Response to Michael Haugeneder (referee #1)

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

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Dear Michael Haugeneder,

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

> The authors present a case study of large-eddy simulations of the atmosphere above the highly heterogeneous high-mountain terrain surrounding Hintereisferner (HEF). They compare a reference simulation featuring realistic ice surfaces with two sensitivity anal- yses comprising different surface properties. In one run, they only replace the upstream glacier surfaces, while in the other sensitivity run, all glacier surfaces are replaced by bare ground. With these different setups, the authors aim at investigating the influence of upstream surface properties on the flow dynamics over the HEF ultimately affecting the heat fluxes in the near-ice atmosphere at HEF.
> Their work contributes significantly to understanding the larger-scale (and non-local) influences on the local flow field. Comparing runs with modified surface properties to verified "real-world" runs offers promising insights advancing the current understanding of process interactions at a wide range of scales. However, I suggest revising the presen- tation of the results to facilitate better understanding.
> If the authors have any questions, please don't hesitate to ask.

We thank you for the encouraging comments, especially for the detailed inquiries on the model's performance, which will lead to an improvement of the revised manuscript.

Major comments

> 1. language and spelling: The manuscript will strongly profit from a thorough revision of the language including syntax and spelling. At this stage, it is sometimes difficult to follow the author's line of argumentation.

Thank you for the comment. We re-read the revised manuscript thoroughly and hope that the readability improved and no typos are present anymore. Furthermore, we modified the figures according to your suggestions.

> 2. It should be possible for the reader to follow the main ideas of the study without completely reading Goger et al. (2022). This particularly refers to the model setup. What is the vertical and horizontal grid spacing of the inner domain? What is the height of the lowest model level? Can you briefly summarize the main findings of the comparison of the REF run with the measurements during HEFEX? Why do the REF run and observations diverge after 1200?

- We agree that the current manuscript should be readable without knowing the contents of Goger et al. (2022). We already mention the horizontal grid spacing and the vertical grid spacing in Section 2.2:
  "We use a nested set-up consisting of four domains, where the outermost domain spans Europe with $\Delta x = 6$ km and receives ERA5 reanalyses (Hersbach et al., 2020) as boundary and initial conditions. We subsequently nest down over $\Delta x = 1$ km, and $\Delta x = 240$ m to the innermost domain at $\Delta x = 48$ m (Goger et al., 2022, their Figure 1b)."
  and furthermore
  "In this study, we only show the output domain 4 for our analysis, and any mentioned numerical data will stem from this domain at $\Delta x = 48$ m."

- We mentioned the lowest model level in the results, but we agree that we have to introduce it already in Section 2.2:
  "The lowest model level in the innermost domain is located at 7 m, resulting in the lowest model half-level height of $z = 3.5$ m."

- Unfortunately, we have to summarize the results from Goger et al. (2022) in a concise way to avoid repeating ourselves, and we give an overview of the major results of the model run in comparison with the HEFEX observations at the beginning of Section 3:
  "The reference simulation (REF) is a real-case simulation of the glacier boundary layer from August 17, 2018. Under North-Westerly synoptic influence, a gravity wave formed over the North-Western ridge close to HEF, leading to a continuous disturbance of the glacier boundary layer. 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 layer. Details on the simulation and further results can be found in Goger et al. (2022), their "NW day"."

- In general, in real-case LES there is always the so-called 'scale separation' problem, meaning that large-scale flows and smaller scales interact with each other (Schemann et al., 2020). This is also evident in our simulation, especially due to the strong synoptic forcing, and the large-scale flow starts to dominate over smaller scale features due to misrepresented scale interactions (e.g., as discussed in Goger et al. (2022) with the 'too strong' gravity wave or in (Umek et al., 2021) with foehn-cold air pool interactions). To overcome this deviation of results after 12:00, we would have to re-start (re-initialize) the model again with a new analysis field. However, observations suggest that the situation over the glacier does not change drastically therefore that the restarted simulations would not bring many new insights into the topic, therefore we decided to stay with the 6 hour period for our analysis. We added the following sentence to the beginning of Section 3:
  "The REF starts to deviate from the observations after 12:00 UTC (Goger et al., 2022, their Fig. 2) due to the scale separation problem in LES (Schemann et al., 2020), but observations suggest that the situation over the glacier does not change drastically after 12:00 UTC. This period is relatively short, but since observations suggest that the situation over the glacier does not change drastically from 12:00 until sunset, an extended analysis does not bring new insights. "

- In general, we extended Section 2 and now also discuss the assumptions and shortcomings of our current model setup.

