Summary of major changes

We thank both the reviewers for their constructive comments. They made us really think about the limitations of our model, particularly the problems of overfitting and extrapolation errors. We revise our manuscript with the following major changes.

1. We modify the method to get rid of the multilinear regressions to predict $\bar{u}$, s, and τ, as it was prone to overfitting.
2. From an assumed balance between friction and buoyancy, and based on dimensional argument, we derive approximate estimators for the three model parameters, which we test using the data. (Sect. 3.1.3)
3. The mean wind is predicted based on a combination of mean summer temperature and the regional slope (Eq.10). The sensitivity *s* and time-scale parameter τ had limited predictability (Fig 5), so we use empirical, constant values for all glaciers (the median of the corresponding distribution of 28 best-fit values).
4. We clarify that our method cannot predict within-glacier variation of wind speeds.
5. We will add discussions on limits of applicability of the model, showing the representativeness of the set of 28 glaciers in terms of geometric and climatic variables.

The performance of the revised model in the 28 stations is similar to that of our original model (see Fig. 6 from the manuscript pasted below), but without the use of any outlier correction or ad-hoc cutoffs.

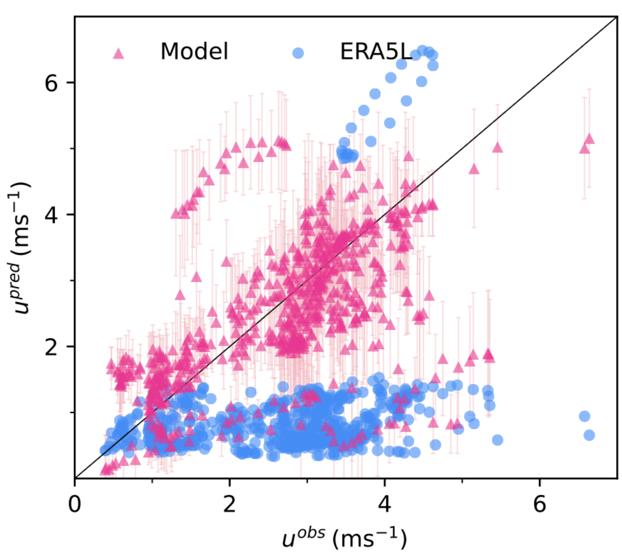


Figure 6. (a) The mean summertime diurnal wind speed predicted using Eqs. 1, 7–9 (red solid triangle) were compared with corresponding

The RMSE of the predicted wind speed had a mean (median) value of 0.87 (0.64) m/s, and an interquartile range of 0.42 – 0.88 m/s . In comparison, the ERA5L wind speed product had about two times higher mean (median) of 1.98 (2.09) m/s, and a larger interquartile range of 1.29 – 2.57 m/s

For more details, please see specific replies to each reviewer's comments below.

Response to the anonymous reviewer 1 comments on "Predictability of mean summertime diurnal winds over ungauged mountain glaciers"

General Comments

In this paper, the authors present an empirical model designed to predict the mean summertime diurnal wind speed on valley glaciers. This model, which utilises reanalysis temperature data and a small set of topographic variables, provides a promising approach to overcoming the challenges associated with sparse in-situ meteorological measurements. The potential for applying this model to ungauged glaciers is particularly noteworthy, as it enables more accurate glacier mass-balance calculations in areas where observational data are lacking.

While the authors' methodology is an interesting contribution to the field, I believe some points need further clarification.

We thank the reviewer for the overall positive comments. The point by point responses to all their comments are given below.

Specific comments

-It's necessary to specify the reference systems used in the text (see also the following comment). 'Wind speed u' is mentioned, but the reference system is not explained. Since the flow is described over slopes, I'm unsure whether it refers to north-south, planar fit, or slope coordinates.

We now clarify that *"We used a coordinate system in which z-axis points vertically up and x-axis is along the glacier flow direction."* (L119)

-Figure 1) It is not clear how this plot was created in terms of normalisation; I think it's necessary to specify this either in the caption or the text. X and Y represent the longitudinal and transverse distances, but relative to which reference system? It would also be interesting to understand how many of these stations are positioned uphill, or if they follow the glacier downstream.

