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 remove the multilinear regressions which we had used previously to predict ū, s, and τ.
2. We argue that a balance between friction and buoyancy can be expressed as $\bar{u}^2 \times \mathrm{Slope} \sim \mathrm{T}$. Then we verify this from our data and estimate ū as $\bar{u} = \beta^u \sqrt{T/\mathrm{Slope}}$, where T refers to mean summer temperature derived from ERA5L, and Slope is the regional slope of a 10x10 km grid box around the glacier centroid.
3. Sensitivity is redefined by scaling out the diurnal temperature amplitude. The redefined sensitivity has limited predictability, so we use a constant value for all glaciers.
4. Similarly, the time-scale parameter τ has limited predictability and a constant value is used for all glaciers.
5. In both the above cases, the median values obtained for the 28 glaciers are used.
6. We clarify that our method cannot predict within glacier variation of wind speeds, and we predict only a constant value for the entire glacier.
7. We will add discussions on limits of applicability of the model, showing the representativeness of the set of 28 glaciers.

We model the mean diurnal wind speed as:

$$u = \bar{u} + s\,T_d - s\tau\frac{dT_d}{dt}$$

(Eq.1)

- We estimate ū as follows:

$$\bar{u} = \beta^u \sqrt{T/\mathrm{Slope}}$$

(Eq. 2)

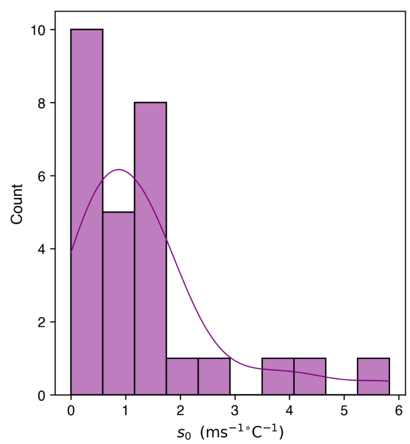
where T refers to mean summer temperature derived from ERA5L, and Slope is the regional slope of a NxN km grid box around the glacier centroid.

- We model s as follows:

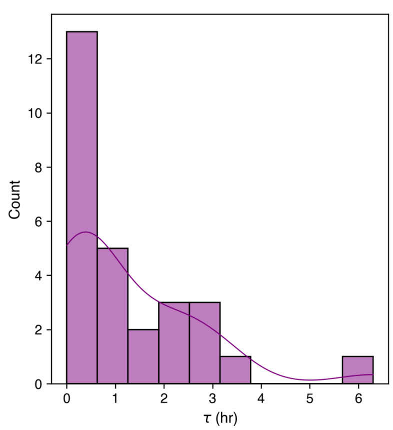
$$s = \frac{s_0}{\Delta T}$$

(Eq.3)

where ΔT is the mean diurnal temperature range derived from ERA5L. The value of s_0 for the 28 stations has a median value of 1.1 m/s, with an interquartile range of 0.5-1.6 m/s (See figure below). Hence we estimate s with a constant s_0, calculated as the median s_0 from all stations.



- We model τ as the median of its observed best fit values from the 28 stations.



- The errors of predicted winds are calculated based on standard error of regression of ū, and the interquartile ranges of the sensitivity and time-scale parameters.

The performance of the revised model in the 28 stations is similar to that of our original model without the use of any outlier correction or ad-hoc cutoffs. The RMSE of the predicted wind speed had a mean (median) value of 0.81 (0.58) m/s, and an interquartile range of 0.34 – 0.81 m/s (Fig. 1). In comparison, the ERA5L wind speed product had about two times higher mean (median) of 1.87 (2.03) m/s, and a larger interquartile range of 1.29 – 2.57 m/s.

