Reply to RC1: 'Comment on egusphere-2025-711', Anonymous Referee #1, 09 Apr 2025.
In the following text, the Referee's comments are reported in bold text and the author's answers are noted in italics. The edited text in the manuscript is in green.

We thank the reviewer for the positive and constructive feedback, and we agree with the summary provided by the reviewer. We believe we have addressed all the comments and will go through them step by step below.

The present study proposes an analysis of observations of sensible and turbulent heat fluxes during a few months in 2019 within the Greenland Ice Sheet, from three different eddy covariance measurement systems. This is a unique dataset, extremely difficult to obtain, and allows us to explore the importance of latent heat flux on the local mass balance. After presenting an intercomparison between the three measurement systems (which ultimately agree very well), the data are used to train a 'bulk transfer' type relationship in winter and in other years, enabling comparison with turbulent fluxes simulated by two climate models. The multiple challenges of measuring and simulating these fluxes in this type of environment are then discussed.

I really enjoyed reading this paper. The plots are clean and well put together. The text is well written and the sequence of ideas is easy to follow. With the modifications I recommend below, I believe this article has a rightful place in a journal of the caliber of The Cryosphere.

GENERAL COMMENTS

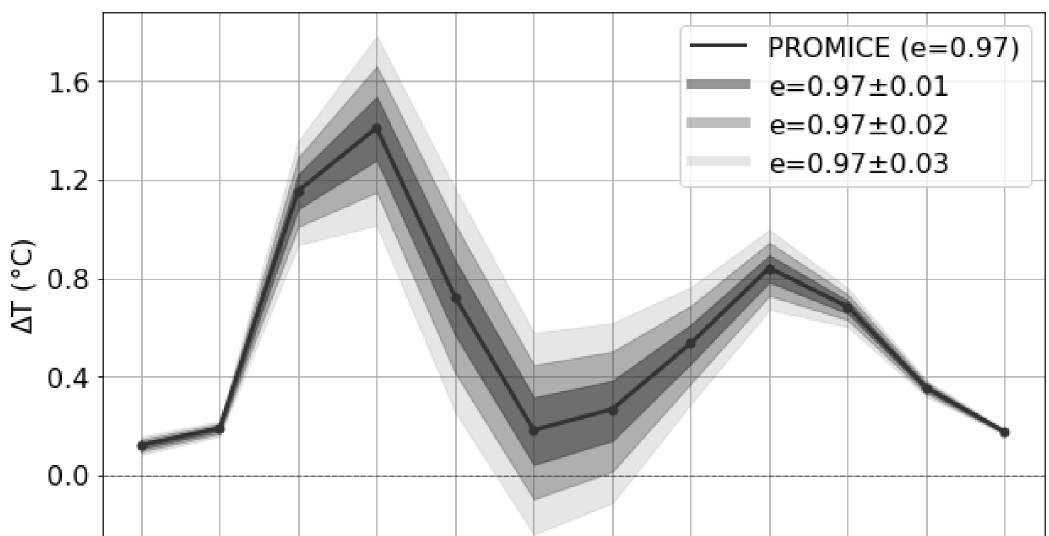
LHF and SHF in winter

I am puzzled by the 'observed' winter fluxes. The approach used to estimate these fluxes is to apply bulk transfer from a standard weather station based on z0m, z0t and z0q values calibrated in summer. It is clear to me that these values do not hold in winter, partly because the surface does not have the same roughness. More troubling is that the measured temperature difference between the surface and the air about 2.6 m above is very small in winter (at most 1°C in Jan. 2019!!). This seems impossible. Since qsfc depends on Tsfc, both turbulent fluxes are affected.
I know that most of these issues are raised by authors. I would like to see more options explored to improve these winter results:
We agree with the reviewer that the winter temperature gradients are puzzling, but we want to highlight that this very small temperature gradient is measured by both the PROMICE and GC-Net AWS. This, together with the arguments provided in the manuscript, is reason for us to explore the possibility that the small temperature gradient is not a measurement error, but could also indicate some limitation in our understanding of arctic boundary layer processes, e.g. concerning katabatic wind flows, in the winter.

- Have you tried estimating winter z0m from standard weather station data or other means?
Unfortunately, this is not possible using the available AWS observations alone, since an independent measurement of either the flux (u) or the gradient (from two wind speed measurement levels or more) would be needed. Although the GC-Net AWS does provide measurements at two levels, the uncertainty in extrapolating* $z_{0,m}$ *would be too large, due to the combination of the measurement levels being close together and the small gradients. Although other options for obtaining estimates of the surface roughness using remote sensing exist, e.g. using photogrammetry or laser altimetry (van Tiggelen et al., 2021), radar altimetry (Scanlan et al., 2025) or GNSS reflectometry (Pickell et al., 2025), we believe this falls outside the scope of this manuscript.*
In order to still get a better understanding of the uncertainty of the fixed roughness length, we have conducted a sensitivity study to investigate the influence of the roughness length on the resulting (winter) fluxes (see review 2). The sensitivity study is added to the supplementary material and expanded on in the manuscript.