The figure caption has been updated as:

”(a) Locations of 28 on- or near-glacier weather stations (red solid circles) used in this study. The circles have been artificially
spread out up to about 2◦ to avoid overlap. The number of stations in different regions are shown within light blue circles. (b) The relative, scaled locations of the weather stations used in this study (red circles) with respect to an idealised glacier (light blue rectangle) having a unit length and width. Here X and Y denotes the longitudinal and transverse distance, respectively, with respect to a coordinate system whose origin is at the headwall of the idealised glacier and X increases downglacier.

-In lines 94-96, the paper mentions that the ERA5L wind speed at 10m was not adjusted to 2m due to the complex nature of the boundary layer above glaciers. However, the model's 10m data is later used to compare with the 2m wind speed observations. While I understand this is based on data availability and what ERA5L provides, this seems inconsistent. The paper acknowledges the challenges associated with sloped flows, which complicate comparisons between wind speeds at different heights. Yet, this issue is only briefly mentioned in lines 94-96, with no further exploration of how it might affect the comparison between the two heights. I suggest that this topic be addressed in more detail in the discussion and limitations sections.

We now clarify this issue as follows (L98–101):
“The 10-m wind speed from ERA5L was used for comparison with the 2-m windspeed predicted using the model developed here. We did not attempt to correct the ERA5L wind speed to bring it to 2-m level, using the logarithmic law as is typically done in these cases (Claremar et al., 2012). Wind profiles of slope wind systems are known to have a maxima, and hence cannot be described with a logarithmic profile.”

We have also acknowledged the same in the discussion section (Sect 5.5 Limitations) as follows (L 392):
“Here we acknowledge that the ERA5L-derived 10-m wind speed is not directly comparable to the 2-m values. However, as discussed before, the interpolation of the value to 2-m level is complex”

-Technical corrections

20) You could mention 'anabatic wind' for symmetry.

The line is revised as *“Away from the glacier, as the valley floor and the hillsides are heated up during the day by solar insolation, the buoyancy forcing acts up-slope, leading to a daytime* ***anabatic*** *valley wind which flows up-valley and is relatively deeper”*

24) Who do 'they' refer to? Valley wind and glacier winds, or up-valley winds and down-valley winds? The statement is clearly true for both cases.

The line is revised as *“Glacier winds and valley winds fall under the general category of thermally driven slope winds.”*

141) This is not a bilinear regression, but rather a multivariate linear regression (?).

The line is removed as our present approach does not involve any multivariate regressions.

177) Include where these results are presented.

186) Similar to the previous point.

It is advisable to ensure that the order of panels mentioned in the text matches the order in the figure. For example, in Figure 1, panel b is mentioned before panel a.
In general, all figures should be self-explanatory. This means the caption must include explanations of the symbols used. For example, in Figure 2, all abbreviations should be introduced in the caption, and the same applies to the other figures.

All the above suggestions have been accepted in the revised manuscript.

Response to the anonymous reviewer 2 comment on "Predictability of mean summertime diurnal winds over ungauged mountain glaciers"

This manuscript presents an empirical model for estimating the summertime mean diurnal wind cycle near glaciers using reanalysis temperature and topographic variables. The model is calibrated using observations from 28 glacier and valley weather stations. Relative to ERA5-Land, the model reproduces the observed mean diurnal wind cycle, primarily by correcting biases in mean wind speed.

The analysis focuses on relating fitted model parameters to local geometric and topographic descriptors in order to enable wind-speed estimation at locations without in-situ measurements. While the approach yields improved agreement with observations, the model is highly empirical, and its general applicability relies on assumptions about parameter interpretability and extrapolation beyond the calibration sample.

We thank the reviewer for their overall positive and constructive comments. Their feedback has significantly helped us strengthen our manuscript.

Overall, the manuscript is well written, but I have concerns about the regression's generalizability and physical interpretation. I have focused the comments below on these major items.

Thanks a lot for the extremely valuable comment. We have revised the modelling strategy, and use a physically motivated predictor for the mean wind speed, which hopefully addresses the concerns. We address each of their comments below.