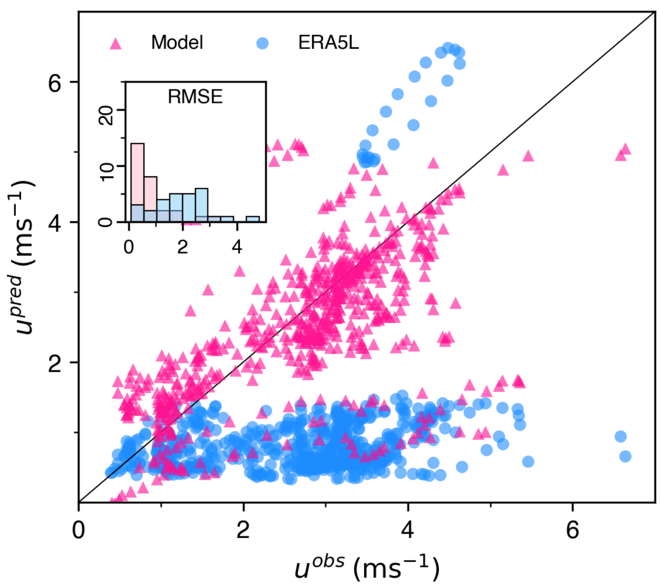


Figure: The mean summertime diurnal wind speed predicted using Eqs. 1–3 (red solid triangle) were compared with corresponding observation. The corresponding comparison for ERA5L wind speed is also shown (blue solid circles). The inset shows the distribution of RMSEs for 28 stations for both our model (shown as light pink histogram) and ERA5L (shown as light blue histogram).

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 use a gravity aligned coordinate system in which gravity is along the z-direction. If we denote the 2-m wind speed using the vector $v = v_x\hat{i} + v_y\hat{j} + v_z\hat{k}$, the magnitude of horizontal component of wind speed ($v = \sqrt{v_x^2 + v_y^2}$), perpendicular to gravity, is denoted by u. In our model, we do not assume a wind direction, although in general the winds are either along the up-slope or down-slope direction.

-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.
All changes will be accepted in the revised manuscript.

-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.
Wind speed at 10 m height is typically brought down to 2m height by assuming a logarithmic law of the wall wind profile. Wind profiles of slope wind systems are known to have a maxima, and hence cannot be described with a logarithmic profile. Assuming a logarithmic profile would

introduce new biases, so we choose not to correct the winds for elevation differences. We will add a discussion about this in the revised manuscript.

-Technical corrections

20) You could mention 'anabatic wind' for symmetry.
The line: “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 up-valley valley wind that is relatively deeper ” now reads “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 “Both of these winds fall under the general category of thermally driven slope winds.” → “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 (?).
Yes, this is a multivariate regression in two variables. We will clarify this in the revised manuscript.
177) Include where these results are presented.
186) Similar to the previous point.
All suggestions will be accepted in the revised manuscript.
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.
All suggestions will be accepted in the revised manuscript.
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 suggestions will be 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.

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.

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

We propose to clarify in the revised version the following:

- Ours is a completely empirical model which is not a linear response model.
- While the model does reduce to a linear response model at the $\omega^2\tau^2 << 1$ limit, this limit is not valid for some of the stations considered (See FigR1 below).
- Acknowledge that a derivation of the present model starting from, say, the exact Prandtl model of glacier wind, is missing.
- τ is indeed not the response time and we shall define it as the 'time-scale parameter' throughout.

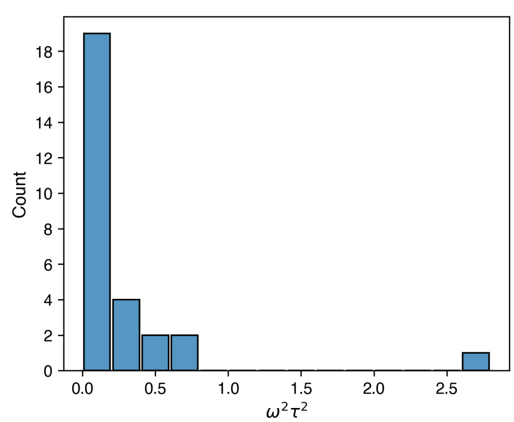
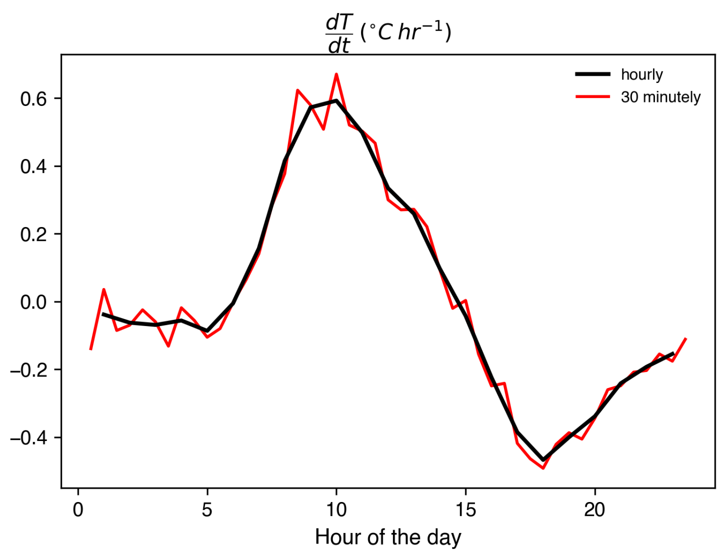


