Responses to comments of " Contrasting Impacts and Mechanisms of Clustered versus Isolated Summer Atmospheric Rivers on Arctic Sea Ice Melt " egusphere-2026-1025 to TC

We thank Referee #1 for the detailed and constructive assessment. We understand that the original manuscript raised substantial concerns about terminology, methods, figures, statistical support, language style, data availability, and the strength of the interpretation. Below we provide a point-by-point response describing how we plan to address these concerns in the revised manuscript. At this stage, we focus on the revision strategy and methodological additions. A full marked-up manuscript will be provided if a formal revision is requested.

Major comments

Comment 1. Atmospheric river, moisture intrusion, and pulse terminology are used interchangeably.
Response: We will standardize the terminology throughout the manuscript. The sector-based IVT events will be called "regional AR pulses". Broader physical descriptions will use "moisture intrusion" only when referring to the general process of poleward water-vapour transport. Object-based ARs will be distinguished from regional AR pulses and will be used only for the external catalogue check. We will avoid switching between "AR", "moisture pulse", "IVT pulse", and "moisture intrusion" without definition. Labels such as P1 and P2 will be removed from the main argument.

Comment 2. The Introduction lacks sufficient context on Arctic ARs and sea ice.
Response: We will expand the Introduction to include Arctic moisture-intrusion, Arctic AR, and sea-ice surface-energy-budget studies. The revised introduction will distinguish general AR literature from Arctic-specific work and will include recent studies on AR effects on Arctic sea ice, summer melt, moisture intrusions, clouds, radiation, and sea-ice recovery. We will also add a clearer link between winter/spring moisture-intrusion literature and the summer melt-season problem addressed here.

Comment 3. The Methods section is too brief and not reproducible.
Response: We will rewrite the Methods section in standard paragraph form. We will add a table listing all ERA5 variables, native units, processing steps, and final sign conventions. The revised Methods will include the temporal and spatial resolution of the datasets, the four sector definitions, the IVT calculation, the regional AR-pulse detection threshold, the clustered/isolated/transitional classification rules, event windows, matching variables, bootstrap procedure, sensitivity tests. We will also describe how accumulated ERA5 fluxes are converted and how Qnet and melt-equivalent potential are calculated.

Comment 4. The AR detection and attribution method is unclear.
Response: We will clarify that the primary framework is a regional IVT-threshold method designed to capture AR-like moisture forcing over predefined sea-ice sectors. We will not present it as a complete object-based AR tracking algorithm. To address detection-method uncertainty, we will add an independent object-catalogue check using the PIKART AR catalogue. We will also add sensitivity tests for the IVT percentile threshold and the clustering-gap definition, and we will explain why the isolated-event window is used as a recovery-window endpoint.

Comment 5. The manuscript does not isolate AR effects from cyclones or other synoptic weather.
Response: We agree with the reviewer that the impact of atmospheric rivers cannot be fully separated from the accompanying synoptic systems, including extratropical cyclones, blocking patterns, and other weather disturbances, in a purely observational framework. In the revision, we will therefore avoid attributing sea-ice response to AR moisture transport alone. Instead, we will treat regional AR pulses as moisture-transport episodes embedded within broader synoptic environments. To reduce this confounding, we will compare clustered and isolated pulses using matched-event controls for month, cumulative IVT, pulse duration, peak IVT, pre-event SIC, and pre-event SKT, and we will explicitly discuss the remaining influence of cyclones and circulation background as a limitation. We will also use sea-level pressure / circulation diagnostics and the 2012 cyclone-influenced case as supporting context rather than as the basis for the general conclusion. Thus, the revised interpretation will focus on the melt-active energy-input window associated with AR-pulse timing, while acknowledging that the parent synoptic circulation partly controls both AR occurrence and sea-ice response.

Comment 6. Sea-ice data are not described in the Methods.
Response: We will add a full sea-ice data description. ERA5 SIC will be used for event-state matching, OISST will be used as an independent daily sea-ice consistency and event-response check, and OSI SAF drift vectors will be used to constrain dynamic redistribution. We will explain the role of each product separately and avoid treating SIC as the only measure of sea-ice impact.