General comments

1. Equation 1 needs to be introduced, derived, and physically motivated. At present, it is unclear where this equation has come from. Because this model is introduced under the auspices of a linear response model, and because the authors end their introduction with "there is a clear need for a simple physically-based model of wind speed...", it was obfuscated that the model presented here is a harmonic regression. As the authors show, the response functions would be the same in the limit of small $\omega^2\tau^2$. However, if τ is a few hours (L121) this is not a valid approximation. In the results, they fit values of τ up to 6. $6 \times 0.26 \approx 2.5$, which is very much not asymptotically smaller than 1. Additionally, as this is not a linear response model, τ should not be interpreted as a response time.

We agree that the model introduction could have been clearer, emphasising the empirical nature of the model, and highlighting that it is not a linear-response model, despite the apparent similarity. A first-principle derivation of the model is ideally needed. However, in general, deriving a linear response model is difficult for complex problems.

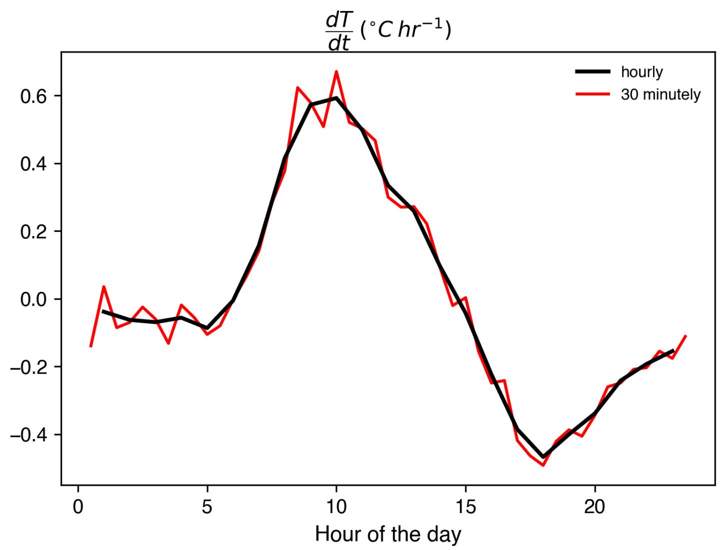
In the revised manuscript we have made the following changes:

- We clarify that Eq. 1 is an empirical model, which is not a linear-response model, but one that is motivated by them (L127-129).
- We discuss that the model reduces to a linear response model at the $\omega^2\tau^2 \ll 1$ limit (L129-131). Also, the large value of τ at Pyramid station was due to an error in the code, which we fixed. The present maximum value is 3.4 hours, which gives a slightly more respectable $\omega^2\tau^2 = 0.8$. Moreover, 77% of the τ values are less than 2 hours, so that $\omega^2\tau^2 < 0.3$.
- We acknowledge that a derivation of the present model starting from, say, the exact Prandtl model of steady glacier wind, is missing (L359-361).
- We agree that τ is not a response time, and we refer to it as the 'time-scale parameter' throughout.

2. The uncertainty estimation written in 3.4 assumes the covariance of $\bar{u}$, s, and τ is 0. This is unlikely to be true since they are derived from the same regression, so this uncertainty

is an underestimate. Other sources of uncertainty, such as error induced by the finite difference approximation and the ad-hoc handling of outliers, should be considered.

- We have modified our methodology to estimate uncertainty in the revised manuscript. Now we do a Monte-carlo simulation where the model parameters are assumed to vary within a gaussian distribution (section 3.3).
- In the above we add independent Gaussian noise to the three variables as we verified ū–s,s–τ,ū–τ are not correlated even at $p<0.1$ level for the 28 stations.
- We have checked for a few stations with subhourly data that the time derivatives estimated at hourly and subhourly scales are consistent, except for a larger noise at smaller timescales. (Figure below shows the example of Hinterisferner glacier).
- No ad-hoc cutoffs have been used in the present revised version.



3. The model selection procedure for eq 7–9 searches over a very large space of predictor combinations, but it is unclear whether predictor selection is nested within cross-validation. If variable selection is performed prior to LOOCV, then the cross-validation performance is likely overestimated.

