

## Response to review

We highly appreciate both the very positive feedback from Reviewer 2 and the constructive feedback from Reviewer 1, which have improved the manuscript in several ways. Nearly all the specific comments are resolved as suggested.

Reviewer 1 suggests merging this manuscript with part 2 of this study. However, in agreement with us, reviewer 1 also states that part 2 is already very long. Furthermore, the reviewer's arguments of not publishing this manuscript as a separate paper appear to be largely related to inaccurate phrasings that we believe are corrected in the revised version. Reviewer 1 also requests a more quantitative analysis. To comply with this comment, we have added more direct numerical spatial and temporal comparisons, particularly in Sections 3.3 and 3.4. As a response to the extra interest in comparisons across weather stations, we also added a new appendix figure (Fig. A1), illustrating how weather conditions during piteraq events differ at regional scales. Based on our revisions and the argument about the length of part 2 of this study, and in light of the very positive feedback from Reviewer 2, we argue that it is best to keep the two parts separated, with one part focusing on local-to-regional observed characteristics of piteraq events, and the other part addressing larger scales using reanalysis data.

During the review process, we did some minor corrections that were not related to the responses by the reviewers. These include a correction of the interquartile range using median instead of mean in Figs. 3 and 4 and some updated figure labels and captions using "air" in front of "temperature" and "pressure".

We have added the anonymous reviewers to the acknowledgement in case the article gets accepted for publication.

Response from us is from here marked by **green colour**. Line numbers refer to lines in the revised document unless stated otherwise.

# Reviewer 1

## General

This is Part 1 of a study of the forcing (thermo-)dynamics of piteraq winds in southeast Greenland, using a combined observational and modelling approach. The paper aims to systematically define and explore downslope windstorms in southeast Greenland, identified as piteraq events, by combining in situ near-surface observations from six automatic weather stations and vertical radio soundings in the region around Tasiilaq. In addition, these piteraq events are evaluated for societal importance using media reports retrieved from various archives.

The paper succeeds in conveying the large inter-event variability of piteraqs in a reasonably clear way. I most liked the sections where inter-AWS along-surface differences and vertical profiles are presented, which could serve as a basis for a more quantitative analysis, but here also the discussion remained rather descriptive. Unfortunately, I often stumbled upon formulations that were quite frequently inaccurate, which negatively impacts the readability, see below for a non-exhaustive listing. The section on piteraq impacts from media events comes across as somewhat disconnected and does not really add substance to the paper.

Sorry, but in view of the above I do not recommend publishing this manuscript as a separate paper. At the request of the editor I also reviewed Part 2 of this paper. That part is based on CARRA re-analysis and uses the events identified in this study. A natural solution would be to merge both parts into a single study, by adding the parts about the selection criteria from this paper as a new section to Part 2.

We thank the reviewer for the thorough feedback. We agree with the reviewer that there are phrasings in the manuscript that should be modified for better accuracy.

Considering the reviewer's comments, we have tried to connect and clarify the role of the media analysis in the study. Furthermore, we have provided more quantitative analysis to support our findings, particularly from the comparison across weather stations. For example, we now present wind speed differences across stations and comparisons between piteraq conditions and climatology in Section 3.3 (lines 181, 187, 194, 196, 204) as well as for some temporal and spatial temperature and humidity comparisons in Section 3.4 (lines 227-228, 242-244). The largest modification of the manuscript is possibly the addition and discussion of one appendix figure (Fig. A1) showing the differences in the weather evolution during piteraq events at several weather stations.

In accordance with the comments by Reviewer 2, who is very positive to the manuscript as it is, we argue that the manuscript could be published as a separate paper after correcting for the issues mentioned above. As pointed out by Reviewer 1, the second part of this study is already very long and with a different focus. Merging these two parts

will therefore disturb the readability. Currently, the two papers are clearly separated by the local/regional vs. larger scale focus as well as by the use of in-situ observations vs. reanalysis products. We argue that we should keep this separation for a clearer focus of the study. We also believe that the impact of the study is higher with two parts, as each part may be of interest to different groups of readers.

With our corrections and improvements based on the reviewer's constructive feedback, we argue that the manuscript is now at a level worthy of publication.