- Have you explored surface emissivity values other than 0.97? Satellite data for Tsfc?
The reason for using a surface emissivity of 0.97 is that this is also used in Fausto et al. (2021). We have added a sensitivity study using different emissivities below. Here the seasonal cycle of the PROMICE near-surface atmospheric temperature gradient (T_{2m} - T_{surf}) *is shown. In the shaded areas, the same gradient is shown, but with* T_{surf} *calculated following Fausto et al. (2021) using different emissivity values. Here it can be seen that although using different surface emissivity values leads to some difference in surface temperature and therefore also the temperature gradient, this is limited during winter. So the choice of surface emissivity cannot explain the near-neutral temperature gradients during the winter months.*

$$T_s = \left(\frac{LR_{out} - (1 - \epsilon) \cdot LR_{in}}{\epsilon \cdot 5.67 \times 10^{-8}} \right)^{0.25} - 273.15$$

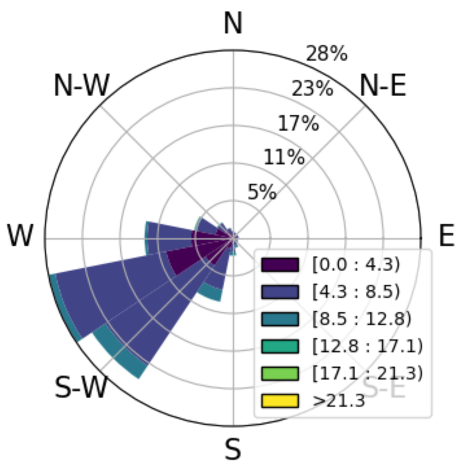


Remote sensing estimates of the land surface temperature are available through infrared radiation measurements. However, as these measurements are both influenced by atmospheric absorption and also depend on the snow emissivity, the resulting uncertainty is similar to the in-situ observations.

- What about radiative flux divergence? See for instance:
https://journals.ametsoc.org/view/journals/apme/46/9/jam2542.1.xml
We thank the reviewer for this suggestion. However in the paper two radiometers at 2 and 48 m are used. As the PROMICE AWS at EastGRIP only has one radiometer, this would only be possible when the surface temperature is known, meaning when it's melting. As temperatures are below zero most of the year round at EastGRIP, this is unfortunately not possible.

Wind

While temperature and humidity gradients play a key role in turbulent fluxes, wind is also key. This is barely mentioned in the article. What about katabatic winds at the measurement site? How do they affect the results? This should be covered in the introduction, results and discussion.
The reviewer is correct that winds play an important role in turbulent fluxes. However, for the EC-comparison, a windspeed filter is applied to the data to ensure suitable conditions, and in the model comparison, we see a high similarity between the windspeed in the models and observations. Therefore, this does not come back in the discussion as much.
With regard to the katabic winds, EastGRIP is indeed in the katabatic wind zone, as can be seen in the wind rose below of the wind speed and direction recorded by the PROMICE AWS in the period from 2016 to 2020. We have added more information in the description of the conditions at EastGRIP and in the discussion of the EC comparison and the model comparison about the katabatic winds and the possible influences.



"EastGRIP is located in the north-eastern part of the Greenland Ice Sheet (Fig. 1a), in the katabatic wind zone, and approximately 350 km NNE of Summit, the highest point of the ice sheet."

"The mean windspeed was 4.5 m s^{-1}, with a maximum of 13.7 m s^{-1}, an average direction of 254°, and strong directionality due to the katabatic winds (Fig. 1b)."

"The average wind direction during this 4-year period was 242°, with 74 % of the time the wind direction falling within the 200 − 280° sector."

"However, systematic biases with the EC method, due to boundary layer characteristics in polar conditions cannot be ruled out, such as the influence of the katabatic wind maximum, and during (sub)meso motions (Lan et al., 2022), which both typically take place under very stable conditions and can cause the transport to deviate from a fully turbulence-dominated regime."

"They propose that the difference in LHF between the model and observations during summer arises from a negative bias in downwelling longwave radiation, as also found by Fettweis et al. (2017), from the cloud scheme, while the winter bias may be caused by vertical mixing through katabatic winds that is not represented in the model (Dietrich et al., 2024)"

SPECIFIC COMMENTS

Abstract: Mention the exact dates of the measurement period.
The exact dates are added during which the EC intercomparison was carried out, together with the years over which the model comparison is done.

