egusphere-2024-320
"Quantifying the Impacts of Atmospheric Rivers on the Surface Energy Budget of the Arctic Based on Reanalysis"
Response to the Reviewers
Second Revision Response to the Reviewers

By Chen Zhang, John J. Cassano, Mark Seefeldt, Hailong Wang, Weiming Ma, and Wen-wen Tung

We appreciate that the inclusion of the new metric and corresponding results shown in panel (c) were well received by the reviewers in the first round of revisions. In the following, we respond point-by-point replies to each reviewer's comments in blue, using an italic font to indicate text that has been copied verbatim from the Reviewer's reports. We also supplement this revised submission with a copy of the manuscript in which changes from the previous version are highlighted in red.

Reply to Reviewer #2, Jonathan Wille**:**
We thank the reviewer for the detailed and insightful comments in the first round that improved our manuscript. We are glad that the reviewer found our answers satisfactory. We have also made all suggested changes this time.

Reviewer #2: Jonathan Wille
I am happy with the response to my comments along with the response to the other reviewers who made important points regarding the study's methodology. I have one technical comment related to the added text.

Line 605-606: I find this sentence confusing. How can positive AR anomalies in turbulent heat "contribute only moderately negatively to TH climatology…"? Shouldn't the AR anomalies contribute positively or make the TH climatology slightly less negative?
Reply: Thank you for pointing it out. We have changed to "*Over subpolar oceans, substantial positive AR TH anomalies act to partially offset the strong negative TH climatology, averaging a -7% to -11% relative to its climatology*".

Reply to Reviewer #3:
We truly appreciate the Reviewer's detailed and thoughtful critique for further clarification.

Reviewer #3:
1. The addition of the new panel C in Figures 2-3 and 5-7 is appreciated and helps illustrate how important ARs are to each term of the surface energy budget. However, the original metric still does not seem mathematically sound enough for the conclusions that are being drawn from it.

In the SEB contribution metric, the relative importance is inflated in some regions since the net SEB is near zero. Therefore, even a very small anomaly (e.g., < 8 W m^{-2}) is deemed to have a massive contribution even if it isn't enough to make any kind of material difference to the SEB/temperature/etc. This is especially true over land regions where seasonal net SEB can be very small (e.g., SEB = 0.4 W m^{-2} for Greenland in MAM). This is an especially problematic issue because some of the terms contributing to the SEB are of higher magnitude than the net SEB, with correlated but offsetting terms (e.g., LWD v. LWU or LWnet v. SWnet) can make the SEB near zero seasonally. Because of this, you can have anomalies that seem large when compared to net SEB, but actually are only a small contributor to seasonal totals (which is illustrated well in Figure 2 panels c vs d).

If you consider the spatial pattern of net seasonal SEB, it is evident that the spatial variation in the relative contribution results (e.g., Figure 2d) are determined primarily by variation in the denominator (net SEB) rather than the numerator (e.g., LWD, LWnet, etc.). For example, in Figure 2d, there are much higher relative values over land than over the ocean, especially in Fall and Winter, least so in Summer. This tracks exactly with the net SEB being practically 0 W/m2 over land in Fall and Winter and appreciably positive (downward) in Summer. Every variable (including some that counteract each other) gives higher relative results over the continents. This issue is well demonstrated by Spring in Figures 2 (LWD) and 3 (LWnet). Comparing central Siberia to the Laptev Sea, LWD anomalies are clearly stronger over land than over the Laptev Sea (Figure 2b). In relative terms, the continental anomalies are also higher than the Laptev Sea (Figure 2d). Both central Siberia and the Laptev Sea have smaller absolute anomalies in LWnet than LWD – and the anomalies about the same continent versus land (Figure 3b). However, the relative impact again shows the continental anomalies being higher. It doesn't matter if the land has higher magnitude absolute anomalies than the ocean or not – both Figure 2 and Figure 3 yield higher relative anomalies for the land. In other words, the relative figures are telling us a lot more about the denominator than the numerator, meaning Figure 2d and Figure 3d are primarily telling us about SWnet rather than LWD or LWnet.