3. Although the authors state in l. 324ff. that "we cannot assume that the local glacier boundary layer [...] is simulated realistically", RQs 2 and 3 and Section 4 focus on the effects of the overlying flow on the near-surface heat exchange in the local glacier boundary layer over HEF. I suggest giving a more comprehensive reasoning as to why analyzing the effects on the near-surface atmosphere is valid or focusing the manuscript on larger-scale interactions between the (breaking) gravity wave and other flows and leaving out near-surface processes.

Generally speaking, we are aware of the shortcomings of our simulations, e.g., that MOST is likely invalid over complex terrain, and that scale interactions pose a challenge for high-resolution numerical models due to the scale separation problem (Schemann et al., 2020). However, we discuss these challenges both in the current manuscript and also in our previous publications with similar setups (Goger et al., 2022; Voordendag et al., 2024). Our major findings include that the absolute values of quantities (e.g., horizontal wind speed, 2 m temperature, etc), exhibit a certain bias, but we also showed that the relevant physical processes (e.g., dependence of the sensible heat flux on the wind speed, stationarity of sensible heat fluxes, horizontal advection patterns, and the heat budget) are represented well in the model. We have to emphasize that we now have a starting point with these simulations –

of course, we could simulate plenty more cases, but given the expensiveness of the simulations, we have to focus on single case studies. We think that this particular NW day is an excellent day to study the impact of large-scale flows on the glacier boundary layer, especially because we can expect that the gravity wave and associated processes are already resolved on the grid.

Furthermore, we have to differentiate between the local glacier boundary layer, i.e., a boundary layer dominated by katabatic flows, and the 'disturbed' boundary layer we noted in simulations and observations (Mott et al., 2020; Goger et al., 2022). The small-scale processes in stable boundary layers (with katabatic flows) would require much higher horizontal and vertical grid spacings to be resolved fully - Cuxart (2015) even suggests horizontal grid spacings of 1 m, which is clearly not met by our simulations.

However, in our case study the local glacier boundary layer is not able to form due to the strong dominance of the gravity wave in the REF simulation (Goger et al., 2022). Therefore, we do not have a down-glacier katabatic flow (cross-glacier flow instead), and as the vertical profiles reveal in Fig. 4, rather a neutral boundary layer above the location glacier tongue. Therefore, we have a gravity-wave-dominated, almost neutral boundary layer above the glacier tongue, and we can expect from our model that surface-exchange processes under these conditions can be expected to be resolved correctly.

We, however, agree with the referee that the statement in the manuscript is misleading. We added one clarifying sentence to the beginning of Section 3:

"Due to the aforementioned phenomena, the local glacier boundary layer is heavily disturbed both in simulations and observations, and no katabatic down-glacier flow is present."

> 4. Can you include a brief review on gravity waves (generation, overturning, breaking, hydraulic jump (l. 154) and their influence on the isentropes and the turbulent kinetic energy as this is an important part of your study?

Thank you for this comment - we re-organized our introduction and dedicate a paragraph to gravity waves and also added additional literature references.

> 5. The introduction should present a comprehensive motivation for the conducted research. Can you clearly state the knowledge gap you address with this study? Your RQs 2 and 3 leave the impression that the study aims to investigate the effects of artificial surface modifications on the atmosphere. However, I think the authors performed the ice-removed runs as a method to gain more information on upstream influences on the atmosphere above HEF.

