Response to Cole Lord-May (referee #2)

Investigating the influence of changing ice surfaces on gravity wave formation and glacier boundary-layer flow with large-eddy simulations

Brigitta Goger, Lindsey Nicholson, Matthis Ouy, and Ivana Stiperski

November 21, 2024

Dear Cole Lord-May,

We would like to thank you for the thorough evaluation of our manuscript. Below we address our detailed responses to all the comments. In this response document we try to clarify and address each of your suggestions, comments, and questions by you. Therefore we have copied the comments in blue boxes and have addressed them one by one. In the response we use italic fonts to quote text from the revised manuscript. In some cases, we explain why we didn't concur with your suggestion. In addition to the revised manuscript, we have uploaded a version of the manuscript with highlighted track changes that indicate where the manuscript has changed (**red**=changes made in the revised manuscript).

Best regards,
Brigitta Goger, Lindsey Nicholson, Matthis Ouy, and Ivana Stiperski

> Building upon a previous study, the authors present three 6 h large-eddy simulations of glacier-atmosphere interactions above Hintereisferner (HEF). In their case studies, ice surfaces are replaced with bare rock to better understand how stabilizing conditions at a larger glacier-system scale impact smaller outlet glaciers. The authors find that upstream glaciers act as a strong control on the outlet glacier, altering local gravity wave breakup, wind patterns, and spatiotemporal heat flux distributions. The manuscript serves as a great numerical complement to the hypotheses laid out by Conway et al. (2021), and explores questions I've often asked when standing on the outlet glaciers of large icefields. The authors present some interesting insights into the role of upstream glaciers on local conditions, and in turn, on the future of glacier-scale surface energy balance modelling. While implications of their results are very compelling, the presentation could be improved to better sell the results themselves. Throughout, I suggest a better quantification of the results, as many of the conclusions drawn rely on the reader's ability to visually differentiate between spatial maps. Additionally, I'm wary of the use of WRF in resolving the near-surface glacier boundary layer, so a clearer statement of assumptions and exploration of limitations is needed to trust the model outputs. Please contact me if you have any questions. I'm happy to provide the `.tex` file if it makes the response easier.

We thank you for the encouraging comments, especially for the fundamental questions on the underlying assumptions, and we will address the concerns below.

Major comments

> G1 A careful re-read for spelling and syntax throughout would improve the manuscript.

Thank you for the comment. We re-read the revised manuscript thoroughly and hope that the readability improved and no typos are present anymore.

> G2 Answering the second and third research questions hinges on WRF accurately resolving the stable boundary layer above the glacier surface. As stated in L324, this is a questionable assumption (although L55 suggests otherwise). As the authors found in Goger et al. (2022), the sensible heat flux was overestimated by roughly 2×. Although their grid is more coarse, Draeger et al. (2024) found an underestimation of sensible heat fluxes,

but also found that the results varied significantly depending on the model parameterization scheme chosen. As accurate modelling of the glacier boundary layer is paramount to the results presented, the manuscript would benefit greatly from a systematic enumeration of assumptions and limitations, and a clear argument of why the model outputs are to be trusted. Many of my specific comments throughout are related to this point.

- Thank you for this remark, which is also in accordance with referee #1's questions. We think we did not accurately emphasize that in our case study, the katabatic glacier boundary layer is either heavily disturbed or completely eroded, leading to neutral (REF, NO_UP) or even convective (NO_GL) vertical profiles. (cf. Fig. 4a-d in the manuscript). Therefore, we assume that on this case study day, larger-scale processes, i.e., the gravity wave, dominate over the local glacier boundary layer and ultimately erode it. The current LES setup is able to represent the relevant large-scale dynamical processes, while we agree, that small-scale glacier boundary layers are very likely not resolved appropriately on the grid. We would also like to mention at this point that we could very likely not use our model setup to investigate katabatically-driven flow regimes, because our model's grid spacing is too coarse and because underlying assumptions (e.g., MOST) break down in katabatically-driven situations. We added a paragraph on the model's limitations in Section 2: *"At the current horizontal grid spacing (Δx=48 m) not all scales in the LES are resolved equally well. As for all real-case LES, the scale separation problem (isolating the smaller from the larger scales) is inherent (Schemann et al., 2020). This leads to a 'better' representation of the larger scales (e.g., the dynamically-induced gravity wave), which is also evident in the NW day simulation of Goger et al. (2022), where they noted a too strong erosion of the glacier boundary layer by the gravity wave. Furthermore, we cannot expect that the small-scale stable boundary layer over HEF is resolved accordingly (both in the vertical and horizontal), because according to Cuxart (2015), a horizontal grid spacing of less than 10 m is necessary to simulate stable boundary layers in a realistic way. Still, since we focus in this study mostly on dynamically-driven processes, we think that we can provide important information on the impact of gravity wave formation on the glacier boundary layer flow development."*