#### Major comments

l. 3: "...but with enhanced global warming and declining sea ice in the Arctic, their summer characteristics may become more relevant in the future"; without further knowledge this is unclear, unless for the Piteraq area current summers look like future winters, which in many respects (e.g. radiation balance) is not the case.

We agree with the reviewer that future shoulder seasons won't become identical to current summer seasons due to, for example, radiative effects. The main message we intended to convey with this statement is that there are *some* summer characteristics (e.g., warm air, less sea ice and hence more moisture) that will in the future more frequently characterize also the shoulder seasons, although other seasonal characteristics will remain similar to the current situation (e.g., incoming solar radiation). To avoid confusion, we have moderated and rephrased the sentence in lines 3-4 to

"...but with enhanced global warming and declining sea ice in the Arctic, **some of their** summer characteristics may become more relevant **for the future shoulder seasons**"

While the abstract should remain brief, we provide further discussion of these summer characteristics in the introduction (lines 63-65) and in the discussion (lines 257-261).

Figure 1: I presume this has been attempted by the authors, but having the same colour scales for the wind roses in b) and c) would make them easier to compare and hence interpret.

Yes, we have tried using the same colour scales, but due to the large differences in wind speed, it was difficult to see the necessary details in some of the wind roses. To address this comment, we tested a new colour scale. Details are well represented with this new scale, and the big advantage of direct comparison makes this scale a better solution. We have updated Fig. 1 accordingly.

l. 79: "horizontal and vertical weather characteristics" this is inaccurately formulated; do you mean "horizontal and vertical gradients in meteorological variables"?

Yes, we agree this sentence is inaccurate and have changed the sentence to the suggested version (see line 79).

l. 94: Are the 1/3 hourly wind data instantaneous or averaged? How were the 1-hourly and 3-hourly Tasiilaq wind data combined? Is this mentioned in line 166? Please organise text in such a way that this information is grouped in single location.

We agree that this needs clarification and apologise for the confusion. The 1/3 hourly wind data from DMI is the mean value from the last 10 min, while all the hourly data from PROMICE is hourly averaged. This is now described in lines 94-96.

We use all available measurements for the analysis, except for the detection of events (mentioned in the original line 166) and for the calculation of the 99<sup>th</sup> percentile in Fig. 2, where we use 3-hourly observations for consistency to periods where only 3-hourly data are available. We have added this information in lines 96-97 and in the caption of Fig. 2 and hope it is clear now.

l. 118: " This seasonality of extreme wind speed is in agreement with the seasonality of piteraq frequency"; please explain the nature of the agreement and quantify it if possible.

We have now provided numbers for the frequency in lines 179-181, which clarifies the agreement between the seasonality of the intensity and frequency:

“Extreme wind speeds are strongest from November to March and weakest from June to August, with the 99<sup>th</sup> percentile for all decades ranging from 7.7 m s<sup>-1</sup> in July to 18.0 m s<sup>-1</sup> in February. This seasonality of extreme wind speeds is **expected from and in agreement with the seasonality of piteraq frequency reported by Olthmanns et al. (2014), where the mean monthly number of downslope wind events in Tasiilaq each year ranged from c. 0.1 in June to c. 1.1 in February.**”

l. 126: "...we argue that a wind speed threshold of 15 m s<sup>-1</sup> should be used to define piteraq events at TAS..."; it is confusing that a little later there is a full section 3.2 on the definition of piteraq events. Please organise text in such a way that all definition related issues is grouped at a single location.

We agree that the structure should be improved. We have decided to restructure the text by first presenting the section on the definition and move the section on wind climatology to after the section on the evaluation by media reports. This restructuring resulted in some minor rewriting, but the main messages are unchanged. The largest change is that the 99<sup>th</sup> percentile of the wind speed is now first briefly mentioned in the beginning of Section 3.1 to motivate the wind speed threshold, but then later examined in more detail, when temporal and spatial wind climatology is evaluated in Section 3.3. We think the new structure of Sections 3.1-3.3 is much better, also because the wind climatology naturally leads to the following Section 3.4 on weather evolution during piteraq events.

l. 168: "their detection remains challenging due to data gaps"; this is again inaccurately formulated. If there are data, detection is easy, if there are no data From Tasiilaq) detection is impossible. Suggest to reformulate into something like: "Construction of a homogeneous time series of piteraq events remains challenging due to data gaps." This would also be a good place to refer to the availability of re-analysis data that can be used to fill data gaps in space and time (discussed in part 2?).