We wish to investigate the influence of the upstream glaciers on the atmospheric flow structure and gravity wave formation, since the NW day case study of Goger et al. (2022) showed that under NW flow conditions, the local glacier boundary layer is eroded. Since previous literature (Turton et al., 2018; Jonassen et al., 2014) suggests that ice surface have an impact on the atmospheric static stability aloft, we want to investigate the role of removing these upstream ice surfaces on the flow structure over HEF. We want to stress that these numerical simulations are, although semi-idealized, the only way to investigate this question.

Since our case study is under the influence of strong (synoptic) North-Westerly flow, we cannot isolate the 'upstream location' and the 'atmosphere above HEF' from each other and rather have to view them as a connected system (this was also highlighted in the final conclusion on a 'system of glaciers' instead of isolated glacier tongues).

We re-formulated the research questions, though, and hope now to address the open questions and knowledge gaps better.

> 6. A separate method section that contains the definitions you use (e.g. Scorer parameter, up-valley wind index, advection formula, ...) can enhance the readability of the results section. Additionally include a description of the surface parameters used for ice and replaced ice surfaces in section 2.

We removed the equations from the Section 2 and added a subsection called "analyses performed". We also added a Table (Tab. 1) about the changes in the surface parameters and an accompanying description text to the manuscript (Section 2.2).

Land-use category	Albedo (%)	Moisture availability (%)	Emissivity (% at 9 μm)	z_0 (cm)	Thermal inertia (1 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).

7. In the discussion, add ideas on how to cope with the lack of near-surface process representation in your model. Would nesting with a finer-scale model, such as HICAR (Reynolds et al., 2023), close to the surface improve the results?

We discuss the challenge in simulating near-surface processes throughout the manuscript and also mention the challenges of the model in the first paragraph of the discussion, where we cite literature on the very setup and the discussion there, so we cannot repeat already published work one-by-one. We are aware that there will be always a certain misrepresentation of surface processes in the model, but we also want to stress that we investigate a case study day where we simulate strong dynamical forcing - so small-scale processes are not so important as in, i.e., katabatically-driven cases.

To our current knowledge, HICAR's target resolution is 50 m according to Reynolds et al. (2023, 2024). Our innermost LES domain (the analysis domain for this current manuscript) has a horizontal grid spacing of 48 m, therefore, a HICAR simulation would only bring advantages if the horizontal resolution were finer than 48 m. However, we can think about running simulations with intermediate complexity models as HICAR, especially to perform seasonal runs to explore further how often strong cross-glacier flows occur over HEF or any other glacier or icefield worldwide. This would also help answering the questions on representativeness raised by Cole Lord-May (referee #2). We added the following paragraph to the discussion:
"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."

8. The discussion lacks a clear storyline. Please revise the structure of this section. Furthermore, a main part of the results section is dedicated to the surface energy exchange, but the results are not discussed.

We agree, and we revised our discussion section thoroughly. Pasting everything here would be too much, but we would like to direct you to our tracked changes manuscript to see the substantial changes we made on the discussion.

Specific comments

1. At the first occurrence, state that all times are in UTC and then remove the "UTC" throughout the rest of the manuscript.

Thank you. We added a sentence at the first occurence of UTC:
"All further time information in this publication refers to UTC, so we will omit 'UTC' at all further occurrences."

2. l. 32f.: Mott et al. (2020) found the same over HEF

The referee is right, but we discuss the findings by Mott et al. (2020) in the following paragraph in much more detail (as one of our major motivations for this study), therefore we decided to cite them at this later occasion.

3. l. 51: High-resolution → decameter-resolution

Changed the sentence to:
High-resolution large-eddy simulations (LES) at decameter grid spacings have emerged [...]

4. l. 51: Introduce the "LES" abbreviation at the first occurrence

Done.

5. l. 51f.: Include Mott et al. (2019). They investigated the near-surface boundary layer over a perennial ice field using measurements and a high-resolution modelling setup.

Thank you for the reference, we added it.

6. l. 55: Can you detail more on why you are confident that the LES can resolve the "relevant mesoscale flow"? Here, it would help to talk about the results from Goger et al. (2022).

We changed the sentence to:
With a horizontal mesh size of 48 m, the topography governing the mesoscale flow evolution is already sufficiently resolved in the model for the successful simulaiton of structures and wind patterns on the glacier for both summer and winter [...]