   We clarify this issue now as follows:
   "Note that the LOOCV results may vary depending on whether we search for the best set of predictors at each iteration. However, the present estimates are likely to be conservative, as we may not be working with the best model in some of the iterations, which is likely to increase the RMSEs (L195-196)*"*

4. The applicability of the regressions in eq 7–9 requires more discussion and analysis. How do the distributions of gauged and ungauged AR, R1, R5, Z10, Zs, and S0.1 compare? Is the training sample representative of the population? Would this model

produce realistic results for most ungauged glaciers? Glaciers with long-term measurements tend to be biased to be more accessible and shallower, and already 7% of the training data had to be discarded or modified.

This is an excellent point, which we consider now. This will strengthen the paper, and we thank the reviewer for this. We have added an “Applicability of our model” subsection under the discussion (Sect. 5.4), where we have discussed if the set of 28 glaciers used here is representative or not. We have provided ranges of six parameters (eg, area, slope, …) that this set spans (L340-348).

We found out that our station distribution was indeed biased towards larger and gently sloping glaciers (see figure below).

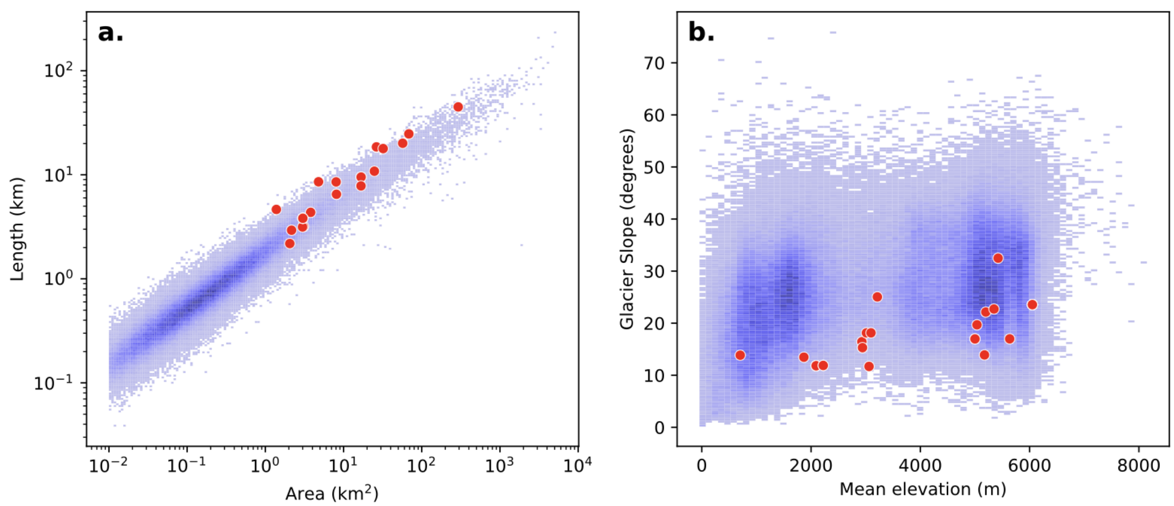


Figure 9. (a) The distribution of glacier length and glacier area for glaciers in this study (red dots) compared with that of the global population of glaciers (blue dots). Similar comparison for mean glacier elevation and glacier slope is shown in (b). Our stations are biased towards larger and gently sloping glaciers, suggesting the possibility of extrapolation errors for smaller steep glaciers.

5. A clear physical interpretation of the regression results is required, and compared to examples in literature. For example, what conceptual model can one use to understand why the 1 km relief increases ū, while the 5 km relief decreases it? I'll admit that I am somewhat skeptical of the usability of such a result. While the regression is identifying relationships in the data, it is difficult to interpret these as causal or physically grounded mechanisms rather than empirical correlations. It is surprising, for example, that position along the slope plays no role, since glacier winds are known to strengthen downslope.

Thanks a lot for this great comment, which motivated us to change our approach. Now in the first step, we use a physically motivated combinations of relevant variables to come up with minimal predictors for the model parameters - largely based on dimensional arguments (3.1.3).