"In this study, we present an intercomparison of three independent EC systems from the 28th of May 2019 until the 31st of July 2019 at the EastGRIP site at ~ 2700 m a.s.l on the Greenland Ice Sheet to assess the accuracy of LHF and SHF measurements."

"Using improved values for z0,m, z0,q and z0,t, recomputed bulk fluxes are compared to fluxes simulated by regional climate models MAR, RACMO2.3p2 and RACMO2.4p1 for the period from 2016 to 2020."

L22: I know this comes up later, but the introduction should distinguish between surface and blowing snow sublimation.
We have clarified this sentence, so it is clear the study focuses on surface sublimation.

"The turbulent latent heat flux (LHF) directly impacts the SMB by adding or removing water through surface evaporation/sublimation and condensation/deposition and linking the mass balance to the surface energy balance (SEB)."

L33: According to your Fig. 5h, a third of the day in summer experiences unstable conditions. I think we tend to assume that as soon as there is snow or ice, the atmosphere is necessarily stable, which is not the case.
Because this feature is surprising many, we have added a sentence to the manuscript which highlights the observation:
"*It is noteworthy that contrary to the commonly assumed stable atmospheric boundary layer over snow surfaces, in 18 %, 21 % and 21 % of the 10-min time intervals measured by the*

EC-IRGASON, EC-Li-7500 and EC-KH20, respectively, the conditions were unstable (z/L<-0.02)"

L43: Discuss the strengths and weaknesses of open-path and closed-path gas analyzers.
We have added a sentence on the main weaknesses and strengths of open- and closed-path gas analysers.

"Wind and temperature fluctuations are typically measured at high temporal resolution (10 − 50 Hz) using a sonic anemometer, along with humidity variations using an open or closed path water vapour analyser. However, both suffer from limitations as open path analysers experience less attenuation than closed path analysers, but open path analysers are more sensitive to disturbances such as precipitation (Polonik et al., 2019)."

L54: you need to elaborate on the limitations of the MOST approach - what exactly is at stake during stable atmospheric conditions?
We have added a sentence elaborating on the limitations of MOST.

"*The existing limitations in the validity of MOST, in particular for moderately and strongly stable atmospheric conditions (e.g., Grachev et al., 2005; Cullen et al., 2007; Schlögl et al., 2017; Pfister et al., 2019), which are common on top of the Greenland Ice Sheet during winter (Cullen and Steffen, 2001), are expected to increase uncertainty for corresponding SHF and LHF bulk estimates. This is because vertical turbulent mixing becomes limited with increased stability, and non-local phenomena, like gravity waves and inertial oscillations, may occur.* **"**

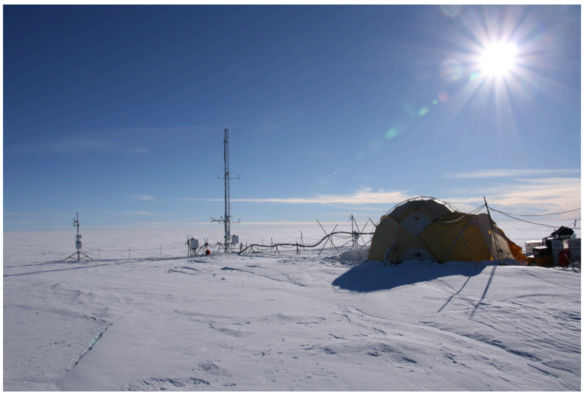
L74-75: I do not understand how this ties in with the main objective. Is it possible to reword the objective stated at the beginning of this paragraph to include climate models?
We have added a sentence so it is clear from the beginning of the paragraph how the eddy-covariance intercomparison and the comparison with the bulk estimates and the regional climate models link together.

"The aim of this study is to address this knowledge gap, and further, to determine the accuracy and validate EC measurements at a high elevation on a polar ice sheet. Using the validated EC measurements, we aim to improve the local bulk flux estimates and evaluate estimated fluxes from RCMs."

Figure 1: If possible, increase the resolution of this figure. A view of the footprint of the EC sensors would be useful. Also, there seems to be a fine-wire thermocouple on the IRGASON, but not on the other devices. Is it then the sonic temperature that is used to calculate the sensible heat flux? Please add these clarifications to the text.