7. l. 69: What do you mean by "stronger due to the ice surfaces"?

We changed the sentence to:
[...] downslope windstorms are stronger due to the stabilizing effect of the ice surfaces [...]

8. Figure 1: color contours → contour lines and colors

Changed accordingly.

9. l. 90: Can you give a number for ice melt during "extreme mass loss [...] in some recent years" to compare to the 1 m over the last 20 years?

Thank you, the sentence now reads
"While it loses around 1 m ice thickness per year over the last 20 years (Piermattei et al., 2024), extreme mass loss has been observed in some recent years (-3319 kg m^{-2} in 2022, 3.2 times higher than the long-term mean for 1991-2020, Voordendag et al., 2023)."

10. l. 101: add a reference to Figure 1b in Goger et al. (2022)

Done.

11. l. 114: How do you define the upstream glacier surface replaced in NO_ UP? What do you set as the "new" parameters describing the replaced surfaces in contrast to glacier ice?

Thank you for this remark, we added an additional explanatory sentence:
The surfaces of the missing glaciers in the sensitivity simulations are replaced with the land-use category of the surroundings, namely bare rock.

12. l. 119: In addition, you use the current (ice-covered) DEM just replacing surface properties, right? So the topography of the replaced surfaces is also not representative of a melting ice cover under the influence of climate change.

We did not change anything on the used DEM, but at a horizontal grids spacing of 48 m it cannot be expected that ice-covered surfaces were resolved in the model in the REF runs. We agree with the referee, this is not representative for a melting ice cap under climate change, which was not our intention of the study. As mentioned above, we added extra information to the manuscript:
"The surfaces of the missing glaciers ('snow or ice') in the sensitivity simulations are replaced with the land-use

category of the surroundings, namely 'bare rock', while the topography remains the same."

13. l. 124ff.: This sentence fits in l. 106.

Thank you, we moved the sentence upwards as suggested.

14. l. 127: More concise caption. Maybe: "Flow structure"?

Changed to *"Simulated flow structure with [...]"*

15. l. 132ff.: Remove that sentence. Already stated above

Removed the sentence.

16. l. 136ff.: That sentence would fit earlier in the manuscript when you talk about the selection of the period

We agree and moved the sentence to Section "Numerical Model".

17. all following figures: Can you make the color of the glacier outline consistent with Figure 1?

Yes. Now, all glacier outlines have the same colour as in Fig. 1.

18. Figure 2: The x-labels of the color plots are overlapping.

We changed this (and in all the follow-up figures with the same issue as well).

19. Figure 2 caption: Height of the lowest model level?

Added.

20. l. 140: Height of the lowest model level?

Added to the caption of Fig. 2.

21. Figure 3 caption: Indicate, that the cross sections are taken looking up valley along the black line in Fig. 1b.

Added.

22. interpretation of figure 3: If you talk about stability, you mean "static" stability, right? Could you note that when you talk about stability inferred from the isentropes?

Thank you, we added "near-surface static stability".

23. interpretation of figure 3: Include the Scorer parameter plots in figure 3 and discuss the parameter in the course of figure 3 in connection with the gravity waves to improve the storyline.

We indeed thought about this, but we think that the Scorer parameter (and the equation) fits better with the vertical profiles, since they directly influence its calculation.

24. l. 145: Can you support your note on "weakening of the cross-glacier flow" with values? I find it hard to see in Fig. 2e,f.

We noted that we referenced the wrong figure sub-panels, this was corrected. Furthermore, we re-wrote the sentence more clearly to
"In the NO_ UP and NO_ GL simulations, however, we note the weakening of the cross-glacier flow, visible in reduced wind speeds after 08:00 UTC (wind arrows in Fig 3g,h)"

25. l. 150f.: repetition of the previous sentence

True, we removed the sentence from the previous paragraph, and changed the sentence to:
To better understand the changes in gravity wave formation in the NO_UP and NO_GL simulations compared to REF, we examine the vertical structure of the upstream flow conditions in the next paragraphs.