In the next step, we find the best predictors, and evaluate its performance carefully – including visual inspection, and the effect of removal of outliers, etc. In case the performance is deemed unreliable, we simply use a constant value for the model parameter, chosen to be the median of the distribution of the 28 best-fit values. This is now illustrated in the figure 5 of the manuscript:

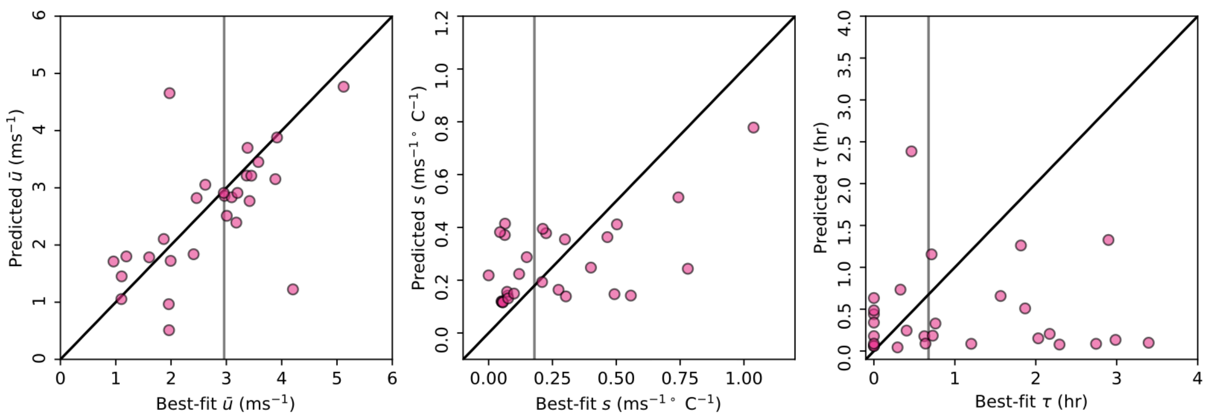
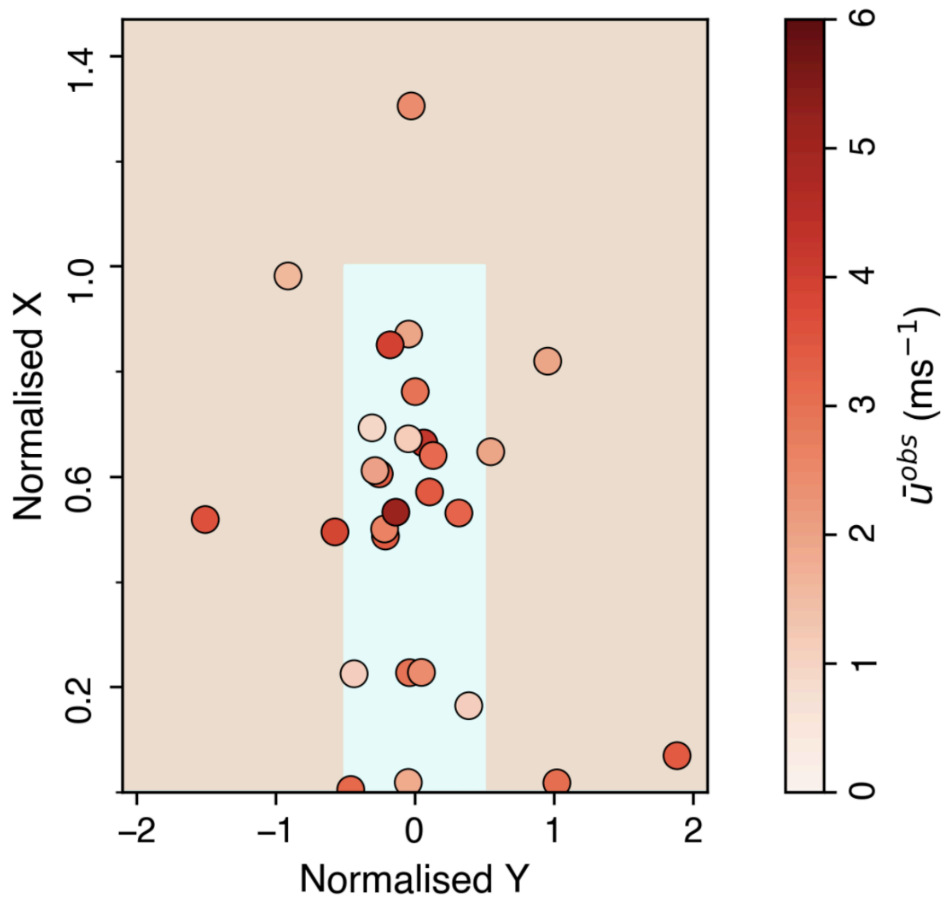