Comment 7. Initial sea-ice state differs between the 2012 and 2020 events.
Response: We will remove the dependence of the main conclusions on a direct 2020-versus-2012 comparison. The revised main analysis will use a multi-year(1990-2024), multi-region(the cross-dateline sector, the East Siberian-Laptev, Chukchi-Beaufort, and central Arctic) event framework and match clustered and isolated pulses by month, cumulative IVT, duration, peak IVT, pre-event SIC, and pre-event SKT. The 2020 and 2012 events will be moved to the Supplement as illustrative cases, and the 2012 case will be described as dynamically influenced rather than as a pure isolated-AR thermodynamic control.

Comment 8. The results need corrected figures, precise numbers, statistical significance, and limitations.
Response: We will rebuild the figure set and align every figure, caption, and text reference. The revised Results will report effect sizes, uncertainty intervals, matched-pair tests, sensitivity tests, and event-level statistical units. We will treat events, not grid cells, as the statistical unit. We will also add a year-block bootstrap to address potential same-year dependence among events. A separate limitations section will discuss AR detection, sea-ice thickness mass-budget limitations, and circulation confounding.

Comment 9. The manuscript lacks citations after the Introduction.
Response: We will add citations throughout the Methods, Results, and Discussion where broader scientific context, data products, reanalysis limitations, AR detection, surface-energy budgets, sea-ice drift, and the 2012 Great Arctic Cyclone are discussed. Dataset citations for ERA5, OISST, CERES, PIKART, and OSI SAF will be included in the Data and Methods and Code and Data Availability sections.

Comment 10. The manuscript contains nonstandard formatting and language that resembles generative-AI style.
Response: We will conduct a full language audit. The revised manuscript will remove nonstandard bold and italic emphasis, excessive quotation marks, unusual coined phrases and unsupported superlatives. The revised style will be plain scientific prose with direct methods, evidence, and limitations.

Comment 11. The tone contains extreme or inappropriate language.
Response: We will remove terms such as "paradigm shift", "catastrophic", "colossal", "final proof", "tipping point", "runaway state", "pressure cooker", "assault", and similar expressions. Claims will be written in terms of diagnosed associations and stated limitations. The revised manuscript will avoid proof-like language and will use measured or diagnosable quantities.

Comment 12. The conclusion is too strong and should reflect the actual work and limitations.
Response: We will rewrite the Conclusions as a single evidence-based paragraph. The conclusion will state that clustered regional AR pulses can enhance melt-equivalent potential when they extend a near-melt, positive-Qnet window. It will also state that the realized SIC response remains modulated by sea-ice dynamics, regional ice state, and the spatial alignment between moisture forcing and vulnerable ice. We will remove broad claims about "the New Arctic".

Comment 13. The title and abstract are misleading because the original manuscript analyzes two case studies.
Response: We will retitle and rewrite the abstract to match the revised analysis. The revised title will emphasize temporal clustering and melt-equivalent potential rather than a general clustered-versus-isolated contrast based on two cases. The abstract will describe the multi-year, multi-region event framework, the matched-event design and the main limitations without implying unsupported pan-Arctic generality.

Figure-related comments

Comment 14. Figure captions contain discussion and do not consistently match the figures.
Response: We will rebuild the main figure set and shorten the captions so that they describe only the variables and windows. Interpretation will be moved to the Results and Discussion text. We will also remove commas in units and standardize units across captions, axes, and colorbars.

Comment 15. Locations mentioned in the Results should be labelled on at least one figure.
Response: We will include a sector map showing all four Arctic analysis regions and their geographic labels. The map will replace the unclear location references in the original manuscript and will make the regional framework explicit.

Comment 16. The original Figure 1 could include sea-ice extent to support the 2012/2020 low-sea-ice-year framing.
Response: Because the revised manuscript no longer relies on 2012 and 2020 as the main evidence, we will not build the main argument around a two-year sea-ice-extent comparison. Instead, the main figure will show the conceptual framework and the analysis sectors. Sea-ice state will be handled through event-state matching and the independent OISST consistency check.

Comment 17. Spatial figures need clearer AR outlines, time windows, contours, and colorbar definitions.
Response: We will define the averaging or accumulation window for every spatial composite. Where contours or masks are used, their meaning and threshold will be stated. AR uncertainty will be handled through the PIKART object-catalogue check.

Comment 18. Several original figures and captions were mismatched.
Response: We will discard or rebuild the inconsistent original figures. The revised figure set will have a one-to-one correspondence among figure image, caption, and text reference. The 2020 and 2012 six-panel case figures will be retained only as supplementary illustrative cases, with captions that clearly state their role and time windows.

Comment 19. The "spatial attribution" figure needs a method description.
Response: We will add a Methods subsection explaining how gridded spatial composites are calculated from matched clustered-isolated event pairs.