- Indeed, the sensible heat flux is overestimated in REF (Goger et al., 2022), and we argued that this is related to the generally overestimated horizontal wind speeds in the model, a common bias in real-case LES over complex terrain with the WRF model (also found in Gerber et al., 2018; Umek et al., 2021; Liu et al., 2020). Despite this wind speed overestimation, a model validation with eddy-covariance observations showed other parameters like non-stationarity of sensible heat fluxes, the relation between sensible heat flux and wind speed, horizontal advection patterns, and turbulence kinetic energy were simulated in a realistic way. Therefore, we concluded that the underlying relevant physics are represented well in the 'NW day' simulations, the base for the current study.

- Draeger et al. (2024) ran their simulations on a coarser grid requiring a complete paramterization of the atmospheric boundary layer. Boundary layer paramterizations include simplified assumptions (e.g., assuming horizontally homogeneous and flat terrain, or excuding 3D effects; Goger et al., 2018, 2019), and these assumptions are clearly violated over complex terrain. To overcome these problems, we ran our simulations on a grid of $\Delta x = 48$ m, allowing us to use a LES closure instead to avoid the shortcomings of boundary-layer paramterizations. The Deardorff (1980) LES closure has the underlying assumption that the largest turbulent eddies are already resolved on the model grid. While this is not the case for small-scale stable boundary layers Cuxart (2015), we can assume that this criterion is met in our case study with $\Delta x = 48$ m where large-scale flows dominate. We would like to refer the referee to the detailed discussion in Goger et al. (2022), where we discuss all the aforementioned issues.

G3 Language like "deglaciation" or "removal of ice surfaces" is used to explain the case studies. How is this done? Is it a removal or a replacement? If the former, more explanation of how the underlying topography is inferred would be needed. If the latter, I would suggest changing the terminology and stating all of the ways in which this affects the WRF simulation (roughness lengths, temperature boundary condition, etc.).

- We agree that we have to be more consistent with our choice of words. Since "deglaciation" usually refers to glacier melting under a changing climate, and since our set-up does not follow realistic future glacier melting patterns, we decided to replace "degaliation" with "removing ice surfaces" in the revised manuscript.

- From the technical viewpoint, the model topography does not change by removing the glaciers in the domain. While in realtiy, melting glaciers lead to surface height change even within days (Voordendag et al., 2023), these surface height changes are not present in the current topography dataset we use (USGS, 2000).

Land-use category	Albedo (%)	Moisture availability (%)	Emissivity (% at 9 μm)	z_0 (cm)	Thermal inertia ($1\ \mathrm{W\,m^{-2}\,k^{-1}\ s^{1/2}}$)
Snow or ice	41.5	95	96.1	5	418
Bare rock	16.9	2	96.5	10	2948

Table 1: Surface parameters from the CORINE dataset for the two land-use categories 'snow or ice' and 'bare rock' for the summer season after Pineda et al. (2004).

- The only major change we implement by 'removing' the ice surfaces is changing the 'land use category' in the model input data from the category 'ice and snow' to 'bare rock'. According to Pineda et al. (2004), this leads to a changes in albedo, moisture availability, emissivity, roughness length, and thermal inertia (Tab. 1). We added the table together with an extended description on the changes in the senitivity studies with the changes in surface parameters to the manuscript, also in accordance with a request by Michael Haugeneder (referee #1).

G4 How does the "No Glaciers" experiment answers the questions asked? I think many of these results could go into the supplementary material, as they did not vary substantially from the "HEF-only" simulations (except on HEF). Might it be more illustrative to run the simulations with the upstream glaciers while removing HEF? This would also better simulate "realistic" deglaciation.

- We agree that the NO_UP simulation already answers most of our research questions. However, we conducted the NO_GL simulation to investigate, on the one hand, how the flow dynamics behave if entire catchment were ice-free, and on the other hand, to investigate the sole impact on HEF tongue on the flow structure. In our opinion, it is also an important result that the flow structure in the NO_GL simulation is very similar to NO_UP. One result from the NO_GL simulation is actually that HEF only (as in the NO_UP simulation), has a completely negligible impact on the general flow structure and the gravity wave development and breaking pattern, despite the negative heat fluxes (see heat flux figure of NO_UP). We actually think this is a major finding - namely, that under certain flow conditions, single glacier tongues are not able to maintain their own small-scale microclimate.
- Unfortunately, our project 'SCHISM' (Austrian Science Foundation, project number I 3841-N32, `https://dx.doi.org/10.55776/I3841`) and the associated computational resources already ended, therefore we cannot conduct additional simulations. However, since our study showed that the removal of the upstream glaciers has an impact on the stability of the upstream vertical profiles and therefore gravity wave formation, we think that keeping the upstream glaciers would lead to a gravity wave of a similar strength and structure as in the REF simulation.
- A follow-up project "Glacier-Space: Assessing the resilience and vulnerability of mountain ice masses" was recently funded by the Austrian Science Fund and some of the open questions arising from this manuscript can be answered in the future within the scope of this new project. We refer to future work on this in our conclusion section.