Fig. 1: The distribution of $\omega^2\tau^2$ for the 28 stations

2. The uncertainty estimation written in 3.4 assumes the covariance of ū, 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.

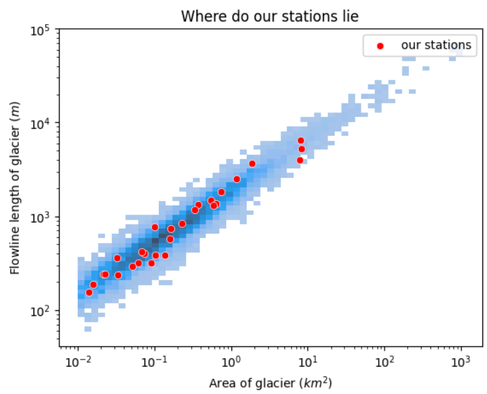
- ū–s,s–τ,ū–τ are not correlated even at $p<0.1$.
- We have checked for several stations with subhourly data that the time derivatives estimated at hourly and subhourly scales are consistent, except for a larger noise at smaller timescales. (Figure below shows the example of Hinterisferner glacier)
- No ad-hoc cutoffs to be used in the revised version.

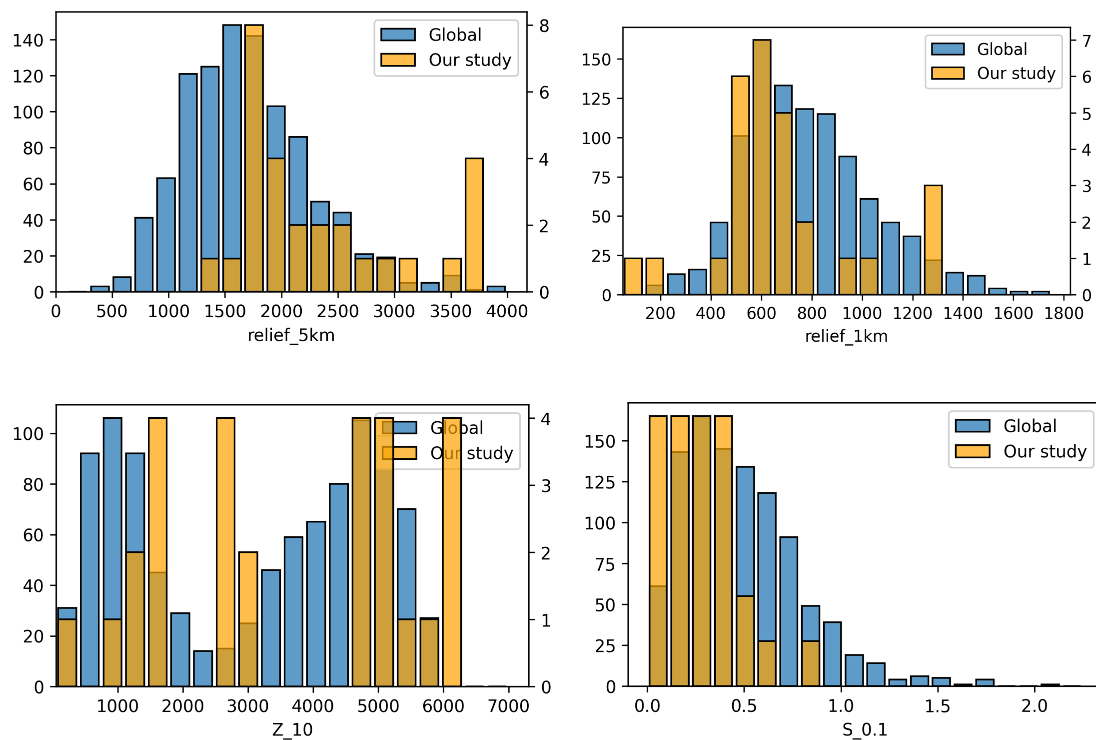


This discussion will be added to the revised paper.

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.
   - In the revised model, the predictor selection will be done only for the Slope parameter.
   - We will perform this analysis in the revised paper and check its effect on the model performance.
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 will strengthen the paper, and we thank the reviewer for this. We shall add an “applicability” subsection under the discussion, where we discuss if the set of 28 glaciers used here is a representative one. In the figure below, the blue histogram shows the distribution of static variables of 1000 randomly chosen glaciers, whereas the red points/ orange histogram shows the same for our stations. While this analysis suggests for most of the variables, the set is a reasonable sample of global glaciers, there is a possibility of extrapolation errors for cases like 5 km relief <1500m, for example.





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.

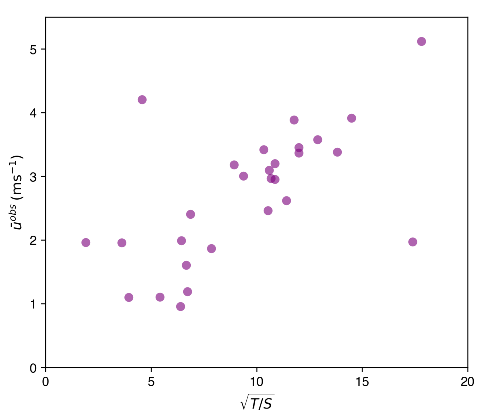