Figure 5. The value of model parameters predicted by Eqs. 2-4 are plotted against their observed best-fit values from all 28 stations. The grey vertical line shows the median value of each model parameter across all stations. a) The predictor $\sqrt{\frac{|\bar{T}|}{\varsigma_{10}}}$ was found to have a strong relationship with $\bar{u}$, with an R^2 of 0.9 (p<0.001). b) For s, the predictor $1/\sqrt{|\bar{T}|/\varsigma_{10}}$ was found to have a significant relationship with an R^2 of 0.57 (p<0.001), but upon removal of the data point marked with arrow, the relationship became insignificant. c) None of the predictors discussed in sect. 4.1.2 showed a significant relation with τ. Due to the lack of predictability of s and tau, we used the corresponding median values to estimate them for ungauged glaciers, which does not capture variability of these quantities between glaciers.

Please see the Supplementary Fig. S8 (pasted below) for the observed mean wind speeds and the relative station location. No discernible spatial pattern for winds is evident in the available dataset. Moreover, only four out of 18 glaciers studied here had more than one station in the same valley. As a result, our model cannot be expected to capture within-valley variability in wind speeds, even as it captures the variability between different valleys.



Supplementary Figure S8 . The observed mean wind speeds (shades of red) of 28 weather stations used in this study, with their relative locations (circles) shown on an idealised glacier (light blue rectangle) of unit length and width (similar to Figure 1b). No clear spatial pattern for winds could be seen from our dataset.

6. Most flux models split the glacier into separate elevation bands due to known changes in u & T along the glacier. The predicted u will change along the glacier as R1, R5, Z10, Zs, and S0.1 change. How do the predicted along-slope profiles of u behave on these glaciers? Is it always physically valid?

   We acknowledge this limitation. Please also refer to the previous response. At present, we recommend use of the model to estimate a single prediction for an entire glacier, focussing on the glacier-to-glacier variation.

7. As the authors note, a lot of the performance improvement in reducing the underestimation comes from the intercept of the regression. Where does this intercept come from? Is it related to any climatological variables? In light of this, I'm not sure RMSE is always the most honest metric throughout -- the RMSE will always improve because of the intercept being added to u.

The revised parameterisation (Eq. 7-9) does not use any intercept.

8. A more careful analysis of the role of τ is required. The three most performant models are identical save for changes in τ. But the relation between τ and S0.1 in figure 6a is not very convincing. From table S3, the correlation is 0.24 and the t-statistics are less than 1.5. Is the model really markedly better with the inclusion of τ? Why would we expect the phase and slope should be related here?

We accept that the observed variability of τ cannot be explained well by our approach, and as such we do not attempt to estimate the variability. We have acknowledged this in the discussion:

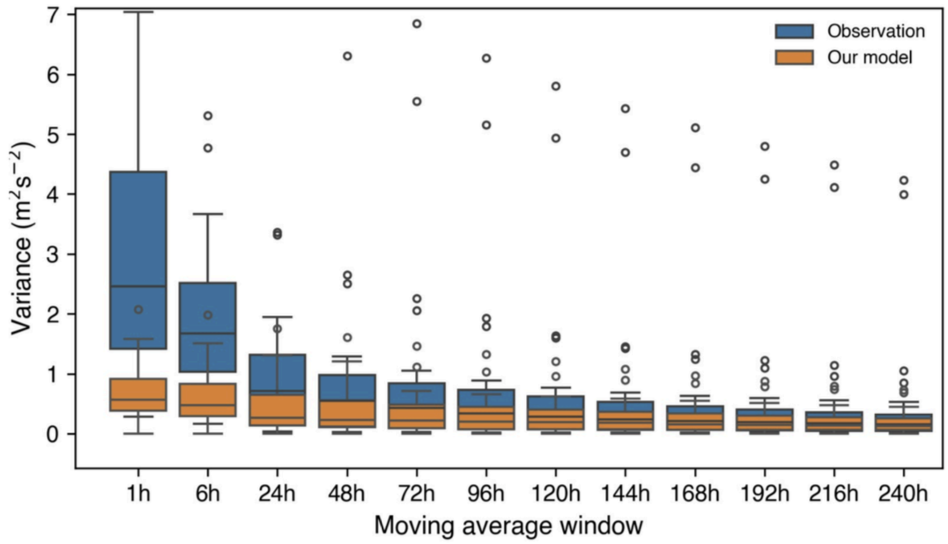
"In fact, most of the improvements in wind speed predicted by the present model, over that obtained from ERA5L, was due to the ability of the present model to capture the variability of the mean wind speed $\bar{u}$ between stations (Sect 4.1.2). Perhaps with more data from a larger set of stations, having a higher time resolution, can help improve the predictability of the remaining parameters." (L294-296)

"In contrast, both the present model and ERA5L had similar mean RMSEs for the diurnal anomaly u_d, indicating that there is scope for improving the estimates of the model parameters s and τ. As can be seen from the Supplementary figure S2, 64% of the stations have a τ less than the time resolution of our data (1 hour). This might explain why it is difficult to parameterise the variability of τ." (L394-397)

9. The time-scale analysis should be clarified. It is unclear how a diurnal parameterization could be meaningful at multi-day time scales. I suspect the improvement comes from more smoothing and as a result, reduced variance.

We thank the reviewer for this suggestion. We have incorporated the following lines in the discussion, and discussed in Supplementary Fig. S6:

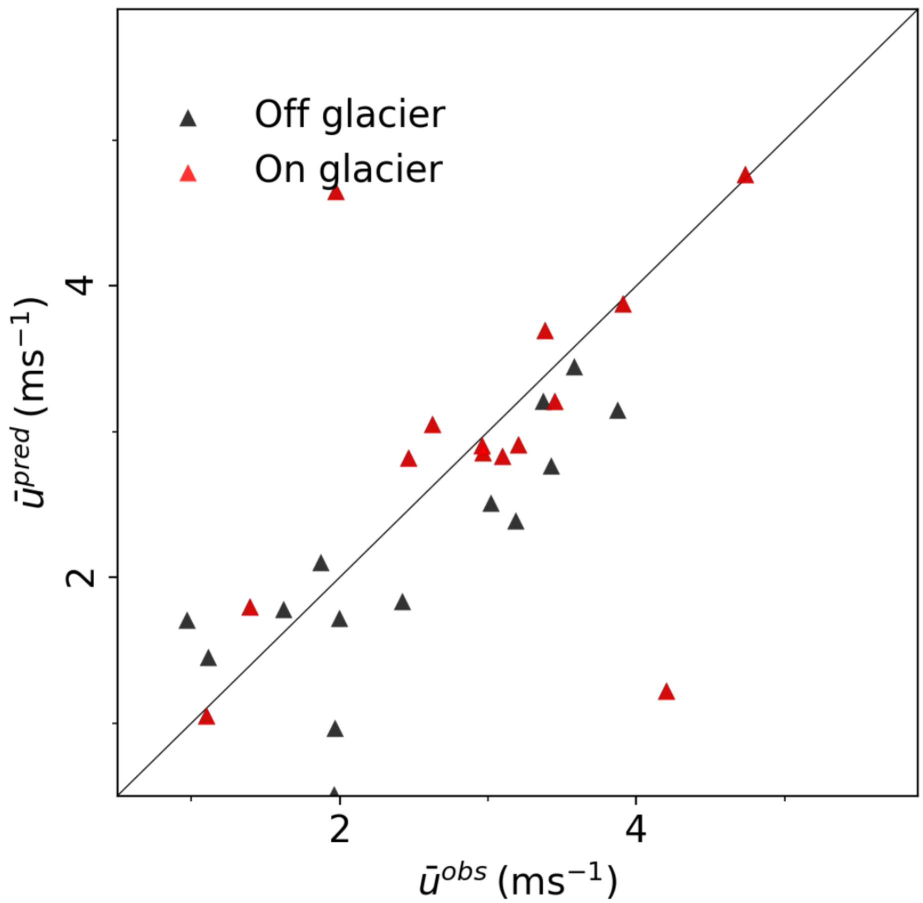
"An analysis of variance of observed and predicted wind speed data across different time-scales (Supplementary Fig.\,S6) revealed that this improvement in prediction is not simply due to a reduction in variance due to higher smoothing." (L309-311)