26. l. 157: Δ TKE = -5 $\mathrm{m^2\,s^{-2}}$

Added.

27. l. 171: Indicate the upstream point in figure 1

The upstream point is indicated, it's the plus sign in Fig. 1. We mention it now again in the caption of Fig. 4.

28. l. 176: lower → weaker

We decided to go with "reduced stability".

29. l. 180: Fig. 4e?

True, thank you.

30. l. 179ff.: Briefly note that the near-surface stratification is often different from further aloft.

Changed the sentence to:
In all simulations at 06:00 UTC, the potential temperature profile at HEF tongue reveals a very shallow part with strong gradients, capped by a mixed layer influenced by gravity wave breaking, and topped by stable background stratification. So we see a three-layer structure on the vertical profiles, very common in Alpine terrain (Weigel et al., 2006).

31. l. 180f.: Do you have a hypothesis as to whether the jet height is different?

The jet height in stable boundary layer flows is dependent on the sensible heat flux - as the sensible heat fluxes are weaker in the NO_UP simulation, and even reverse sign in the NO_GL simulation, we assume that this change in the surface forcing also impacts the jet height.

32. l. 183: maxima → extrema, large decrease → peak

We think it's better to keep "maxima" instead of "extrema", since we did not perform a statistical extreme value analysis on our simulation data. Changed large decrease to peak.

33. l. 185: within ≈ 600 m above the surface

Added.

34. Figure 4: Set the upper limit for the y-axes to 4000 m like figure 3. Consider creating separate subplots for the wind direction to enhance the readability of the plots. i,j,k,l) Sc → l2

We need to set the ylim to 5500 m for the analysis of changes in the Scorer parameter, pivotal to explain the formation of gravity waves over the upstream glaciers.
We agree that especially the panels on wind speed and direction are messy. Therefore, we changed the opacity of the wind direction points to increase the figure's readability.

35. l. 186: potential temperature difference close to the surface

Changed accordingly.

36. l. 187f.: The REF simulation at the upstream location also shows a ≈ 100 m deep near-neutral or slightly unstable layer adjacent to the surface.

We agree and think that this is mostly related to the gravity wave and the turbulent mixing it induced. We already discuss this in (Goger et al., 2022), but we will mention it here again.

37. l. 192f.: Check sentence structure

Re-wrote the sentence to:
The wind direction over the glacier tongue now reveals a distinct up-valley flow in the NO_ UP and NO_ GL simulations (Fig. 4g), while the flow remains cross-glacier in the REF simulation. At the upstream location, the flow is North-Westerly for all simulations.

38. l. 195: differences in the potential temperature profiles close to the surface

Changed.

39. l. 199: the flow changes direction at 4000 m, which is above crest height

We agree - we replaced "below crest height" with "below 4000 m".

40. Figure 6: Could you indicate the cross-glacier and along-glacier flow directions in panel a? Why did you leave the thermally-driven regime out in panel b? Spell out "UWI" in the caption and add a reference to (2).

We added all proposed regimes after Whiteman and Doran (1993) to the revised figure. We spelled out "UWI" in the caption as suggested.

41. l. 205f.: Where do you identify the neutral layer and the inversion in fig. 5e?

We revised this text heavily and this formulation does not appear anymore in the revised manuscript.

42. l. 210: Upper part of HEF

Done.

43. l. 204-215: challenging to follow

We agree and re-wrote the paragraph to:
"In the REF simulation there is a strong stable boundary layer (SBL) at the upper parts of the glacier at 06:00 UTC (Fig. 3a). The SBL coincides with a down-glacier flow, while below 3000 m a.m.s.l., the flow weakens and with reduced stability and higher TKE values, related to the strong cross-glacier flow and the gravity wave present (Fig. 3). In NO_ GL, the isentropes show that there is generally weaker stratification (Fig. 5e) in accordance with lower TKE values associated with the weaker gravity wave (Fig. 5i). Two hours later, REF still shows a SBL over the upper part of HEF, however, in NO_ GL, the stratification over the missing glacier is continuously weakened. Furthermore, NO_ GL exhibits higher TKE values than REF below 3000 m a.m.s.l., related to the earlier breaking cross-glacier gravity wave. The atmosphere above HEF is well-mixed in both REF and NO_ GL at 10:00 and 12:00 UTC (Fig. 5c,d,g,h), and the SBL mostly dissipated in both simulations, while the vertical profiles in the NO_ GL simulation even suggest a convective boundary layer at the glacier tongue (Fig. 4d)."