Following the reviewer's comment, we now use a physically motivated static variable to predict the mean wind speed. At steady state, frictional force is in balance with buoyancy for thermally induced slope flows (Oerlemans, 2001).

The vertical flux of horizontal momentum scales as the product of horizontal and vertical wind speeds. The horizontal buoyant force scales with the temperature difference between ambient atmosphere, and the glacier surface (which is at zero).

$$u^2 Slope \sim T$$

Where u is wind speed, S is slope of glacier surface and T is the absolute value of ambient temperature.

We test this relation from our dataset, and observe that \sqrt(T/S) has a correlation of 0.62, and a statistically significant pvalue=0.0004 (see Figure below). This is used as the sole predictor of mean wind speed in the revised model.



- Glacier winds have been shown to strengthen, remain the same, or weaken along the glacier flowline. Also, only four out of 18 glaciers used in our study had more than one station in the same valley. Since we do not have enough data to verify the along-slope profiles, we do not attempt to capture within-glacier variation with our revised model.

[7] We thank the reviewer for this helpful comment. Figure X shows the observed mean wind speeds for each station and the relative station location on an idealised glacier of unit width and length. No discernible spatial pattern for winds is evident in our dataset. It should be noted, however, that only four out of 18 glaciers in our study had more than one station in the same valley. As a result, our model is better trained to capture basin-to-basin variability in wind speeds than the variability within individual basins.