Caution should be used when drawing conclusions from a metric that can be easily skewed. The relative metric used is not mathematically incorrect, but the issue lies in how strongly the conclusions are drawn from said metric. For example, lines 914-916: "ARs generate relatively smaller absolute anomalies in net SEB over continental areas (Fig. 7b), mainly attributable to the LWN. However, their impact on the mean SEB is substantial, especially in cold seasons (24-90%), far exceeding the corresponding frequency, which is primarily due to the smaller mean SEB (Fig. 7a)." Here, it is stated that the "large contribution" is really just because of the small net SEB. However, the relative contribution metric is still used consistently to mathematically compare to the frequency and draw the main conclusions (e.g., in the abstract), even though it is clear that it is easily skewed.

The goal of understanding AR's contribution to seasonal net SEB can even be well approximated by directly comparing the mean absolute anomalies during ARs to the mean seasonal conditions for each term and the net SEB. Many of the conclusions can *still be made supported by the values shown in panels (b) and (c), rather than strongly relying on panel (d) for the main conclusions. Therefore, although calculating a relative anomaly is mathematically fine (and they can stay in the results), relying on such anomalies in this case is logically flawed, and the authors should change the focus of the conclusions and abstract to reflect this.*

Response: We sincerely appreciate the Reviewers' detailed and insightful comment regarding the interpretation of the relative contribution metric to the net SEB (panels (d)). We agree that the results of the relative metric to net SEB (panels (d)) is primarily influenced more by the magnitude of the net SEB itself-particularly over continental regions and in cold seasons where the climatological net SEB is near 0- rather than solely by the magnitudes of term anomalies (panels (b)). As the Reviewer rightly points out, this can lead to disproportionately large relative values, which may give a misleading impression of physical significance of AR impacts.

In response, we have taken the following steps:

1. **Clarified and revised interpretation in main text.**
   We have revised our interpretations throughout key sections (e.g., 4.1-4.3) to reflect a more cautious interpretation. Specifically, we now explicitly state that the high relative values in panel (d) are largely driven by small background of net SEB values, especially over land in cold reasons, and do not necessarily indicate large absolute AR-induced SEB term anomalies.

2. **Revised Abstract and Conclusions sections.**
   We have updated the Abstract and Conclusions to shift the emphasis away from the relative net SEB contribution metric (panel d). Instead, we highlight the absolute anomalies and their relative contribution to the corresponding climatology (panels b and c), which provide a more physically direct interpretation of AR impacts. We also include a statement of caution regarding the use of relative metric, clarifying that it should be interpreted in context and not isolation to draw conclusions. We also emphasize that both absolute anomalies and relative contributions, both to climatological means and to net SEB, are complementary and together provide a more comprehensive understanding of AR-induced surface energy budget changes.

3. **Panels (d) with adjusted framing**
   We continue to keep the panels (d) in the main figures and corresponding discussion since the goal of the manuscript is to estimate the relative contribution of different SEB components to the net SEB across the Arctic. However, the results of panel (d) are no longer used as the main basis for our key conclusions, but as the supplementary metric.

2. I agree with other reviewers that the net SEB (Figure 7) should be shown much earlier (even before Figure 2 since the results in Figure 2 are relative to the net SEB). It would be helpful to illustrate the regions of low net SEB earlier so it's easy to see why those >> 100% contributions exist.

Response: We appreciate the Reviewer's point and now agree that presenting the net SEB earlier can help contextualize the contribution of individual SEB components to the net SEB (panel d). In response, we have moved the net SEB figure earlier in the manuscript (now shown in Figure 3 in the manuscript) to help readers visualize the spatial distribution for the rest of the analysis. While we did not place it before Figure 2, we adjusted its position in accordance with the logical flow and figure order mentioned in the manuscript.

While the figure itself has been moved forward, we retain the in-depth discussion of the net SEB (now Figure 3) in its original location to proceed through other individual terms in the SEB first and ending with the net SEB, which sums the previously discussed results. All corresponding figure references and related text have been updated to reflect this reordering. We hope this reorganization improves the overall clarity and flow of our discussion.