44. l. 233ff.: At which location and height do you extract the upstream wind direction? The same as before? Can you indicate that in fig. 1?

Exactly, we use the upstream location indicated as a plus in Figure 1. We now mention it again in the caption of Fig. 6.

45. l. 235: add a reference to figure 1 in Whiteman and Doran (1993)

Thank you, done.

46. l. 250f.: Is the model capable of resolving thermally-driven winds so you can say their influence is negligible? Please add a brief comment on that.

Yes, it is very realistic that thermally-induced flows are resolved in our model, at least the plain-to-mountain circulation, up- or down-valley flows, and up- or down-slope flows. According to Wagner et al. (2014), ten grid points across a valley are necessary to resolve the relevant mountain boundary-layer features. This criterion is met for our simulations at Δx=48 m spanning across the ≈3 km wide glacier valley with 62 grid points.

47. l. 257ff.: Split this sentence and be more precise about the indirect dependence of the heat fluxes on the wind direction (via air temperature) in the Monin–Obukhov formulation. Consider presenting the formula and indicate how you diagnose sensible heat fluxes from your model output.

We agree that our argumentation was somewhat chaotic and changed the paragraph to
"The surface sensible heat flux plays a pivotal role in the energy exchange over glaciers in the summer months. Over a melting glacier the surface temperature is constant at 0° C, impacting the bulk formulation of the sensible heat flux (Stull, 1988, their equation 7.4.1d). As the surface temperature over a melting glacier is exactly 0°, sensible heat fluxes strongly depend on the wind speed, we can expect that changing wind patterns drive changes in the sensible heat flux structure."
And we moved the paragraph to the 'sensible heat fluxes' subsection.

48. section 4.1: Make clear when you refer to positive in contrast to negative heat fluxes. I suggest using the terms more/less pronounced or stronger/weaker when you relate fluxes of the same sign instead of higher/lower. Indicate, when you compare fluxes of different signs (mainly in the presentation of fig. 8).

Thank you for your suggestions, we checked throughout the manuscript and now use the atmospheric notation continuously.

49. l. 262f.: negative heat flux corresponds to the transport of heat from the atmosphere into the ice (atmospheric notation)

Changed accordingly.

50. l. 264: Which pattern are you referring to?

We changed the sentence to:
The reduced sensible heat fluxes are present during the entire simulation time at the remaining ice surface in NO_ UP.

51. l. 275: SH fluxes: be consistent with abbreviations

Changed to "sensible heat fluxes".

52. l. 274-281: Consider moving fig. 8 and this paragraph to the supplements and just give a summary in the main study. The results of NO_ GL do not seem surprising and there is already a lot of information.

There is one major finding we would like to highlight with the NO_GL simulation, namely that it shows that HEF as in isolated glacier tongue has almost no impact on the atmosphere aloft, despite the different sign of the sensible heat flux. This highlights that under our particular NW day situation, the glacier is not able to maintain its microclimate and that the upstream glaciers have indeed a larger impact on HEF's boundary layer than HEF itself. We will thereore keep Figure 8 in the main text.

53. l. Figure 7 interpretation: Can you go into more detail about different sensible heat flux magnitudes on HEF and how they relate to the local wind direction? That would help to highlight the importance of the local wind direction on the surface energy exchange and reinforce the manuscript.

This is related to whether the origin of the flow is located over an ice surface or on (heated) bare rock. We touch upon this subject already in Goger et al. (2022), where we state that sensible heat fluxes are generally higher in the NW day (this case study) simulation than if the source were over an ice.