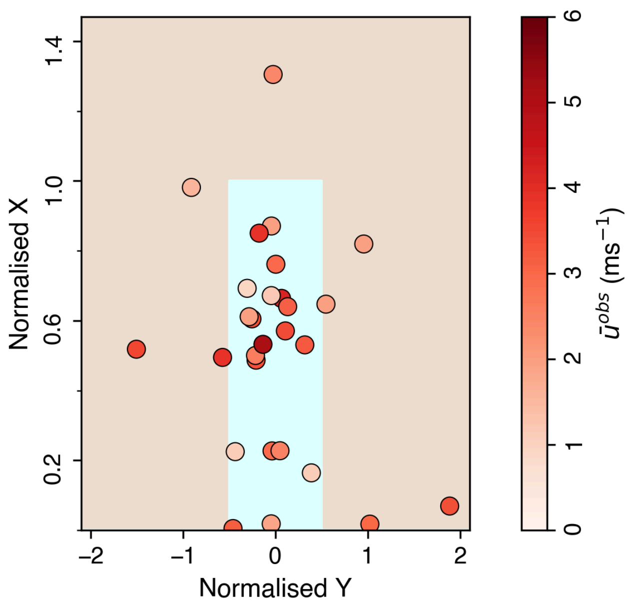
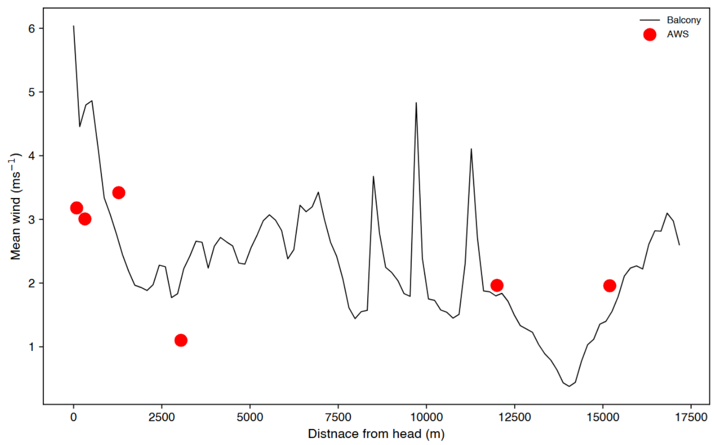


Figure. The observed mean wind speeds (shades of red) of 28 weather stations used in this study, with their relative locations (circles) shown on an idealised glacier (light blue rectangle) of unit length and width.

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 evaluated the along-slope profile of ū predicted using our model (See figure below for the example of Khumbu glacier). For some parts of the profile, the wind speeds are negative, which is unphysical. Also, only four out of 18 glaciers used in our study had more than one station in the same valley. Since we do not have enough data to verify the along-slope profiles, we do not attempt to capture within-glacier variation with our revised model.



We find some studies where the wind speed remains constant in magnitude along the flowline and some cases where it strengthens downglacier.

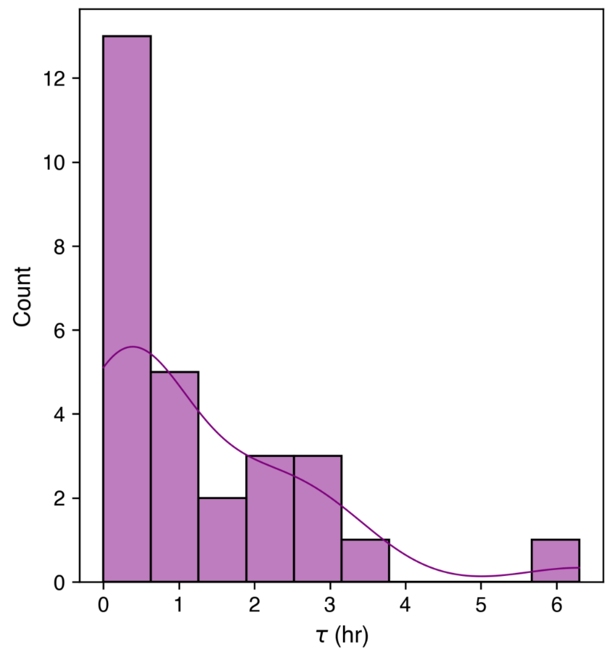
Strasser, U., Corripio, J., Pellicciotti, F., Burlando, P., Brock, B., & Funk, M. (2004). Spatial and temporal variability of meteorological variables at Haut Glacier d'Arolla (Switzerland) during the ablation season 2001: Measurements and simulations. Journal of Geophysical Research: Atmospheres, 109(D3).
Greuell, W., Knap, W. H., & Smeets, P. C. (1997). Elevational changes in meteorological variables along a midlatitude glacier during summer. *Journal of Geophysical Research: Atmospheres*, *102*(D22), 25941-25954.