G5 Gravity wave "formation" is used throughout, including the title. Is gravity wave break-up not what is being simulated?

Both gravity wave formation (in the column above the upstream glaciers) and gravity wave breaking (in the column above HEF) are simulated in the model. The gravity wave forms over the upstream glaciers and breaks over the HEF valley.

G6 How representative is this 6h period of the typical flow conditions observed over HEF? This would help provide a take-away message that is more generalizable to other glaciers/study sites.

- Obleitner (1994) performed a wind climatology analysis for HEF and found that the katabatic glacier winds are mostly undisturbed by synoptic influence over the glacier tongue during summer. However, we have to take into account that the glacier extent was larger than nowadays, and we could expect that HEF's katabatic glacier wind was more resilient to external forcing than nowadays (e.g., Shaw et al., 2024, showed with observations over multiple years that other flows from the thermally-induced valley wind circulation start to dominate over katabatic winds when ice surfaces are shrinking). At the South-facing slope facing HEF, Obleitner (1994) noted in their climatology a significant influence by Northerly gradient winds, which would correspond to a similar situation as our case study (Northerly/Northwesterly snyoptic flow).

- In a more recent dataset from the HEFEX campaign (summer 2018), Mott et al. (2020) found that around 20% of their wind data falls into the "disturbed" phase, i.e., an erosion of the katabatic glacier wind by lateral flows.
- We added the following paragraph to our discussion section:
"A final open question to discuss is how representative our 6 hours of simulation are for HEF and its surroundings. Currently, we can only compare to a wind climatology at HEF compiled by Obleitner (1994), and they found a significant Northerly gradient wind influence on the South-facing slope of the valley, the same wind direction as in our case study. Furthermore, Mott et al. (2020) noted in the HEFEX campaign that in 20% of their wind observations the katabatic flow was 'disturbed' and the glacier boundary layer was eroded. Therefore, we can assume that the described situation of strong North-Westerly winds and gravity waves eroding the glacier boundary layer is not a single occurrence. Still, an updated wind climatology over HEF is necessary to quantify these events. Furthermore, applying an intermediate complexity model such as HICAR (Reynolds et al., 2023, 2024) to the region for entire seasons would shed more light on the typical wind patterns over HEF while being computationally cheaper than a full-physics LES."

G7 The manuscript would benefit from a more holistic introduction to gravity waves. A schematic would make the interpretations of certain figures easier to follow. This should include a discussion of how WRF resolves gravity waves at the given scales. Stull (2017), p.761 highlights that the model grid spacing is not the model resolution, and wavelengths smaller than roughly $7\Delta x$ are often filtered out for numerical stability. What wavelengths do you expect to see, and how well does WRF resolve these wavelengths?

- We agree and re-organized our introduction. In the new version, and enitre section is dedicated to the formation and mechanisms of gravity waves.
- We only analyze the innermost domain of our simulations with a horizontal grid spacing of 48 m. To our knowledge, mostly sound waves are filtered out for stability in numerical models. Still, when we follow the $7\Delta x$ rule, this would mean that phenomena with a horizontal extent of 336 m can be resolved on our grid. Our entire glacier valley has a width of 3 km, and out innermost domain spans 12x12 km^2, corresponding to 251x251 grid points. Henceforth, we are confident that the gravity waves related to the flow over upstream topography is resolved on our grid.
- We added the following sentences in the discussion:
"As discussed in Goger et al. (2022) and Voordendag et al. (2024), as our domain contains more than 10 grid points across the glacier valley, this suggests that the major ABL processes are resolved (Wagner et al., 2014). Furthermore, when we assume that effective horizontal resolution of the model is $7\Delta x$ (Stull, 2017, their pg. 761), phenomena of a horizontal extent of 336 m are resolved on the grid. This is the case for the gravity wave across the valley and the associated processes. Since we focus on the dynamical forcing and gravity waves, which form on scales larger than the small-scale glacier boundary layer, we can expect that the model delivers reliable results on these processes."

G8 I find it hard to substantiate the conclusion that one must, in general, view local flow dynamics in the context of a larger system given that this experiment was only 6 h (especially without knowing how prevalent strong NW synoptic winds are at this location). While I know these simulations are expensive, I feel it would be illustrative to run a no-upstream-glaciers simulation under weaker, or otherwise different, synoptic forcing if this is the intended message.