We appreciate this suggestion and have changed the sentence to (lines 139-142):

"Despite well-defined criteria for piteraq events, **construction of a continuous time series of piteraq events** remains challenging due to data gaps, particularly in the 2010s, and relatively low data coverage, particularly in the previous century. **To fill these gaps, a comparative list of piteraq events since 1991 is made and analysed based on CARRA reanalysis data in part 2 of this study (Silva et al., 2026).**"

Section 3.3: This section shows that the investigation into media reports does not find a robust quantitative connection between pteraq events following the definition used here and said media reports. This can be stated more clearly at the beginning or end of the section.

We agree that the summary of the main findings from the media reports in the last paragraph should state more clearly that there is no robust connection between the media reports and the definition. We have therefore modified lines 167-169, along with some clarifications including a better description of how close some media cases were to the piteraq definition (lines 159-160), how many of the media cases would have been defined as a piteraq if using reanalysis data instead (lines 160-163), and how the most intense piteraq events were represented by media (lines 172-174). These modifications improve the decription of the outcome of the media analysis and the connection to the definition of a piteraq.

Despite the lack of a more robust connection between the piteraq definition and the media reports, we argue that the media analysis still brings new and useful insights to the ongoing research on the piteraq phenomenon, in addition to increasing the societal impact of the study. As stated in the manuscript, important outcomes of the media analysis include 1) illustration of some of the challenges of early warning of piteraq events and 2) indicating that the definition of piteraq is relevant to the local society because it includes nearly all the events with reported consequences.

Figure 5 legend and caption: "Climatology" is used here to represent all data, but I associate it with the average weather. Perhaps replace with "All data"? Same with use of "climatology" in Fig. 6 and l. 245.

Yes, we agree that "all data" is better and have changed this accordingly, except in line 245 (now line 269), where "climatology" is appropriate as it does refer to the long-term weather.

l. 241: How would a lower ice sheet albedo contribute to a larger relative humidity change?

The intention of these sentences was to provide potential causes for the local warming and/or drying during piteraq events. The lower albedo is mainly related to the warming. The larger decrease in relative humidity in summer is probably mostly related to the reduced sea ice around Tasiilaq, making Tasiilaq more humid before it gets exposed to piteraq winds from the drier ice sheet. We agree that this should be stated more clearly and have revised the sentences in lines 263-265 to:

“While the increased warming during summer is likely related to a lower albedo over the GrIS and a larger role of foehn effects, the increased drying is probably a result of a larger transition between maritime and continental air masses when there is less sea ice surrounding Tasiilaq.”

l. 280: "deep vertical scales"; change into "deep atmospheric layers" or "large vertical scales".

Yes, we agree and have changed this to “large vertical scales” (line 304).

#### Minor and textual comments

l. 33: wind speed is weaker -> wind is weaker - or- wind speed is lower.

Corrected in line 33.

l. 166: "(see 1)": Table 1?

Corrected in line 137.

l. 210: What do you mean by "net change in temperature" compared to "change in temperature"?

There are some temperature fluctuations during this 24-hour interval, hence there is some *change* in temperature. However, the *net change* from the beginning to the end of the 24-hour interval is minimal. We therefore argue that *net change* is more correct.

l. 214: stronger -> higher

Corrected in line 234.

l. 247: elevation -> maximum elevation (?)

Corrected in line 271.

## Reviewer 2

This examination of the Piteraq strong wind events of southeast Greenland is a welcome addition to the scientific literature. There is little formal analysis of these events apart from the observational analysis by Oltmanns et al. (2014) and the modeling by Oltmanns et al. (2015). I thought the present observational analysis was the most comprehensive undertaken to date, was competently done, and did not find significant concerns with the manuscript.

The authors would profit by viewing the following UTube video by Jason Box on "Greenland Blowout 🌀 High Resolution Anatomy of a Piteraq Windstorm"

<https://www.youtube.com/watch?v0=fbYvp5pJFy4>

It is based on the very high resolution CARRA reanalysis and is dated June 2026.

We highly appreciate the very positive review. The YouTube video by Jason Box is indeed a very valuable motivation for our study and is particularly a relevant inspiration for part 2 of this study, which uses the CARRA reanalysis dataset.