RC2: 'Comment on egusphere-2025-1893', Arno Hammann, 17 Aug 2025

Summary

The article is very well written and understandable. Its scientific conclusions are sound and the work is well motivated by the lack of discharge data going back into the past. Some more details in the methodology could help the understanding of the reader (details below). The discussion from the point of view of synoptic and climatic controls is enlightening, but some of the suggested causal linkages are more implied than clearly demonstrated (details below).

Thank you very much for carefully reading our manuscript and providing helpful comments. Below, we address the reviewer comments and explain how we revise the manuscript. Reviewer comments are in black and our replies are in red. The line numbers in our replies are as in the original manuscript.

Scientific comments:

450 - The authors claim that the pressure patters in fig. 9 'trigger' the high runoff events, which may well be true, but as far as the work that is shown goes, the linkage is only demonstrated in one direction, namely that the pressure pattern is present when a runoff event occurs. It is not demonstrated that every time the pressure pattern occurs, a runoff event follows. It's not imperative in the context of the study to do that, but the choice of the word 'trigger' is maybe overstating the depth of the analysis somewhat.

We agree that the term carries too much emphasis. The text will be changed as "corresponded with".

With respect to the association between GBI and higher surface temperatures, the manuscript talks exclusively about the role of warm air advection, mostly in connection with cited previous work. I wonder whether decreased cloudiness and higher shortwave radiation also may play a role occasionally, or what the relative importance of the two mechanisms is (e.g. 445, 475).

We compared annual runoff form Qaanaaq Glacier with summer mean downward shortwave and longwave radiations (Figure R1). The downward shortwave radiation in summer has been decreasing over the studied period, and does not correlated with the increase in the glacier runoff as well as top-ranked annual runoffs. Although high annual runoff occasionally corresponded with the increased shortwave radiation (e.g. 2007 and 2015), it is not representative for the runoff characteristics and not very informative to describe in the manuscript. Conversely, summer mean downward longwave radiation had been increasing during the period, which is consistent with the previous study reporting increased cloudiness in northwestern Greenland (Noël et al., 2019) (L444–445). Therefore, decreased cloudiness and higher shortwave radiation are not the case for driving high runoffs on this region, and we would like to leave the text as it is.

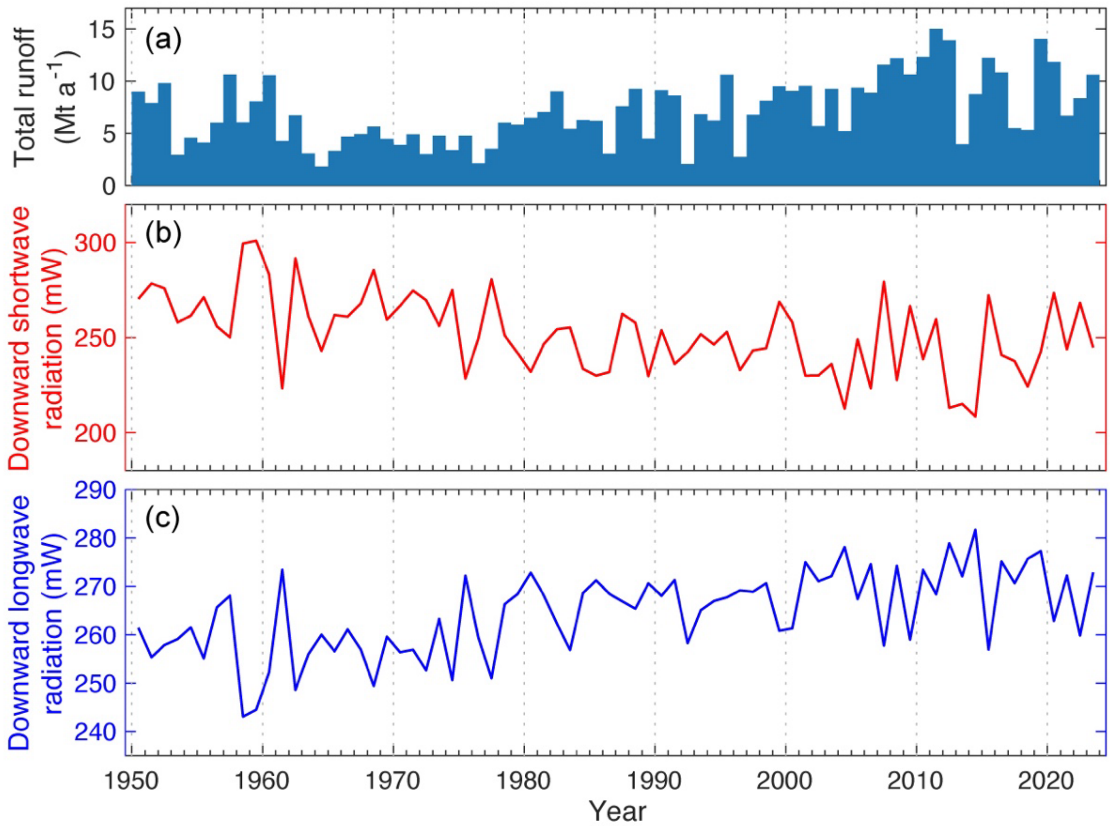

Figure R1: (a) Modelled annual runoff. (b) Summer mean downward shortwave radiation. (c) Summer mean downward longwave radiation.

Noël, B., van de Berg, W. J., Lhermitte, S., and van den Broeke, M. R.: Rapid ablation zone expansion amplifies north Greenland mass loss, Sci. Adv., 5, eaaw0123, doi:10.1126/sciadv.aaw0123, 2019.

Since the composites in fig. 7 contain only very few events, it would be quite nice if the text commented very briefly also on how representative they are for the individual events. In particular, the claim about the atmospheric rivers driving the runoff events, is it true for every single one of the 3 events?