- We agree that a single case study is possibly not representative for entire seasons or other glaciers/icefields. However, as outlined in our response to comment G6, Northerly winds are noticeable in wind climatology of HEF and 20% of data collected during HEFEX (Mott et al., 2020) showed lateral disturbances of the katabatic wind. A possible additional analysis in the future could be exploring data from regional kilometric climate simulations (Molina et al., 2024) to identify situations with North-Westerly flow in the HEF region and quantify their frequency. However, this is out of scope of the current manuscript.

- Unfortunately, our project 'SCHISM' (Austrian Science Foundation, project number I 3841-N32, `https://dx.doi.org/10.55776/I3841`) and the associated computational resources already ended, therefore we cannot simulate another experiment with different or no synoptic forcing. However, we think that under weak synoptic forcing, local thermally-induced flows likely dominate, and they will interact with the katabatic down-glacier winds, similar to the situation in Shaw et al. (2024). The influence by the upstream glaciers would be smaller, and a similar situation as in Goger et al. (2022)'s "SW day" and as in the 'undisturbed' katabatic conditions in Mott et al. (2020) would be present. However, a follow-up project (Glacier Space, see answer above) was recently funded and is currently starting, so some of the open questions arising from this manuscript can be answered in the future.

> G9 Can you comment more on the cause of the deviations between simulations and observations after 12:00? How do you argue that your experiment simulations are still valid at 12:00? In Fig 7, 8, and 10, the most pronounced changes (apart from the missing ice surfaces) seem to occur at the bottom of the domain, far from the removed ice surfaces. Is this physical? How far into the next valley do we see these effects?

- We draw our conclusions on the reliability of the REF simulation from the model validation with observations in Goger et al. (2022). Therefore, we use the REF simulation as a performance baseline for the two sensitivity runs, and since the simulation data of REF deviates from observed quantities (e.g., wind speed, temperature, etc, Fig. 2 in Goger et al., 2022), we do not analyze the results after 12:00 UTC.
- The correct simulation of mountain boundary layers is a major challenge for numerical models (Chow et al., 2019), and even increasing the horizontal resolution does not automatically improve results (Goger and Dipankar, 2024). One of the largest challenges for numerical models are so-called scale interactions between larger and smaller scales. While this problem might be less pronounced in a simulation with no synoptic forcing, in our case study, we have a strong synoptic, large-scale flow, interacting with smaller-scale processes in the glacier valley of HEF. Even for LES grid spacings, the scale interactions are a major challenge (e.g., foehn-cold air pool interactions or the interaction between slope flows and up-valley winds Umek et al., 2021; Goger and Dipankar, 2024). Furthermore, separating the larger and smaller scales (scale separation) is not possible (Schemann et al., 2020), evident in our simulations with the dominating large-scale flow. All the aforementioned issues accumulate with increasing simulation time, and in our case, this point is reached at 12:00 UTC, and this is case study specific. For example, in the winter case study with the same model setup, the WRF model is able to produce reliable results for a longer time period (Voordendag et al., 2024).
- Our focus of this manuscript was on HEF, the upstream glaciers, and surroundings. Still, we think the differences in the next valley are likely due to flow modification due to the missing ice surfaces and different gravity wave breaking patterns. Since the differences are in the range of less than 2 K, we deem the results as physical.

> G10 I would prefer all equations to be included in the methods section. In general, I would encourage a slight restructuring so that the results presented can be better anticipated from the outset.

We added a new subsection called "analyses performed" to Section 2 where we present our analysis methods and the relevant equations.

Specific comments

> • I find that NO_UP and NO_GL read more like filenames than experiments, likely due to the underscore.

We agree to some extent, but we also think that the names of the sensitivity experiments should be distinguishable from the rest of the text, therefore we keep the underscores in the names. Furthermore, it is not uncommon to use underscores for numerical experiments (e.g., see Wagner et al., 2015, their Tab. 1).

• L13: I do not think this study shows that a glacier tongue is never isolated from the surrounding glacier environment. (G8)

We re-wrote the sentence to
“a single glacier tongue is not isolated from its environment under strong synoptic forcing [...]”

• L55: Can you argue this point more clearly? The scale difference between mesoscale and glacier-scale flow is at least a couple orders of magnitude. (G2)

We re-wrote the sentence to
“With a horizontal mesh size of 48 m, the models can simulate mesoscale wind patterns on the glacier for both summer and winter successfully, but struggle wuith the correct representation fo small-scale glacier boundary layer features (Goger et al., 2022; Voordendag et al., 2024).”

• L105: Would you use a different module if the BL was not turbulent?

If turbulence were entirely parameterized (e.g., in a mesoscale simulation with kilometric grid spacing), the fluctuations in the simulation are reduced and henceforth no time averaging would be necessary.