Technical comments:
• Fig 2: the colourbars for panels (c) and (d) are very close to being the same but are slightly different – suggest using the same colorbar if they're almost identical to make it easier to interpret
Response: Thank you very much for your thoughtful suggestion. We agree that visual consistency between color bars can enhance interpretability. However, while the color bars for panels (c) and (d) are similar in structure, they are intentionally not identical due to the difference in the magnitude of the values displayed. Panel (c) shows much smaller magnitudes than panel (d) and using the exact same scale would obscure important spatial variations in panel (c). To improve clarity and comparability, we designed the color bars such that the scale from 0% to 4% is subdivided more finely in panel (c), allowing key features to be more visible. Beyond 4%, both panels share the same scale. We believe this approach balances the need for consistency with the need to clearly visualize the distinct features in each panel.

• Line 338: "the modest large contributions" – unclear how the contribution is both modest and large (perhaps "the modest increase in contributions" is what is meant here)
Response: Fixed it.
• Line 474-475: "except in winter when reduced climatological LWN cooling leads to a slight increase contribution"→ "a slight increase in contribution"
Response: Fixed it.
• Supplemental figures: for the statistical significance figures, "The grey dots are plotted over regions of anomalies outside of the 95% confidence intervals based on two-tailed t-test" is unclear – Are the dots shown where the anomalies are not statistically significant at the 95% level
Response: We have revised the description to *"The grey dots indicate regions where the anomalies are not statistically significant at the 95% confidence level, based on a two-tailed t-test."* We hope this updated wording clarifies the intended meaning.

Reply to Reviewer #4:
We express our gratitude to Reviewer#4 for their insightful and constructive feedback aimed at enhancing our manuscript.

Reviewer #4:

This study quantitatively assessed the contributions of AR on the Arctic surface energy budget for four decades using ERA2 and MERRA 2 reanalysis data. The analysis noted that the ARs resulted in positive LWD anomalies in all seasons, particularly strong in transition seasons, which positively contributed to the net SEB. The AR impacts were also linked to the Arctic sea ice retreat, particularly in sea ice boundary regions, and Greenland glacier melting, which is likely a main result of this study compared to the AR related previous studies.
Response: We appreciate the Reviewer's thoughtful summary of our study. However, we would like to clarify that the current study is based solely on the ERA5 reanalysis date. We do not use ERA-Interim (ERA2) or MERRA-2 in this analysis. We hope this clarification helps avoid confusion regarding the data sources.

This study highlighted the impact of the AR induced energy budget anomalies on sea ice and Greenland glacier melting. The relationship between ARs and Arctic cryospheric changes, particularly in the Arctic Ocean is certainly an interesting topic in climate change researches, and the resultant findings will highly contribute to understand the arctic amplification as well as the future projection. Here it is questioned on why this study was mainly focused on the Arctic Ocean. For example, the AR certainly resulted in positive LWD anomalies in all seasons, while the contribution of the anomalies on the SEB components was overwhelmingly larger in land than ocean. Of course, the manuscript shortly included the point. However, the AR impact related description and discussion was almost directed to the sea ice and glacier. In the cold season when sea ice is formed, grown, and melted in the ocean, snow cover is simultaneously appeared and disappeared on land surface, as well as permafrost is also freezing and warming. The AR induced changes in SEB likely affect the land snow and permafrost, because which are largely implicated to the Arctic climates and arctic amplification, having impacts equivalent to the sea ice change. Therefore, you have also equally to deal with the impacts on the land in the manuscript.
Response: We sincerely thank the Reviewer for this insightful comment highlighting the importance of considering AR-induced energy budget changes over land surfaces. We agree that AR-driven SEB anomalies have the potential to significantly impact snow cover and permafrost, especially during cold seasons, and that these land-based cryosphere changes play an equally important role in Arctic amplification as sea ice and glacier responses. In response to this helpful suggestion, we have expanded the corresponding section to more thoroughly address the potential implications of AR-induced SEB terms anomalies for snow cover impacts and permafrost stability.