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 model does not have an intercept, yet produces a similar RMSE (0.92 m/s).

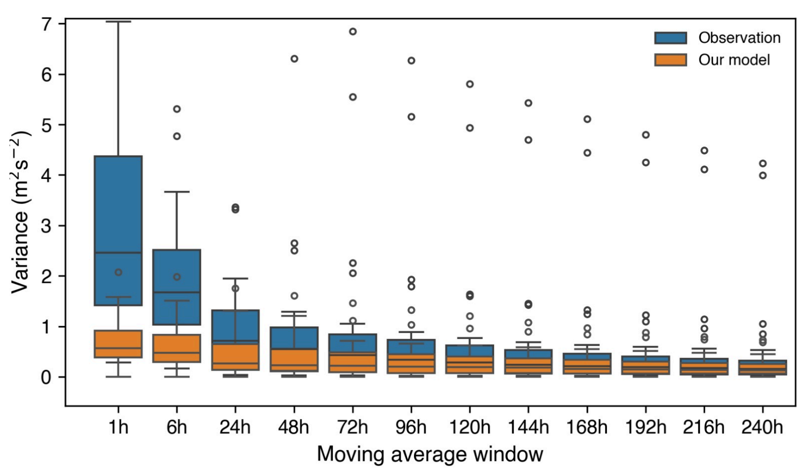
8. A more careful analysis of the role of τ is required. The three most performant models are identical save for changes in τ. But the relation between τ and S0.1 in figure 6a is not very convincing. From table S3, the correlation is 0.24 and the t-statistics are less than 1.5. Is the model really markedly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

We accept that the variability of observed τ cannot be explained well by our model, nor can any physical explanation be given for its predictors. As can be seen from the figure below, 64% of the stations have a τ less than the time resolution of our data (1 hour). Hence our model fails to capture the variability of τ . We have replaced the regression model for τ with a median τ for all 28 stations in the revised paper.



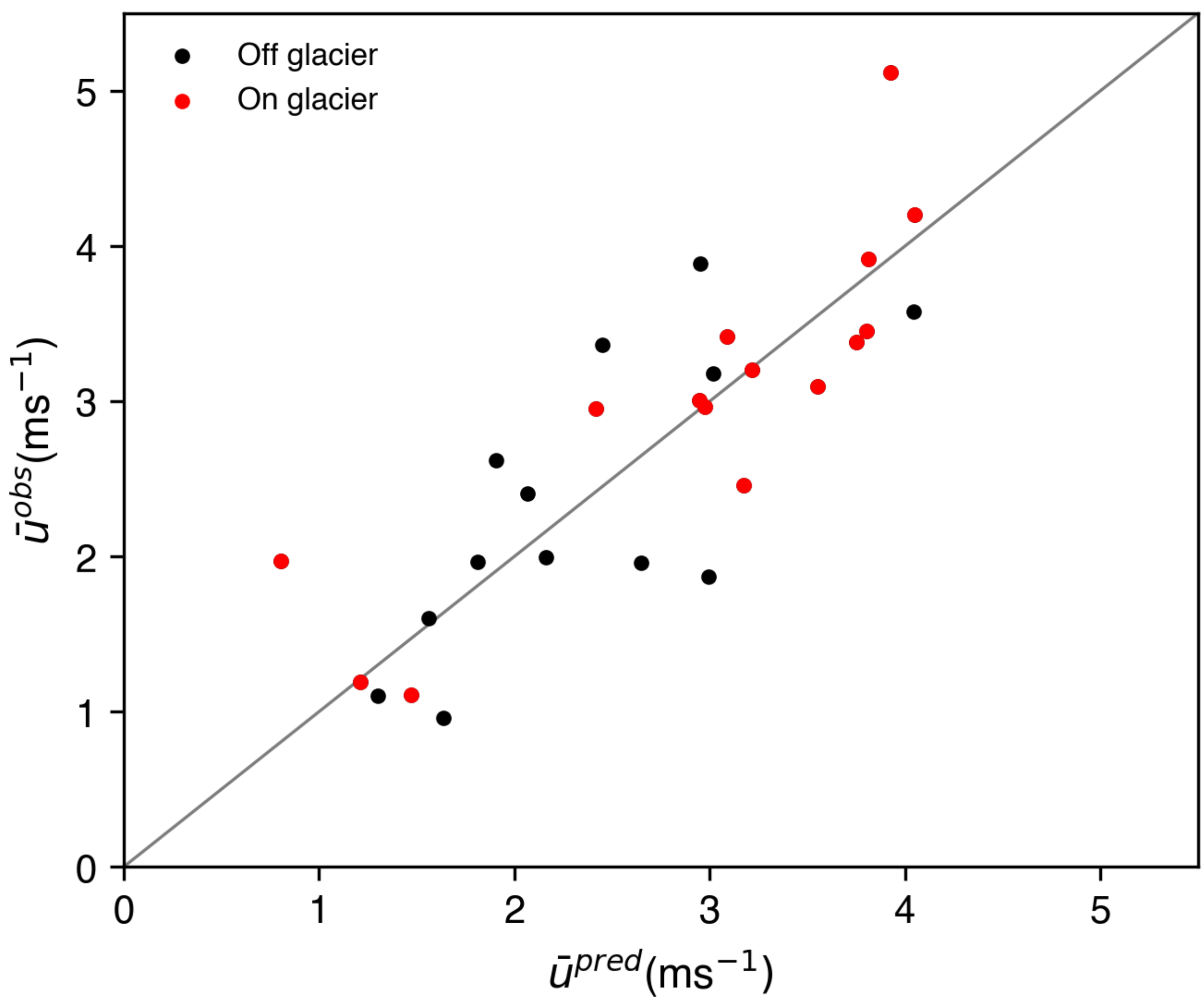
9. The time-scale analysis should be clarified. It is unclear how a diurnal parameterization could be meaningful at multi-day time scales. I suspect the improvement comes from more smoothing and as a result, reduced variance.