• L107: Three hours of spin-up time seems relatively low. Draeger et al. (2024) had a 24h spin-up time, and Liu et al. (2023) showed that the choice of spin-up time depends on process being modelled. To that end, Sun et al. (2014) highlights the importance of spin-up time in convective models. How are you certain that three hours is sufficient in the simulations without the stabilizing glacier surfaces? (G9)

Thank you for the comment, selecting the correct spin-up time is important for reliable LES simulations. As you correctly pointed out, the spin-up time for model depends on the physical phenomenon being modelled. For example, for convective events, the spin-up time is longer and might even need a correct/extended soil moisture initialization to deliver correct results.
The question on the spinup time of our model was also raised by referees for Goger et al. (2022). At that time we performed additional simulations with an earlier initialization, but noticed the same deterioration of model performance after around nine hours of simulation time, as in the current manuscript.
In our case, we simulate gravity waves and local mountain boundary layer processes over glaciers. Pfister et al. (2024) give an overview of the time scales of several phenomena in complex terrain (see Fig. R1). Since the timescales of local flow structures and exchange processes happen at timescales of less than an hour, we know that our spinup time of 3 hours is sufficient. We added an additional sentence to the model description:
“The first three hours of simulation time are considered as spinup, to ensure that turbulence develops accordingly and given the time scale (an hour or less) given our phenomena of interest (gravity waves, boundary-layer processes)."

• L113 (and elsewhere): “Sensitivity study” reads more like testing different parameter regimes than removing entire glaciers.

The term “sensitivity study” is a widely used term for changing land surface or terrain properties in numerical models. Strictly speaking, removing the ice surfaces modifies the flow *regime* in our simulations. Furthermore, e.g. Umek and Gohm (2016) use the term for describing their numerical runs, where water bodies and/or topography are removed to study the mechanisms behind snowfall patterns.

• L130: Non-stationarity meaning changing gradually throughout the day? Or referring to turbulent stationarity? If the latter, this also needs to be mentioned when introducing M-O theory.

We calculated the so-called 'non-stationarity ratio' by Mahrt (1998) in Goger et al. (2022) (their Fig. 8) from high-frequency time series of the sensible heat flux (both for observations and model output). A high non-stationarity ratio corresponds to large fluctuations of the sensible heat flux within the averaging period (in our case, 15 minutes), and non-stationarity was highest during the gravity wave breaking episodes, suggesting strong turbulence. We added to the brief summary of the phenomena in the REF simulation
“The case study day was dominated by cross-glacier flow and high values of non-stationarity (Mahrt, 1998) of the sensible heat flux during gravity-wave breaking episodes and a strong mesoscale influence on the glacier boundary