Supplementary Figure S6 . The variance of observed and predicted wind speeds for different time averaging windows from 1 hour to 240 hours across the 28 stations. While the variance of the observed wind speeds came down as the time averaging length increased, variance of predicted winds remain more or less constant across all time-averaging windows. This illustrates that the improvement in wind speed prediction by our model at shorter time-scales (Sect. 5.2.1) is not simply due to a reduction in variance.

10. The distinction between valley wind and glacier wind merits more analysis. Given the difference in scale between the two, consistent model performance between them surprises me. How do the regressions change if only run on on-glacier vs off-glacier observations?

The model performance is similar for the on-glacier stations as compared to the off-glacier stations. This is now discussed in Supplementary Fig. S5, pasted below.



Supplementary Figure S5 . Prediction of mean wind speed versus their best-fit values across 28 stations are shown as red and black triangles. The model prediction for on-glacier stations are coloured red, and that for off-glacier stations is coloured black. The performance of the model was similar for both the subsets of stations.

11. Are the heat fluxes from ERA5-L being estimated using 2-m temperature and 10-m wind speed? If so, the exchange coefficients will need to reflect this.

We have clarified this point in data section L99-101:

"The 10-m wind speed from ERA5L was used for comparison with the 2-m windspeed predicted using the model developed here. We did not attempt to correct the ERA5L wind speed to bring it to 2-m level, using the logarithmic law as is typically done in these cases \citep{claremar2012sval}. Wind profiles of slope wind systems are known to have a maxima, and hence cannot be described with a logarithmic profile."

12. Because turbulent heat fluxes are nonlinear functions of u & T, computing fluxes from seasonal-mean wind speed and temperature does not, in general, recover the seasonal-mean flux. Some discussion of this is needed.

We have recalculated the fluxes using hourly wind speed and temperature for the entire summer season, and the results are similar (see Figure 8, which we paste below).

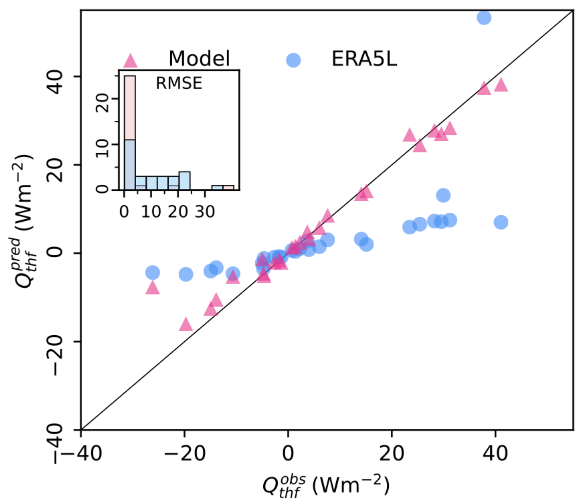


Figure 8. Total turbulent heat flux Q_{thf}^{pred} derived using the mean wind speed predicted in this study (red triangles), and those from ERA5L (blue circles) are compared with the turbulent heat flux Q_{thf}^{obs} computed using observed wind speed. Inset shows the corresponding distributions of RMSEs for the 28 stations.

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