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

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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.

General comments

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

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.

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.).

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.

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

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.

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 [2015], 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?

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 6h (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.

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?

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

Specific comments

- I find that NO_UP and NO_GL read more like filenames than experiments, likely due to the underscore.
- L13: I do not think this study shows that a glacier tongue is *never* isolated from the surrounding glacier environment. (G8)
- 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)
- L105: Would you use a different module if the BL was not turbulent?
- 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)
- L113 (and elsewhere): "Sensitivity study" reads more like testing different parameter regimes than removing entire glaciers.
- 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.
- 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.
- L157: Δ TKE?
- L163: Do you mean "shooting" downslope flow as classified by Mahrt [1982]? If so, please clarify this interpretation.
- 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)
- 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)

- 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)

- 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.

- Equation 1: Brackets aren't tall enough.

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

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

- 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.

- 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,

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

  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.

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

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

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

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

- 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).

- 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?

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

- 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.

- 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?

- 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.

- 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)

- L330: What are dynamical aspects?
- L346: Earlier what?
- 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)

References

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