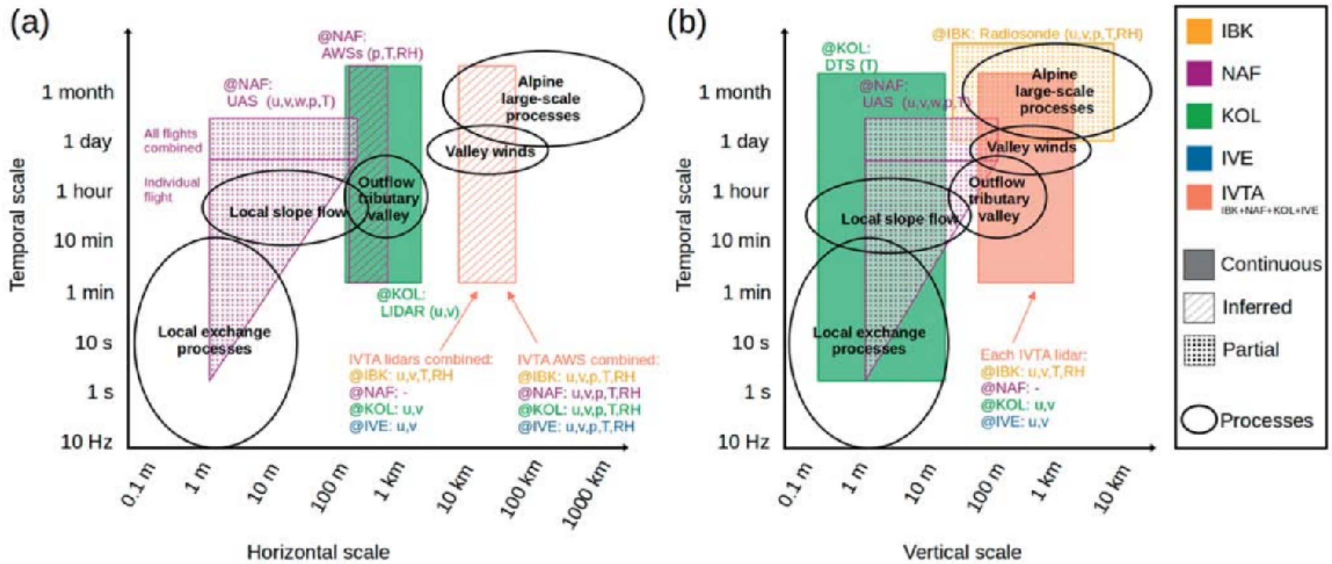

Figure 17: Schematic space-time diagram of resolvable scales during TEAMx-PC22 divided into (a) horizontally and (b) vertically resolvable scales. The Inn Valley target area (IVTA) and corresponding sub-target areas are indicated by colours. The filling of areas categorizes if the resolvable scale is inferred, partial, or continuous (cf. Section 4). A selection of processes and motions under investigation during TEAMx is also added (bold circles and text) to illustrate which of them were actually resolvable and at which location. All resolvable scales have a short description of which instruments were used (AWS: automatic weather station; lidar: remote sensing with light detection and ranging; UAS: uncrewed aerial system; DTS: distributed temperature sensing; radiosonde) and which parameters were retrieved (T: temperature; RH: relative humidity; p: pressure; u, v: horizontal wind speed and direction; w: vertical wind speed).

Figure R1: Figure from Pfister et al. (2024), their Fig. 17.

layer.”

- Figure 2 (and elsewhere): Here you present time as rows, and in other cases you present time as columns. Choosing one would be preferable. Additionally, axis labels here (and elsewhere) overlap.

We changed the overlap of labels (and in all further occurrences as well) and unified the row/column structure in all figures.

- L157: Δ TKE?

Changed accordingly.

- L163: Do you mean “shooting” downslope flow as classified by Mahrt (1982)? If so, please clarify this interpretation.

Thank you for the reference. However, the downslope flow in our simulations is not a gravity flow (like katabatic), but is rather dynamically forced due to large scale forcing, and displays hydraulic-jump-like features. We changed the formation to *“a distinct dynamically forced downslope flow”*.

- Figure 4 a-d (and discussion): If the lowest level shown here is 3 m above the surface, then I am rather surprised that REF and NO_ GL show a temperature difference of only 2 K. How do you explain the increase in temperature toward the surface in the lowest levels of REF? What does “mostly neutral” mean? (G2)

We agree that the difference in potential temperature in the lowest levels over HEF tongue is surprising. However, we have to keep in mind that the glacier tongue is under the influence of the cross-glacier flow, and any (local) glacier boundary layer (e.g., down-glacier katabatic winds) could not establish because of the strong lateral disturbance. Given that the mesoscale flow structure (and the resulting gravity wave inducing severe turbulence) in all three simulations is similar, the weak temperature contrasts between REF, NO_GL and NO_UP are realistic.

• Figure 4 e-h (and discussion): These subplots are visually dense. Some of these profiles look quite surprising – Are there two velocity maxima in the NO_ UP and NO_ GL simulations? What is meant by L198 "chaotic behaviour"? (G2)

We agree and changed the opacity of the wind direction points to increase the figure's readability.
As outlined in the sentence, due to the severe turbulence induced by the gravity wave, there is no distinguishable pattern visible in the vertical profile, therefore we kept the term "chaotic behavior".

• Figure 4 i-l (and discussion): I do not see how the Scorer parameter profiles presented show conditions favourable for gravity waves. In the provided references (e.g. Parmhed et al., 2004), the Scorer parameter is larger near the surface and then decreases, and is more than an order of magnitude larger than the near-surface values presented here. The Scorer profiles here look like they're being significantly affected by the division by a small U. Additionally, how are these gradients calculated? (G2)

We agree that we used a confusing strategy to explain the Scorer parameter and use the parameter in a different way than Parmhed et al. (2004).
In the classical use of the Scorer parameter (see, e.g., this explanation, `https://resources.eumetrain.org/data/4/452/navmenu.php?tab=4&page=5.0.0`), is to check whether conditions are favorable for gravity wave development. The Scorer parameter is usually calculated in the vertical profiles (in our case, by using Δz from the model's grid) upstream of a mountain range or at the peak, as we do by calculating the Scorer parameter for the location "upstream". In these vertical profiles, it is visible that the Scorer parameter has a sharp decrease in the REF simulation, but this sharp decrease is less distinct – or not existent – in NO_UP of NO_GL. Therefore, we conclude that the upstream glaciers impose favorable conditions for gravity wave formation.
We update the text in the manuscript and removed the references (Parmhed et al., 2004; Söderberg and Parmhed, 2006), because we do not observe/simulate a katabatic flow in our time period of interest anyway.

• L156-160: This presentation is a bit hard to follow. I would argue that the bulk method is agnostic to wind direction. That is, one does not check the wind direction to pick the temperature to use in the model, but rather uses whatever temperature is measured (which is likely different under different flow regimes, but then this is an implicit dependence, not an explicit one). Moreover, I'm a bit unsure why the bulk methods are being introduced here.