Yes, the composite figure shows the representative state of the atmospheric variables. For the rain-induced runoff peaks, although the atmospheric river during the event on 22 August 2023 was particularly intense as described in the manuscript (Figure S7), the other two events also exhibited moisture transport exceeding 150 kg m^{-1} s^{-1} over Baffin Bay, which is consistent with the definition of atmospheric river. We will add these points in L354.

Technical comments:

A list of variables measured by the AWS would be nice.

We will add the observed variables in the description of the AWS (L84).

In the methodology, I would appreciated a bit more clarity on whether the non-glaciated part of the catchment is included in the modelling, in particular since rainfall is found to be important in the high-runoff scenarios. If the area is too small to make an appreciable difference, this can just be stated.

We thank the reviewer for pointing out the lack of the description. Ice-free terrain is included in the model to calculate snowmelt and rainfall over the area. This point will be added in Section 3.1.

A little more detail on the tuning of the rainfall estimates from ERA to the glacier surface elevation changes could also help the understanding - e.g. is the whole year taken into consideration (suggested by fig. S4a)? Then how is the snow density calculated / taken into account?

The tuning of the precipitation parameter was based on the results within the period 2013–2019. The new snow density (241 kg m^{-3}) and snow densification parameters were optimized by a Monte Carlo simulation conducted in a previous study to best reproduce the observed snow surface height and albedo in the accumulation zone of the northwestern part of the Greenland ice sheet (Fujita et al., 2021). These procedures will be added in the revised manuscript (L166).

Fujita, K., Matoba, S., Iizuka, Y., Takeuchi, N., Tsushima, A., Kurosaki, Y., and Aoki, T.: Physically Based Summer Temperature Reconstruction From Melt Layers in Ice Cores, Earth Space Sci., 8, e2020EA001590, doi:10.1029/2020EA001590, 2021.

In the model description (3.1), I would find it clearer to say ‘daily averages’ instead of just ‘daily values’ (if indeed it is daily averages that are being used).

We have checked the “daily” term throughout the text as listed below.

- daily surface melt rate (m w.e. d^{-1}), daily runoff depth (m d^{-1}), daily runoff (m^3 s^{-1}), daily mass balance (m w.e. d^{-1})

“Daily runoff (m^3 s^{-1})” refers to “daily mean runoff”, and the other terms refer to “daily total”. However, we would like to keep these terms as they are to follow general usages commonly seen in glaciology and hydrological literatures. Please let us know if you still think “daily mean” and “daily total” are better choices.

A short comment on whether the strong nonlinearity in the Stefan-Boltzmann Law is problematic or not at that level of temporal discretisation might be helpful.

We compared longwave emission from the surface calculated at hourly and daily time intervals. The results showed 0.015% of difference, thus influence of temporal discretization is considered to be negligible. We will add a short comment on this point at L135.

The sequence of equations 7-9: Why not directly just use T_z?

This is because to calibrate L_{calib} with the observed L at the SIGMA-B AWS, which is only possible by the step-by-step procedure described in the manuscript. After the downward longwave radiation from ERA5-Land (L_{ERA}) was adjusted to the SIGMA-B elevation (L_{calib}) with calibrated air temperature (T_{calib}), L_{calib} was compared with the observation and corrected using the linear regression equation as shown in Figure S2e and L158–159. After the correction, L_{calib} is distributed over the elevation range using T_z.

Also, does the calibration of ERA5 temperature happen after the lapse rate adjustment to the elevation of SIGMA-B?

ERA5 temperature (T_{ERA}) was directly corrected with the air temperature observations at SIGMA-B using the linear regression equation (Figure S2a). Therefore, the lapse rate adjustment is implicitly included in the linear regression equation.

110 - "The conductive heat flux, G, (W m−2) was calculated from the temperature profile of the subsurface snow/ice#: evokes the impression that the subsurface temperature profile is an input (and where would that information come from?), when in reality it is just part of the model.

Subsurface snow/ice temperature was calculated within the model using heat transfer equations. This point will be added in the text (L110).

55, 300, 420 - When discussing the role of snowfall, it becomes clear only later in the text that in the context of its importance for ablation it is its effect on albedo in the summer that is referred to (the reader may first think of winter accumulation). This could be made a bit clearer (e.g. in 300).

We agree this point. To clarify the role of winter snowfall on summer glacier ablation, we will add text at L299 as "suggesting snowfall was important for summer glacier ablation and runoff by modulating glacier surface conditions during the ablation seasons".

445 - "increase in annual runoff since 2000 coincided with the years with the top-ranked summer GBI": is a little unclear, if I interpret fig 9 correctly, the time after 2000 is both when the top-ranked GBI years occur and when the years with highest runoff occur, but it is not necessarily a year-by-year coincidence, i.e. not every year

with top-ranked GBI has top-ranked runoff.

The annual runoff variations are not necessarily expected to exhibit complete year-by-year relationship with the GBI amplitude, since the computed glacier runoff is a result of complex interaction among meteorology and glacier surface conditions. In this sentence, we intended to describe the major role of the top-ranked summer GBI to drive pronounced increase in the runoff since the year 2000, and not necessarily pointing year-by-year coincidence. Therefore, we prefer to leave the text as it is.

Fig 9: The nature of the fields displayed is not completely clear to me. The anomalies are with respect to averages over what time spans? And the ‘regression anomalies’ - I presume this is the regression coefficient of Z500 with respect to GBI?

Regression anomaly means the deviation of the meteorological variables from the trend line calculated during the period within 1940–2023, to eliminate the temporal trend (e.g. atmospheric warming) and investigate the control of GBI. To make it clear in the manuscript, the text will be corrected as “anomaly from the temporal trend”.