Technical and line-specific comments

Comment 20. Acronyms, capitalization, punctuation, and spacing need correction.
Response: We will standardize acronym usage by spelling out each acronym at first use and using the acronym thereafter. We will use lower-case "atmospheric river" except where required by grammar or titles. We will remove unnecessary quotation marks, remove nonstandard capitalization, and standardize units and symbols.

Comment 21. "At the melting point" and "partial thermodynamic recovery" need precise definitions.
Response: We will define the near-melt threshold explicitly using SKT and test the sensitivity to nearby thresholds. Recovery will be expressed using quantified diagnostics such as post-event Qnet, post-48 h negative-Qnet fraction, and near-melt duration.

Comment 22. Peak intensity and IVT terminology need clarification.
Response: We will define IVT and distinguish peak IVT, cumulative IVT, and pulse duration. These variables will be used in the matching design to separate timing effects from intensity and duration differences.

Comment 23. The manuscript needs clearer justification for the 48 h clustered and 120 h isolated thresholds.
Response: We will present the 48 h gap as the default clustered-pulse criterion and the +/-120 h window as the isolated recovery-window endpoint. We will add sensitivity tests using alternative cluster gaps and IVT thresholds so that the conclusions do not depend on a single threshold choice.

Comment 24. The climatology or anomaly baseline needs to be defined.
Response: We will define each event window and baseline used in the analysis. Where anomalies are used, the reference period or event-relative baseline will be stated. For the revised main analysis, we will emphasize matched clustered-minus-isolated contrasts and response-window integrals.

Comment 25. The original text contains unclear terms such as "quiescent", "arteries", "thermodynamic defense", "thermal stacking", "Super-AR", and "runaway state".
Response: We will remove these terms and replace them with standard physical diagnostics: regional AR pulse, IVT, Qnet, DLR, SKT, near-melt duration, melt-equivalent potential, response-window shortwave contribution, and post-event recovery.

Comment 26. Claims such as "nearly 100%" conversion, "zero thermal resistance", and "exponential-like increase" need support or removal.
Response: We will remove unsupported conversion and proof-like claims. Melt-equivalent potential will be defined as a diagnostic upper-bound susceptibility metric. Its physical interpretation and limitations will be stated explicitly.

Comment 27. The manuscript should explain why SIC response can differ from energy input.
Response: We will add an "energy-response gap" discussion. This will explain that melt-equivalent potential is a thickness or volume-susceptibility metric, whereas SIC is an area-fraction metric affected by drift, compaction, divergence, melt ponds and retrieval or reanalysis uncertainty. OSI SAF drift diagnostics will be used to constrain part of the dynamic contribution.

Comment 28. The 2012 event should be contextualized because it is associated with a major cyclone.
Response: We will treat the 2012 case as an illustrative dynamically influenced case and move it to the Supplement. It will no longer serve as the sole isolated-event control for the main conclusion. The discussion will cite the Great Arctic Cyclone literature and will state that the revised general conclusions come from the multi-event matched analysis.

Comment 29. The manuscript should discuss summer polar day and the role of radiative cooling.
Response: We will frame the recovery-window mechanism in terms of Qnet and post-event negative-Qnet fraction. This better reflects summer polar-day conditions, where shortwave availability and cloud-transmission changes can be central to the energy budget.

Comment 30. The manuscript needs more transparent statistical design.
Response: We will state that the event is the statistical unit. Matched comparisons will be done at the event level rather than by treating grid cells as independent samples. We will use bootstrap confidence intervals, paired non-parametric tests, sensitivity analyses, and year-block bootstrap resampling to account for event dependence within the same summer.

Comment 31. Code availability and AR-detection code need clarification.
Response: We will provide a reproducibility package. The Code and Data Availability section will specify which datasets are public and which processed files will be archived.

Comment 32. Data availability needs to describe all sea-ice products.
Response: We will revise the Data Availability section to list ERA5, OISST, CERES, PIKART, and OSI SAF. We will no longer mention a sea-ice dataset only in the acknowledgements without describing it in the Data and Methods section.

Comment 33. The AI-related acknowledgement and manuscript style raised concerns.
Response: We will remove the problematic acknowledgement wording and ensure that any statement about language assistance complies with The Cryosphere policy. Independently of the acknowledgement, we will revise the manuscript into standard scientific prose.

Sincerely,

Yuzhi Wang