In our current manuscript, we especially note enhanced sensible heat fluxes in NO_UP compared to REF at the glacier tongue, for cross-glacier flow as well as for the up-glacier flow. We discuss the sensible heat fluxes here: *"The reduced sensible heat fluxes are present during the entire simulation time at the remaining ice surface in NO_UP. Interestingly, the sensible heat flux difference between NO_UP and REF is very small at the upper part of the remaining glacier, where cross-glacier flow is present (Fig. 7h,k,i,l). The largest differences between REF and NO_UP are visible at 12:00, when the gravity wave broke and the strong up-glacier flow is present (Fig. 7i,l). It is not surprising the sensible heat fluxes over the missing ice surfaces change sign and are positive now which range up to 500* $\mathrm{W\,m^{-2}}$ *between REF and NO_UP (Fig. 7k)."*

54. Figure 7: x-labels overlap

We changed the Figure accordingly.

55. Figure 7 and 8: Consider focusing the color bar extent to HEF to make the differences on the glacier more apparent. Include a sentence about the wind arrows in the captions.

We added the information on the wind arrows. We removed the heat fluxes of the surrounding terrain and now only focus on the (missing) ice surfaces.

56. Figure 8 right column: outlines of replaced ice surfaces missing

Thank you. We added the missing ice surfaces outlines.

57. Figure 9: Consider splitting this figure into two figures: The first containing the left column and the second the right column. The subfigures in the right column miss y-axis labels. Furthermore, referring to the right column, in l. 298 - 308, you are mostly analyzing the heating effects at the glacier surface. Consider a simpler time-series diagram with just the surface values or discuss the vertical structure in more detail.

Thank you for this nice suggestion. We split the Figure in two parts, and added the time-averaged components of the total vertical heat budget to the new Fig. 10. We adjusted the accompaying text and discuss the behaviour of the heat budget cmponents now.

58. l. 282: Heat Advection and Heat Budget? Consider splitting this into two subsections.

We renamed the section and added the equation of the horizontal temperature advection as well.

59. l. 283: At which height are you extracting the data from the model?

From the lowest model level - we added a clarification.

60. l. 285 - 295: Please revise this paragraph. I can not follow your presentation.

We revised the paragraph accordingly.

61. l. 296: What do you mean by vertical heat budget? The temperature tendency equation? add original citation (Wyngaard, 2010)

Yes, we mean the temperature tendency equation after (Wyngaard, 2010). We added the reference.

62. l. 297: what terms have you neglected and why?

We neglect the radiative flux divergence as in (Goger et al., 2022), because we consider it small during daytime. We added a sentence on that in the revised manuscript.

63. l. 300: a.g.l.

Changed.

64. l. 300: what do you mean by wavy structure? Spatially or temporally?

Temporally - we clarified the sentence.

65. l. 302: but there are periods of $\frac{\partial\theta}{\partial t} < 0$ at the surface between 0945 - 1000 and 1145 - 1200

We agree and reformulated the sentence to
"This coincides to the gravity wave breaking pattern, because the brief periods positive $\frac{\partial\theta}{\partial t}$ *correspond to the gravity wave breaking in REF (cf. Fig. 3), and the glacier is under warming from 09:00 until 12:00 UTC."*

66. l. 309 - 321: I suggest focusing on the effects on HEF.

Thank you, but we think it is also relevant information to discuss the effect of the ice surfaces on their immediate surroundings.

67. l. 335: you noted earlier that REF is not reliable after 1200

We agree, but we want to mention anyway that there is no change in patterns in the sensitivity simulations - another argument to omit further analysis after 12:00 UTC.

68. l. 345f.: missing end of sentence

True - we removed the last "and earlier".

69. l. 346: Which forces? Refer to Whiteman and Doran (1993)

We added the reference.

70. l. 350: A large part of the results section is dedicated to the surface energy exchange, but the results are not discussed here

We agree and added a paragraph on the surface exchange to the discussion.

71. l. 380: The transition between the summarizing sentences and the outlook is abrupt.

We re-wrote the sentences to
"The present study gave insight to the impact of ice surfaces on gravity wave formation and breaking, and their impact on near-surface processes over a glacier. This is only a single case study, and in the future, similar studies with different upstream conditions could be conducted."

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