section. We agree that we took too much knowledge regarding the stations for granted and enlarged section 2.2 as follows:

220 "This study utilized 10-minute measurements of temperature, wind speed, and precipitation from semi-automated weather stations, provided by Geosphere Austria, Austria's federal institute for geology, geophysics, climatology, and meteorology (GeoSphere Austria, 2024). All meteorological variables required in this study are recorded automatically, while some (unused) parametrizations like cloud type, as well as downstream quality control are performed by experts. Precipitation is predominantly measured using tipping-bucket gauges, although the number of weighing gauges has increased in recent years (Haslinger et al., 225 2025). To ensure data quality, stations were required to provide at least ten years of measurement records for every month and to exhibit plausible average precipitation values. Stations were not required to cover a common overlapping period, which reduces the influence of decadal variability on the derived correction factors; the full period from which station data were drawn is 1995-2024. The stations are densely distributed across Austria, as shown in Fig. 1a. Austria's two highest stations, located at elevations of 2864 m and 3109 m, were excluded from the station ensemble since they would otherwise place 230 disproportionate weight on elevation bands with low station density. This process resulted in a final set of 261 stations, with the highest remaining station located at 2327 m and the majority of stations being located below 1500 m (see Fig 7 for the elevation distribution). As the catch efficiency transfer function utilized in this study (Kochendorfer et al., 2017) was derived using 30-minute measurements, the 10-minute data were aggregated to 30-minute temporal means for wind speed and temperature and 30-minute sums for precipitation."

235
We further rephrased our comment in Line 277 regarding the proximity of sensors:

"This uncertainty could arise from local installation constraints, where precipitation gauges and wind sensors are not always co-located."

240
Section 2.4.

– Lines 155-160, including equation (1) : You present the CE transfer function from Kochendorfer et al. 2017a (which is the correct reference for the presented equation at this point; please also see the reference comment for line 89-91). However, this equation was not, as stated in the next sentence, developed for tipping bucket gauges, but for weighing gauges. The 245 equation to be used for tipping bucket gauges would be from Kochendorfer 2020 (also in your list of references). It is, however, slightly different and requires different parameters. Depending on the gauge type in your dataset, which needs to be mentioned and described in section 2.2. (see earlier comment), you may want to change the equation, which would demand a re-calculation of the gridded data set as well. If you are deciding to keep this equation, please double check your constants. You write $\alpha=0.6023$, while I find $\alpha=0.06023$ in Kochendorfer et al. 2017a, Table 2.

250 Thank you for pointing that out. We sincerely apologize for this honest mistake. We decided to use the equation by Kochendorfer et al. (2017) over the equation from Kochendorfer et al. (2020), both visible in Fig. 3, for the following reasons:

- Explicit use of temperature as a variable in the transfer function.

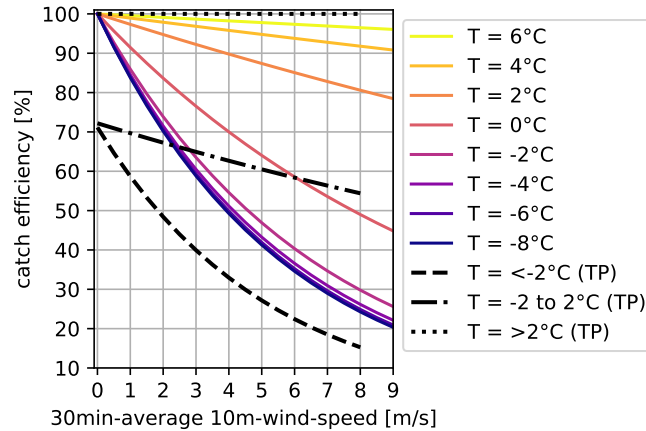


Figure 3. Comparison of the undercatch correction function for weighing gauges (color-coded, Kochendorfer et al., 2017), and for Tipping Buckets (TP, black, Kochendorfer et al., 2020) for different temperatures.

255

- Small, almost linear correction for high wind speeds for $T > 2^{\circ}\text{C}$. Both of these properties are missing for the tipping bucket equation (Kochendorfer et al., 2020).
- Weaker correction for low wind speeds and no a-priori correction of approx. $1/0.7$ for wind lulls. Using the tipping bucket correction in exposed terrain would have led to even stronger corrections and would have required to employ an even stronger exposed terrain penalty.