We re-wrote the paragraph to make the presentation more accessible. We present the bulk method here because this is the method how surface fluxes are calculated in the model.

• Equation 1: Brackets aren't tall enough.

Changed.

• L174: $N = N(z)$ is correct, but a bit misleading as it hides the $\frac{\partial \theta}{\partial z}$ dependence.

We added the equation for $N(z)$ to the manuscript.

• Figure 5: I think units of m would be preferable along the slope.

True, but our cross-section consists of several appended cross sections (see Fig. 1), so we thought keeping lat,lon is better for the readability of the figure.

• Figure 6: How do A and B relate to each other? It appears from A that the REF observations have the same variability (-0.5 to 1) as the NO_ UP and NO_ GL. Yet in B, The REF observations have a reduced range of observed wind directions relative to NO_ UP and NO_ GL.

Thank you for this remark. Indeed, in the REF simulation, almost all blue points are on the diagonal and correspond to the dynamical regime. Some of these blue points indeed collapse over each other on the diagonal and are therefore 'on top of each other', unfortunately.

To answer the second remark, the range of wind direction is reduced because the up-valley flow only establishes much later in the REF simulations (because of the still stronger gravity wave), while in the NO_UP and NO_GL simulations, the yup-valley flow establishes much ealier and increased turbulence over the glacier surface (compared to REF) also leads to a wider range and chaotic structure of the wind directions.

Furthermore, we double-checked the python script behind the figure again. All three simulations use the same number of data points, but the blue data points (especially in the lower right part of the plot) collapse on each other in the dynamical regime, and/or are concealed by the points from the NO_UP and NO_GL simulations (esp. in the case of the channelling regime).

- Equation 2: Typeset this equation using "upright" cos and brackets of appropriate height. I also prefer UWI and wdir to be upright and not slanted here and in the text. The conversion to radians is implied. So I prefer,

$$\mathrm{UWI} = \cos |\mathrm{wdir} \quad \phi|,$$

or similar. That said, if the UWI is only computed at one point along the glacier, I feel that wind direction alone (perhaps oriented such that 0 is upglacier) is sufficient, and simpler. It seems the utility of this metric arises when comparing observations from multiple locations where "upslope" might be different.

Thank you, we changed cos to "upright", but not the rest of the equation, since Weather and Climate Dynamics' guidelines require italics for variables in equations (see `https://www.weather-climate-dynamics.net/submission.html`, "Mathematical notation and terminology").
We prefer to keep Shaw et al. (2023)'s proposed formula to calculate UWI, because it also takes glacier/valley orientation into context. This formula has the potential to be widely used in studying wind regimes over glaciers in the future because it only requires few observations, and we would want to demonstrate its usability/potential.

- L223: Worthwhile to introduce the negative case. Perhaps more clear to state, say, $|\mathrm{UWI}| < 0.5$ indicates cross-glacier flow.

Added to the manuscript:
"When $UWI = 1$, the flow is exactly up-glacier, while $UWI \approx 0.5$ indicates cross-glacier flow, and $UWI \approx 0$ implies down-glacier flow."

- L234: Why use upstream location and not wind direction from a higher level in the same column?

We prefer to use the upstream location instead of the column directly above HEF tongue, because our focus of the manuscript is on the change in upstream conditions (removing glaciers) and their impact on the flow structure. Another reason is that measurements with a weather station are potentially possible as in (Whiteman and Doran, 1993), in contrast to the location directly above a glacier, where a vertical profiling method would be necessary. Furthermore, we want to stay consistent with our location selection with Fig. 4.

- Section 3.3: The latter half reads as a discussion and not a presentation of results.

We agree, but since we use Section 3.3 to explain the observed patterns in the previous sections, we think that it is appropriate to discuss the results at this point.

- L276-281: As you say, none of these results are particularly surprising. (G4)

Yes. We moved Figure 8 to the appendix.

- Figures 7 and 8: The choice of colorbar scales makes interpretation challenging. I would be far more interested to see the differences in sensible heat flux into the glaciers. A colorbar with a nonlinear scaling might help here. Overlapping axis labels. Inconsistent sensible heat flux vs. "SH" (here and in text).

Thank you, we removed the sensible heat fluxes of the surroundings and now focus on the (missing) ice surfaces only.

- Axis labels do not overlap anymore.
- We removed the "SH" abbreviations (both from figure and text).

- Section 4.2: Please clarify the intended message of this section. The beginning sentences do not seem related to the section title, directly. It seems the focus is on advection of heat and not momentum?