For example, regarding the discussion of LWD, we include the statement in Lines 319-321: *"These AR-related LWD anomalies are particularly important over continental regions where they may contribute to the warming and thawing of seasonally snow-covered ground and permafrost, potentially altering surface energy budgets and hydrological process (Guan et al., 2016; Goldenson et al., 2018)."*

The Lines 445-447 are included for the LWN: *"While smaller in magnitude, the AR-induced LWN anomalies also appear over Arctic continental regions. In cold seasons, the anomalous LWN from ARs can contribute to earlier snowmelt, reduced snow accumulation, and altered permafrost thermal regimes"*

In the Conclusions section, we also expand on that to acknowledge the broader relevance of our findings to Arctic land processes and climate feedback. Such as in Line 978-980: "Nonetheless, even moderate AR-driven SEB perturbations might have significant impacts on Arctic continental environments, potentially accelerating permafrost thaw, altering snowpack evolution, or affecting surface hydrology, ultimately shaping land-surface processes."

The ARs induced higher LWD also resulted in higher SST, which increased outward longwave radiation, consequently lower LWN, which was in turn offset by the negative SWN caused by the ARs. Based on a simple budget calculation, the energy source that likely forced the sea ice and glacier melting was probably TH anomalies from the atmosphere. The resultant net SEB anomalies caused by the ARs were less than approximately 20 W/m2 in the sea ice covered ocean. It is questioned on how much the 20 W/m2 can derive sea ice melting. For instance, in spring that sea ice starts to melt, snow over the sea ice is melted earlier than the sea ice melting. These process suggests that the AR induced net SEB may be almost used for the snow melting, although it depends on snow amounts. Therefore, the assertation that the ARs induced net SEB likely triggered the sea ice and glacier melting is an overstated expression. If the net SEB affected the sea ice and glacier, the influence was likely limited to the sea ice margins, particularly North Atlantic, Barents, and Chukchi sea where SST was relatively warm. This was included in the manuscript, but the description could cause a misunderstanding that the ARs likely affected the sea ice melting of the Arctic scale.
Response: We sincerely appreciate the Reviewer's thoughtful critique and detailed discussion regarding the interpretation of AR-induced net SEB anomalies and their potential role in sea ice and glacier melt. We agree that caution is warranted when attributing sea ice and glacier melt directly to AR-induced net SEB anomalies, especially given the complex processes involved in the seasonal melt transition.

In response, we have revised the relevant statements in the revised manuscript to avoid overstatement. Specifically:

Lines 325-326 (regarding AR-related LWD anomalies): *"However, the degree to which these anomalies contribute to sea ice melt or delay ice growth likely depend on local cryosphere conditions, such as snow and ice conditions."*

Lines 696-699 (regrading AR-related net SEB anomalies): *"However, the extent to which these anomalies directly trigger sea ice melt likely depends on local snow and ice conditions, and the influence is expected to be most relevant in regions with relatively warmer sea surfaces such as the Barents, Chukchi, and Arctic suboceanic sectors. In areas with thicker snow cover or colder background conditions, these anomalies may have more limited or indirect effects."*

Lines 938-941 in the Conclusions Section: *"These short-term increases in the SEB potentially hinder sea ice refreezing in fall and winter (Zhang et al, 2023b) and may trigger sea ice melt in spring (Huang, Dong, Bailey, et al., 2019; Huang, Dong, Xi, et al., 2019). Nonetheless, the impact of these anomalies on initiating sea ice melt is contingent upon regional snow and ice conditions, with the most pronounced regions characterized by warm sea surface temperatures"*

Line 947-949 in the Conclusions Section: "*In spring, the combination of large AR-induced LWD anomalies and a smaller net SEB climatology result in a substantial relative contribution to the climatological net SEB. This suggests that AR-driven SEB anomalies play an important role in modifying the climatological SEB, potentially supporting early-state melt in spring, particularly of snow over sea ice, and influencing the minimum sea ice extent in fall (Huang et al., 2019).*"

This study was based on seasonal means during the period 1979–2019. If the AR induced anomalous energy budgets affected the sea ice and glacier melting, the impacts could be more significant at the recent decade when the climates were relatively warm. That is, the anomalous impacts include larger uncertainty in the interannual variability. Furthermore, the seasonal means have also the similar uncertainty. For example, in spring when the ARs induced net SEB anomalies were relatively large, if the AR events were biased to the early spring of colder climates, the impacts on the sea ice and glacier could not represent the seasonal variability. Of course, there are the contrast cases.