260

Using the tipping bucket equation would, in our judgement, not be feasible for the high-alpine application we were planning to apply it to. Although the predominant stations still use tipping buckets, more weighing gauges have been installed in the systems of GeoSphere Austria in recent years (Haslinger et al., 2025). We therefore still propose to stay with the current version. Also, it was a typo in the constant - thankfully only in the manuscript, not in the development code. We fixed it accordingly. Further, we modified the text in the manuscript:

265

"This transfer function was developed for unshielded weighing gauges using wind speed measurements at 10 m mast height. Although tipping buckets are still the predominant gauge type in the GeoSphere Austria network, weighing gauges have been increasingly installed since the early 2000s (Haslinger et al., 2025). A similar transfer function for tipping buckets was proposed by Kochendorfer et al. (2020); however, we use the weighing-gauge formulation for our high-alpine application. This choice is motivated by the absence of an explicit temperature term in the tipping-bucket function and by its comparatively strong wind-speed correction, including corrections of around 40 % for $T < 2^{\circ}\text{C}$ and $U = 0\text{ms}^{-1}$, which would have been difficult to implement in high-alpine regions as wind speeds are typically considerably higher at peaks (Graf et al., 2019)."

270

- Line 165. What is your rationale behind aggregating your exposure factors to monthly averages? Both temperature and wind can vary a lot within a month, thus creating possibly the whole range of corrections factors. Off course local climate

275 will be seen on the average – but this rather extreme aggregation step (30min to 1 month) needs more explanation, as you lose a lot of information you just have gathered. You could show the distribution within the months (are they normally distributed, what is the variance, how often do you have extreme values which may be overrepresented in your average. . .).

280 A similar comment was posed by referee 1. We therefore included an additional Figure in the supplement and deepened the discussion about the goal of the aggregation in the manuscript. to improve your reading experience, we repeat here our response to referee 1.

We therefore decided to include an additional Figure in the supplement comparing the hourly corrections to the climatological monthly factors at selected stations across different time scales and included the following lines in the manuscript:

285 "Monthly correction factors are then obtained by relating pr_{corr} to the uncorrected precipitation pr , resulting in one climatological correction factor per station and month. While this aggregation leads to some loss of information and may oversimplify individual events, it does not introduce a systematic bias for climatological time frames, which are the focus of this study (see Figure S1 in the Supplement) and is easily transferable for future studies."

The supplement reads as follows:

290 "Figure S1 compares the transfer-function undercatch correction (TF) proposed by Kochendorfer et al. (2017) with our climatological factors (CFs), which were derived using the GAM methodology, across different time scales. It shows that individual events differ in their estimated precipitation correction: the event in Fig. S1c occurred under lower wind speeds than the CFs imply, whereas the event in Fig. S1d is underestimated. Note that the same event is displayed in panel a and d at two nearby stations, one located on the valley floor and the other on the summit, for which substantially different corrections are suggested.

295 At the scale of monthly sums, differences between the TF-based and CF-based correction already begin to decrease. On an annual timescale, no systematic bias is evident, and deviation in precipitation sums relative to the TF-corrected values remain in the lower single-digit percentage range or below. Since the transfer function includes a wind-speed cutoff (Kochendorfer et al., 2017), and no systematic bias is visible, we conclude that extreme events are not over-represented."