The resolution of the figure has been increased. A view of the footprint and the landscape in the windward direction is provided below. The footprint area during times of regular wind direction included only an undisturbed snow surface (clean snow area). The reviewer is correct that there is a fine-wire thermocouple on the IRGASON, but this was not used in this *study. So the temperature used for the sensible heat flux is indeed the sonic temperature, which has now been clarified in the text.*

“For all three EC systems, the air temperature measured by the sonic anemometer is used.“



L87: Define 'clean-snow' area.
The clean-snow area refers to an area besides the ice-core drilling campsite where people are instructed not to walk. Therefore, ensuring that the snow in this area remains undisturbed. This has now been clarified in the text.

“The clean-snow area is a designated limited-access area oriented away from camp in the direction of the main wind direction to ensure undisturbed snow conditions.”

L89-90: Why was the period from 28 May to 31 July 2019 used? Please explain.
The reason for this period is due to the availability of the necessary infrastructure at the site during the summer period, since the camp is only operated during the summer season.

L93: At this stage of the paper, it is not clear why the period from 2016 to 2019 is mentioned for the model comparison.
The aim of the model comparison is to show a comparison between observations and regional climate models on a longer timescale of several years. This specific period is chosen based on the availability of the different datasets since the PROMICE weather station was installed in 2016, and the MAR simulation runs until the end of 2019. This explanation has been added to the manuscript.

“A model comparison is done for the 4-year period from 2016-2019, during which both data from the PROMICE AWS and MAR simulation are available.“

L95: Is it possible to better describe the observed wind regime, beyond the typical values of wind speeds and directions? Are there any katabatic winds?
As mentioned in the general comments, we have added more information about the katabatic winds to this section.

“EastGRIP is located in the north-eastern part of the Greenland Ice Sheet (Fig. 1a), in the katabatic wind zone, and approximately 350 km NNE of Summit, the highest point of the ice sheet.”

“The mean windspeed was 4.5 m s^{-1}, with a maximum of 13.7 m s^{-1}, an average direction of 254°, and strong directionality due to the katabatic winds (Fig. 1b).”

“The average wind direction during this 4-year period was 242°, with 74 % of the time the wind direction falling within the 200 − 280° sector.“

Section 2.2 and following: For the whole document, always present the three devices in the same order, to make it easier to follow.
The reason for presenting the devices in different orders is that for the introduction of the instruments, the order in which the systems are set up is used. But for the results, the EC-Irgason is used as reference system. To make this clearer, we have now explicitly added that the EC-Irgason is the reference system in the manuscript.

“The second EC system is the IRGASON (Campbell Scientific), hereafter used as reference system, which is a combined sonic anemometer and open-path gas analyser (Fig. 1e)”

L100: How and how often were these devices calibrated? Same question for the radiometer used to calculate the (very crucial) surface temperature.
For calibration of the PROMICE sensors, see Fausto et al. (2021). All three EC systems were calibrated in the factory. For the KH20, it is known that although the absolute humidity drifts over time, it still produces accurate flux measurements (Campbell Scientific, 2021). Humidity measurements from the IRGASON have been compared against a factory-calibrated PICARRO (Wahl et al, 2021) and the PROMICE humidity measurements. The Li-7500 was calibrated in the lab in December 2017, following LI-COR’s recommended practice for a zero and span calibration for both the H2O and CO2 measurements (Li-COR, 2004).

L111-113: This should have been mentioned earlier.
The longer availability of the EC-KH20 is now mentioned directly after the introduction of the instrument.

“The first EC system is a combination of a CSAT3 sonic anemometer (Campbell Scientific) and a Krypton Hygrometer 20 (KH20, Fig. 1d, Campbell Scientific). Besides 2019, this EC system was also deployed at EastGRIP during the summers of 2016, 2017 and 2018 (Steen-Larsen et al., a, b, 2022).”

L125: Mention that this is saturation with respect to ice. What is the validity of this hypothesis?
Following the approach of Fausto et al. (2021), we assume that the saturation is with respect to ice since we are on top of an ice sheet with very few days of temperatures above 0°C. While we agree that theoretically supercooled liquid could exist down to -20°C, we note the presence of ice crystals in the air, which would serve as heterogeneous nucleation nuclei.

“The surface specific humidity is determined using the surface temperature and assuming saturated conditions relative to ice.”

L147: Presentation of Andreas' (1987) formulation would be useful.
The presentation of the Andreas parameterisation has been added as an appendix to the manuscript.