We calculated the variance of observed and predicted wind speeds for various time averaging windows from 1 hour to 240 hours (See figure below). While it is true that some part of the RMSE improvement is due to the reduced variance, it does not entirely explain it.



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 slightly better, if not similar, for on-glacier stations as compared to off-glacier stations (Figure below).

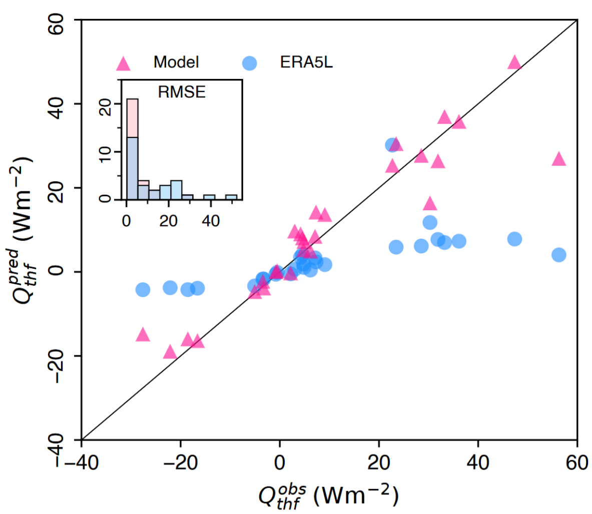


11. Are the heat fluxes from ERA5-L being estimated using 2-m temperature and 10-m wind speed? If so, the exchange coefficients will need to reflect this.

ERA5L 10-m wind speeds are often adjusted to 2-m height using a logarithmic law-of-the-wall profile. However, slope wind systems over glaciers are known to exhibit a wind-speed maxima, which cannot be captured using a logarithmic profile. Applying such a correction would therefore introduce additional, physically unjustified biases. For this reason, we do not adjust ERA5L winds for height differences and instead use the 10-m ERA5L wind speeds as provided.

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 SX below). This will be updated in the revised manuscript.



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Oerlemans, J. (2001). Glaciers and Climate Change (1st ed.). CRC Press. https://doi.org/10.1201/9781003760672