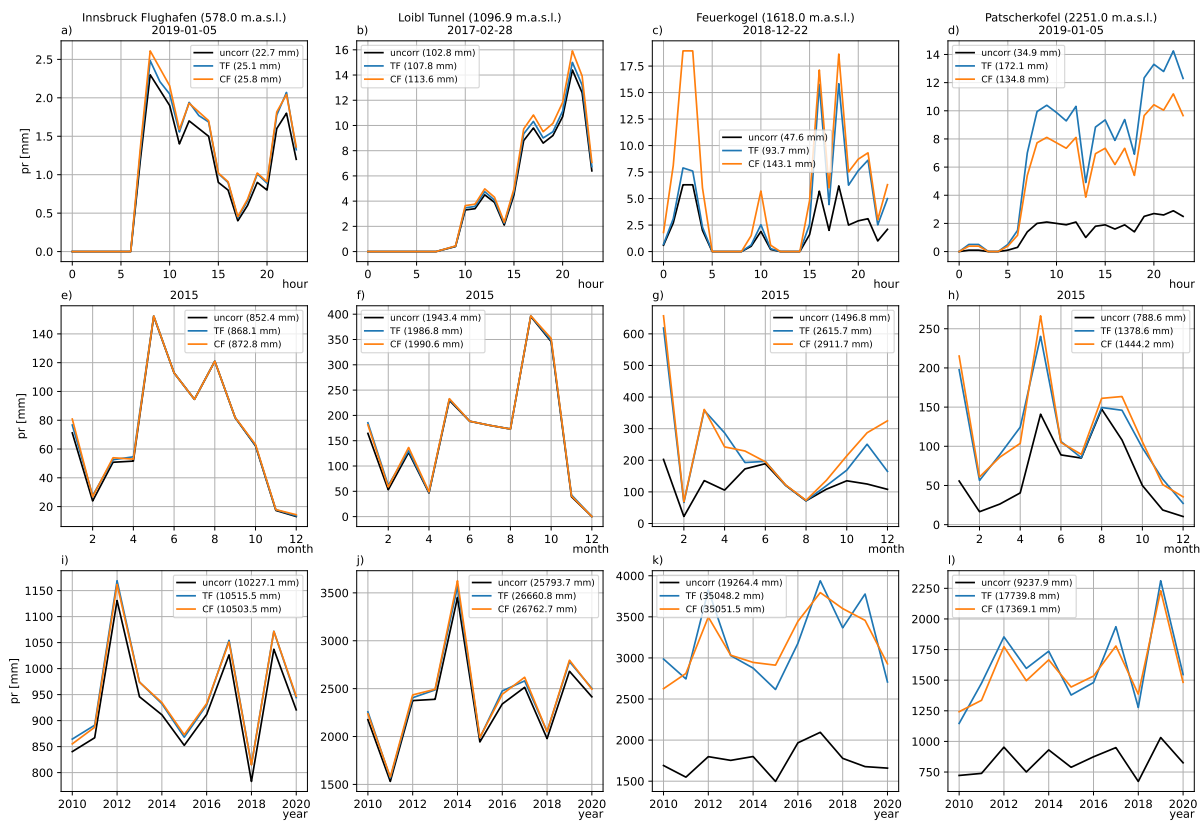


Figure 4. Comparison of station precipitation in mm across different timescales for a-d) hourly, e-h) monthly and i-l) yearly sums. Shown is the uncorrected station (uncorr, black), transfer-function (TF, blue, Kochendorfer et al., 2017) and climatological GAM-factor (CF, orange) corrected precipitation. Individual columns refer to different stations, whereas the elevation of the stations increases each column. Precipitation sums are given in the legend.

Section 3.2 Description of Figure 6, Lines 256 ff.

300 – Please be more concrete here and describe your results in more detail. The term “dominant relationship” means in your case a whole order of magnitude, factor of 10 at least for the summer months. It is not so clear, however, if this factor is dominant for the winter months, as all winter curves seem to gather under a multiplier value of 5, which would be in the order of magnitude with the two other features. Also, for me it is not obvious why the elevation dependent factor should be much larger during summer than during winter – I was expecting it to be the other way.

To answer this comment as well as one minor comment from you regarding this section, we reworked the whole paragraph, 305 where we discuss the smooth functions. We hope that we now have sufficiently discussed these results. It reads as follows:

"Figure 6 presents all twelve smooth functions from the individually trained GAMs. The curves show the exponentially back-transformed relationships between the undercatch factors and the individual predictors, with the undercatch factor calculated as the product of the individual spline values. Terrain elevation (Fig. 6a) emerges as the dominant predictor, with contributions that can exceed those of the two exposure metrics (Fig. 6b,c) by up to one order of magnitude, particularly in summer at higher elevations. Although this may appear counterintuitive, the pronounced seasonal dependence of the intercept factor in Fig. 6d is key to understanding this behavior. The large summer values in panel a are multiplied with intercept factors close to zero in panel d. Thus, the steep slopes of the elevation functions are required to accurately represent the onset of snowlines, while still predicting undercatch factors at the highest peaks. Since undercatch is generally more pronounced in winter, this behavior is not suggested by the GAMs during the winter months, when the smooth function contributions are multiplied by a larger intercept factor. The exposure predictors (Fig. 6b,c) tend to capture finer-scale behavior and are characterized by stronger curvature. In Fig. 6b, the particularly strong response in March (purple) is notable and is likely attributable to concavity among the predictors: the simultaneous decrease of both 51-km exposure and elevation contributions in that month is compensated by a strong increase in the 5-km exposure smooth function. We therefore interpret this feature as a model artifact. In Fig. 6c, a clear seasonal distinction is visible: winter months show increasing undercatch with positive exposure values, whereas this response saturates or even reverses in summer, when the steep elevation smooth functions dominate the correlation at high elevations."

Minor Comments

– Section 2.2 – Weather Station Data: Line 125-126: Readers outside Austria may not know what Geosphere Austria is – please explain. It is listed here in the format of a reference, but not listed in the reference list, that can be solved by spending a few more words on it.

We added "..., Austria's federal institute for geology, geophysics, climatology, and meteorology (GeoSphere Austria, 2024)."

– Section 2.5, Lines 190-194: Please explain your definition of "exposure". I do think that terrain elevation may also be a proxy for wind speed.

We added ", wind speed and precipitation amount," to terrain elevation and "(defined as the difference of topography on a 1 km grid to a smoothed topography)" to exposure as we agree, that terrain elevation also is a proxy for wind speed, although potentially to a lesser extent compared to exposure.

– Section 2.6, Line 212 – here you mention you want to account for the use of gauge totalizers in the gridded precipitation data set by Hiebl and Frei, 2018. Is there a difference in the gauges used for the gridded dataset and those for calculating the correction factors? In which way?

We added the following paragraph on the description of section 2.7 - "Historical Climate Data" to point out differences between the gridded data set and station observations for a better basis for this (later-appearing) statement:

340 "For Austria and the Swiss Engadin, the GeoSphere Austria SPARTACUS dataset was used (Hiebl and Frei, 2016, 2018). This data set provides daily meteorological information intended for climatological applications. The station observations incorporated in this data set originate from long-term, homogenized weather stations in and around Austria, including totalizers in some high-alpine regions to reduce measurement errors (Hiebl and Frei, 2018). Consequently, the stations used in this study for undercatch correction only partly overlap with those included in the gidded data set."

– Section 2.7, Line 230. Include “Figure” before 4 Done.

345 – Section 3.2, Description of figure 6: For the 5km-exposure (6b) one month is clearly different but not mentioned. Please also reconsider your choice of color. For me, it is difficult to decide which of the “blue months” is the one showing the different behavior. Further, the 51-km exposure in figure 6c shows a significant different behavior for winter months which should be mentioned and discussed in the text.

We changed the choice of colors in the Figure. Further, as described above in one of the major comments, we have reworked the discussion of the smooth functions.

350 – Section 3.3, Line 341-343 – The changed seasonality in your two catchments is a very interesting result and should be highlighted more.

We have added some additional sentences to put our findings in more context and highlight its novelty:

355 "Although analogous seasonality shifts have been reported in other wind-prone regions such as the Rocky Mountains and attributed to severe undercatch (Pan et al., 2016), and high-alpine hydrological evaluations in the Alps have arrived at similar conclusions(Pulka et al., 2024), the reduction of the dominant summer precipitation peak in Alpine catchments represents a novel finding. The plausibility of the corrected winter precipitation is therefore a central focus of the hydrological validation presented in the following section."

360 – Section 3.3, Line 351 – I guess here you mean that the pure dependency of the transfer function on wind speed is not enough in complex terrain, as first the use of your ETP gives you the wished results? I do in principle agree that complex terrain is very challenging to adjust, but I wonder if it is simply because it is more difficult to find a wind speed measurement representative for the conditions at the gauge’s orifice as a nearby wind sensor may capture different conditions and thus give a wrong correction. In any case, please clarify your argument.

We refined our statement, and included your suggestion, as follows:

365 "However, this indicates that some processes in high-alpine, wind-exposed terrains are not fully captured by the transfer function, either because the proposed relationship for the change in catch efficiency is modified by the complex, turbulence-inducing terrain, or the wind speed measurement is not representative of the conditions at the precipitation gauge orifice. We therefore conclude that the undercatch correction carries the greatest uncertainty at the highest peaks and that further research specifically targeting precipitation undercatch processes in complex terrain is needed."