L150-151: I do not understand the changes resulting from the 'physics cycle CY47R.1' update. Is it possible to explain the highlights?
The physics cycle CY47R.1 refers to the physical parameterisations of the Integrated Forecast System (IFS) cycle 47r1. The complete list of upgrades coming with the updated physics cycle is described in Van Dalum et al., 2024. A summary of the upgrades coming with this physics cycle has been added to the manuscript:
"The upgraded physics cycle constitutes changes in the precipitation, convection, turbulence, aerosol and surface energy exchange schemes. RACMO 2.4 now uses the IFS radiation physics module ecRad, the new cloud scheme has more prognostic variables, and a multilayer snow module for non-glaciated regions is introduced. A fractional land–ice mask, as well as new and updated climatological data sets (such as aerosol concentrations), are used."

Equation 1a: I suggest removing the minus sign and writing Ts - T.
The suggested edit has been implemented

Equation 1b: I suggest removing the minus sign and writing qs - q.
The suggested edit has been implemented

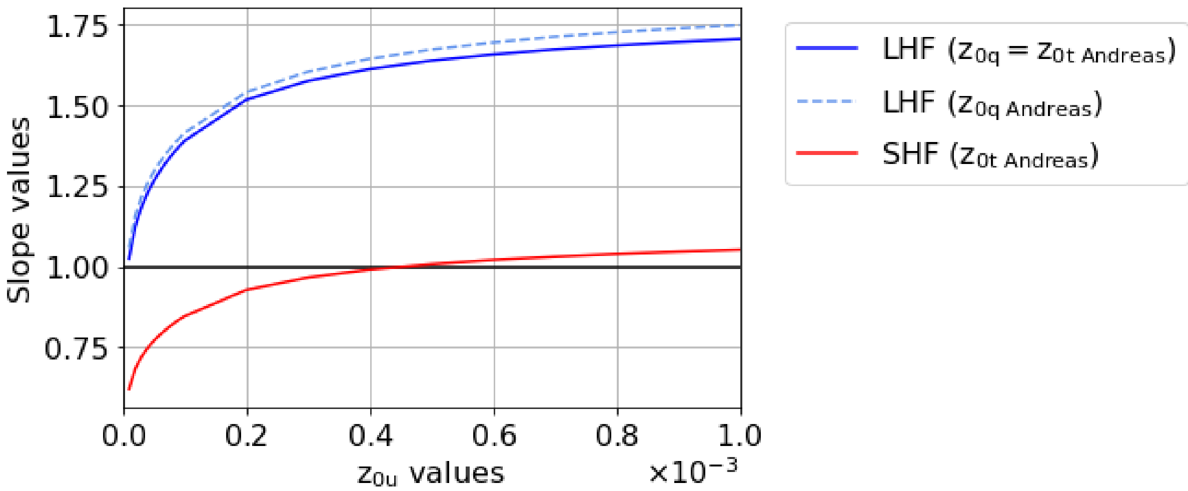
Equation 4b: why not use the specific humidity q instead of a?
The covariance of the vertical windspeed and humidity provided by the processing software TK3 uses the absolute humidity instead of the specific humidity. Several correction steps (e.g. despiking and planar fit) are applied to the raw EC data before providing the covariance. The absolute and specific humidity are related via the air density, which is non-constant over time; therefore, a covariance of the specific humidity would need to be recalculated with the specific humidity. Since the TK3 software does not provide this option, the recalculation, including the corrections of the raw data, would need to be done manually. For simplicity and consistency in data processing with 4a and 4c, it was therefore chosen to convert the averaged specific humidity to absolute humidity instead.

L275-280: again - what was the calibration strategy (zero and span) for this instrument?
As also mentioned in the previous question, the Li-7500 was calibrated in the lab in December 2017, following LI-COR's recommended practice for a zero and span calibration for both the H_2O and CO_2 measurements (Li-COR, 2004).

Figure 2 and equivalent: add a white box under the performance metric values and add the units on the RMSE.
We have implemented the suggested edits.

L298: How have the values of z0m, z0q and z0t been optimized?
Two approaches were used for this. The first is using the Andreas parameterisation to obtain z_{0t} and assuming $z_{0q}=z_{0t}$. The second is using the Andreas parameterisation for both z_{0t} and z_{0q}. For a discrete number of z_{0u} (1e-9, 1e-8, 1e-7, 1e-6, 5e-6, 1e-5, 5e-5, 1e-4, 1e-3), the

slopes of the correlation between the computed bulk and measured EC flux (similar to figure 5) were computed where a slope close to 1 indicates a correctly simulated diurnal flux amplitude. In the figure below the slopes of the correlation between the bulk method and the EC are shown for the range of z_{0u} values. The figure shows that there is no optimised z_{0u} value using the Andreas parameterisation for both approaches that is suitable for both the LHF and the SHF. That is why we ended up optimising the three roughness lengths separately.



L299: 5.7e-7

We correct the missing exponential.

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