We agree and added introductory sentences at the beginning of the section:
"Observations and numerical simulations agree that the local flow patterns over HEF strongly affect the heat transport and advection processes over the glacier tongue (Mott et al., 2020; Goger et al., 2022; Haugeneder et al., 2024). Therefore, we explore the impact of changing the ice surfaces in the next paragraphs."

- L290-292: What would be the foundation of this assumption?

This idea was proposed by Mott et al. (2020), but was, however, disproven by identifying the gravity wave being the major mechanism between advection patterns on HEF tongue (Goger et al., 2022).

- Equation 3: This is not a complete heat budget. Please explain which terms are omitted and why. What length scales are the derivatives taken over? ADV and vHFD appear upright in the text, so should appear upright here. That said, they are only used this once so don't need to be abbreviated.

We added an additional sentence on our assumptions:
"We neglect the radiative flux divergence as in (Goger et al., 2022), because we consider it small during daytime. Horizontal averages are taken over Δx=48 m."
Furthermore, we removed the abbrevations ADV and vHFD.

- Figure 9: I prefer the colorbar only over the subpanels where it is relevant. (d-f) I feel would be better presented as components of the budget. Is the budget closed? I can't tell if this is the LHS or RHS of eqn. 3 at present. (c) Downglacier advection? Or total horizontal advection?

We split, also based on the suggestion from the other referee, into two separate figures. Panels (a)-(c) from old Fig. 8 are now new Figure 8, while the new Fig. 9 includes panels (d)-(f) from old Fig. 8, while adding a row of panels of the time-averaged components of the total vertical heat budget to the respective simulations. We constrained to the colorbar to panels (a)-(c). We also added extra text to describe the behaviour of the components of the vertical heat budget at HEF tongue.

- Figure 10: Similar to before, is the message here related to the temperature differences over the whole domain, or the temperature differences on the glacier? The colorbars could show this better, if the latter. Panel d is not needed.

We calculated time averages over our simulation period of interest (06:00-12:00 UTC) over the entire domain, as Fig. 9 shows, so the latter case is true. Furthermore, we indicate in the text description whether we are currently talking about the glacier or about close-by surroundings. We removed panel (d) in the Figure.

- L313: Why do you trust this assessment of 2 m temperature given the criticisms of M-O theory applied to katabatics (e.g., Grisogono et al., 2007, and references therein). (G2)

We are aware of the breakdown of MOST in katabatic flows over glaciers. However, as we state in the manuscript, an analysis of surface temperature output from the model would not make much sense (since it is constant at 0°C over the ice surface). During our time period of interest used for the averaging in Fig. 9, there is no katabatic glacier wind observed, since the glacier boundary layer is disturbed due to the cross-glacier flow (Mott et al., 2020; Goger et al., 2022). Therefore, we can trust the 2 m temperature values to some extent, if we keep in mind that MOST has several other shortcoming and needs substantial revision Stiperski and Calaf (2023), but unfortunately, this is the only option we currently have to compare the temperature patterns from model output over HEF. We added a clarifying sentence
"However, although MOST breaks down in katabatic flows (Grisogono et al., 2007) and is in need of substantial revision (Stiperski and Calaf, 2023), we do not observe katabatic winds in our time period of interest, since the glacier boundary layer is heavily disturbed by the cross-glacier flow, as outlined in the previous sections and in Mott et al. (2020); Goger et al. (2022)."

- L330: What are dynamical aspects?

We agree that this formulation is unclear. We changed it to "dynamical forcing".

- L346: Earlier what?

Changed to
"breaks 30 minutes earlier"

- L355: If this is an intended take-away message, it would be good to (1) make this a more clear objective, and (2) quantify these effects more clearly. How did you decide on 5 km? The Columbia Icefield is the study of Conway et al. (2021) is very large. Do you expect the size of the icefield to play a role? Do you expect this to be true in all synoptic conditions or just some? (G6/G8)

Thank you for the remark. We touch upon the spatial scale of our glacier catchment in the discussion, but we added more sentences discussing other icefields in the discussion as well:
"Given that, it makes sense to speak of a system of glaciers influencing each other's local micro-climates on a scale of around 5-10 km, given the scale of HEF and its surroundings. However, this length scale for the development of mesoscale ice breezes - similar as in Conway et al. (2021) - depends on the size of the glaciers and connected upstream ice fields. An example for larger icefields influencing the local wind patterns on glacier tongue would be the Columbia icefield in Canada (Conway et al., 2021), the Jostedalsbreen icefield in Western Norway (Haualand et al., 2024), or Vatnajökull icecap in Iceland Björnsson et al. (2005). All of them are larger than 50 km, and dependent on flow direction, icefield breezes or gravity waves are favorable to develop. Our work allowed us to shed light on the impact of upstream icefields/glaciers on local glacier boundary layers, but more detailed research in combination with wind climatologies and flow-resolving simulations are necessary in the future."

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