References

370 Please note that I didn't check every single reference. After checking a few citations, I noticed repeatedly slight mismatches between the citation in your text and the content in the paper. Here are some examples. But I advise having a thorough check of all your references.

- Line 26: Formayer et al., 2023 presents and publishes the dataset SECURES-Met, developed for electricity model applications. Its hydropower potential parameter is based on river discharge measurements. It mentions also challenges connected with topography, but I don't think it is the right source for supporting the statement that reliable precipitation estimates become increasingly critical and have impacts on the hydropower generation in alpine regions. The word precipitation is mentioned only once in the entire article.

375 We may have extrapolated too far with this citation: The data regarding hydro power in this data set, in which some authors of our undercatch study contributed, are based on river discharge, which is calibrated with precipitation information under climate change. Therefore, we suggested this citation for our statement. Nevertheless, we replaced it with more direct citation: Wasti et al. (2022).

- Line 47: I think it is appropriate to mention other gridded data products with adjustments for precipitation in complex terrain as for example Norway and Switzerland. I added two to sources to consider, but there are possibly others: Lussana et al., 2019: seNorge_2018, daily precipitation, and temperature datasets over Norway, Earth Syst. Sci. Data, 11, 1531–1551, <https://doi.org/10.5194/essd-11-1531-2019>, 2019; Isotta et al., 2019: Long-Term Consistent Monthly Temperature and Precipitation Grid Data Sets for Switzerland Over the Past 150 Years DOI:10.1029/2018JD029910;

385 Thank you for pointing that out. We modified this section accordingly and added the suggested references:

"Some data sets address this problem by explicitly accounting for undercatch undercatch (Lussana et al., 2019) or by representing complex terrain through model-based reference fields (Lussana et al., 2019) or terrain-informed principal component patterns (Isotta et al., 2019). However, the ideal solution for tackling undercatch errors - generating gridded precipitation data using undercatch corrected stations - is not common practice. "

- Lines 89-91: inconsistency/typo? Here you are referring to Kochendorfer et al. (2017b) as source for your undercatch adjustment method, while in your methods chapter, lines 155-158, you state that you are using the undercatch correction developed from Kochendorfer et al. (2017a). Please be consistent and also check my comment on the use of the equation from Kochendorfer et al. (2017a) under major comments.

395 Thank you for your thorough reference analysis, this was indeed a typo. It should be (2017a) and is fixed in the revised version.

- Line 95: It reads as you are introducing the "exposure-dependent penalty" by citing papers Kochendorfer et al (2017a), Hiebel and Frei (2018) and Gnann et al. (2025). Kochendorfer et al. (2017a) mentions only that complex terrain can introduce a higher uncertainty when using adjustment functions, Hiebel and Frei (2018) are mentioning only once that

400

undercatch may be part of their data and Gnann et al (2025) mentions a lot of topography's influence on the development and distribution of precipitation, but also here undercatch is only mentioned once. Neither of these papers is suggesting any exposure-dependent penalty as this sentence and citation suggests. It rather seems, that the ETP, is your invention - so I guess, here a slight reformulation of the citations will do.

405 We apologise for this misleading use of references. By stating them, we wanted to highlight where potential uncertainties may stem from, as described in section 2.6. We modified the statement as followed: "We further introduce an additional exposure-dependent penalty to account for remaining uncertainties in highly exposed terrain, stemming from different sources like the use of the undercatch transfer function in complex orography (Kochendorfer et al., 2017), totalizers in the gridded base precipitation data (Hiebl and Frei, 2018) and plateauing precipitation lapse rates (Gnann et al., 2025)."

410 – Line 120: Please reference where the numbers of the glacier coverage for your catchments are taken of. Also, I was wondering if you have newer numbers than from 2003, as you at other places cite Hugonett et al. (2021) which gives you a timeseries of glacier volume (shown in figure 14)

415 We have added the reference to the glacier cover numbers. While there are more recent glacier extent data sets for Austria, we use the 2003 values taken from the Randolph Glacier Inventory (RGI) for several reasons: it covers the whole study region (case study and Austria), including the upper Engadin catchment which is not covered by the Austrian glacier inventory; and it coincides with the start of the simulation period (2001), providing a suitable initial glacier extent from which volume changes are modeled. While more recent Austrian glacier outlines exist (e.g., from 2015), using them would introduce spatial inconsistencies across regions and could not serve as initial conditions for a simulation starting in 2001.

420 – Section 2.3. - Historical Climate Data: In this entire section you refer to numerous other data sets which you are using and explain how some of the data you are using were derived or processed. Please clarify your writing to clearly indicate which of these processing steps were done by you within this study and which by the creators of the cited datasets. A representative example: Line 141, citation of Jiawai Zhuang et al., 2025, which refers to a dataset (zenodo-DOI) but is cited after the sentence: "After applying the undercatch correction, both the raw and corrected precipitation data were patch-interpolated to the study regions" - Who did the patch-interpolation and what exactly is in this dataset? Its name, when opening the link: "pangeo-data/xESMF: v0.9.2 - Third time's a charm" does not give enough information to the interested reader. Please revise the citations in this entire section to clarify.

425 We reworked this section to clarify, which steps were taken in this study and which data was taken from existing, available data sets. Regarding the xESMF citation: This Zenodo page does not describe a data set, but a software package, from which the patch interpolation originates. The citation given in the manuscript is their official recommendation on how to cite them (see the recommendation at the bottom of this page: <https://xesmf.readthedocs.io/en/latest/>). It may be inelegant, but unfortunately not in our control.

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– Section 2.7, Figure 4: There is no citation for this figure, which I assume is taken from one of the COSERO-documents?
Please add. Done.

435 – Section 2.7, Line 250: Paper Wolff et al. 2015, alongside with two others, is cited for the statement that precipitation lapse rates may be plateauing at ridges above 3000 m - and while this paper is presenting results from a 1000m-high alpine station, it doesn't include any discussion about precipitation undercatch in higher altitudes or their "lapse rate". I did not check the two others here – please do so yourself.

440 The citation of Wolff et al. (2015) was an accidental leftover in this part of the manuscript from when we wanted to generally mention increasing uncertainty in undercatch in higher elevations. Thank you for pointing that out. We apologize for that and removed it here and in other locations when aimed to support the precipitation lapse rate statement. As we are confident in the other two citations, we revised this statement as follows: "... and (3) accounts for potential plateauing of precipitation lapse rates near high ridges, where flow blocking and moisture depletion can reduce orographic enhancement (Jiang, 2003; Gnann et al., 2025)."

445 – Section 3.2., Lines 312 and 318 – you are referring to the same three studies twice for almost the same thing, the dependency of undercatch on the terrain elevation as the major descriptor (and its seasonality added in the second time) without any concrete example/comparison or differentiation. At the very least, strike one of the sentences. I do, however, suggest a more thorough discussion here, especially as all these three studies seem to discuss possibly adjustment methods for precipitation in the alpine region and you clearly take inspiration from those. How is your method differing and what is similar? Are any of the methods superior and why?

450 We have removed the first mention of the three studies due to repetitiveness and reworked the final part of section 3.2 as follows:

455 "We conclude that the GAMs produce physically plausible smooth functions that realistically capture the seasonal and elevation-dependent structure of precipitation undercatch, supporting the application of the derived factors to gridded precipitation data. While some model artifacts attributable to predictor concavity remain, the overall form of the smooth functions, their seasonal variation, and the dominant dependence on terrain elevation are consistent with findings from previous studies (Avanzi et al., 2021; Herrnegger et al., 2018; Pulka et al., 2024). Importantly, unlike these studies — which derived elevation-dependent corrections from additional in situ or satellite snow depth observations or from rainfall–runoff modelling — our approach relies solely on correcting existing meteorological station data, yet arrives at similar conclusions. Our methodology extends previous work in two key aspects: first, by replacing stepwise linear (Pulka et al., 2024; Herrnegger et al., 2018) or exponential fits (Avanzi et al., 2021) with more flexible smooth functions that better capture nonlinear relationships; and second, by providing a more detailed representation of seasonal variability through monthly correction functions, rather than a simple separation into summer and winter conditions (Herrnegger et al., 2018)."

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