Response: We thank the Reviewer for this important comment regarding the variability of AR-induced anomalous energy budgets and their impacts on sea ice and glacier melt. We agree that interannual variability could influence the actual impact of ARs. We also acknowledge that ARs may exert a stronger influence on surface melt in recent, warmer decades or later in spring, when snow and ice are more vulnerable to energy inputs. Therefore, we have incorporated a new discussion section (now Section 6.4, Limitations of the analysis) to better acknowledge the limitations of our seasonally averaged approach and to highlight the potential for greater AR impact in warmer recent years, as follows in Lines 913-919:

"This study is based on seasonal-mean composites from 1980 to 2019, which inherently mask the interannual and intraseasonal variability of AR-induced SEB anomalies. While the analysis discusses the potential seasonal climatological impacts of ARs, their actual impact on sea ice and glacier melt is likely modulated by the background climate state. For example, in warmer recent decades or in late spring when surface snow and ice are more susceptible, the same AR-induced energy anomalies may exert stronger melt impacts than in earlier, colder periods. Furthermore, seasonal averaging may obscure temporal biases within the season—if ARs tend to occur in early spring, their potential to affect surface melt could be diminished despite large seasonal-mean anomalies."

The AR induced anomalies resulted from the limited days that the ARs occurred. Following a simple calculation based on the AR occurrence frequency, the percentages of 10-12% mean approximately 10 days of AR occurrence in individual seasons. We don't have knowledge on how long the AR caused anomalies are maintained and affected. The impacts of the anomalies are likely dependent on the maintenance length. This is considerably complex. The manuscript could give a misunderstanding like that the ARs induced anomalies have likely continuous impacts. Therefore, you have clearly to describe the limitations of composite analysis, with the related discussions.

Response: We thank the Reviewer for raising this important point regarding the persistence of AR-induced SEB anomalies. We agree that the impacts of the anomalies are likely dependent on the maintenance length. We have also included this point in the new discussion section (Section

6.4) to clearly acknowledge the limitations of our composite approach, as follows in Lines 919-913:

"Another limitation stems from the composite approach itself: AR-induced anomalies reflect short-lived perturbations associated with limited AR occurrences (10–12% or ~10 days per season). Our results suggest the potential cumulative impact of these short events, but they should not be interpreted as sustained seasonal effects. The actual influence depends on the persistence and timing of the anomalies, which we do not resolve in this analysis. Future work using event-based or lagged analyses is needed to evaluate the duration and full impact of ARs on sea ice and glacier melt."

The result section is quite long. Thus it needs compactly to rewrite it, particularly 3.2.1, which is recommended to divide into the sections of LW and SW.
Response: We thank the Reviewer for this helpful suggestion. In response to this comment, we have substantially reorganized the Results section during our first-round revision. Specifically, the original Section 3.2.1 has now been divided into three more focused subsections to improve clarity and readability:

- **Section 4.1.1**: Surface downward longwave radiation (LWD)
- **Section 4.1.2**: Net surface longwave radiation (LWN)
- **Section 4.1.3**: Net surface shortwave radiation (SWN)

The discussion section includes a discussion about uncertainty related to two methods in AR production. The difference only represents the impact of the different spatial scales. Despite the two methods were used similar sources of reanalysis data, they resulted in larger differences in AR detection. It additionally needs a comparison with the AR result detected from the different reanalysis data with this result.
Response: We appreciate the Reviewer's thoughtful comment regarding the need to further examine uncertainties associated with reanalysis products. However, extensive literature has shown that the variability in AR characteristics and their associated impacts is far more sensitive to the choice of AR detection methods than to the differences among reanalysis datasets themselves (e.g., Collow et al., 2022; O'Brien et al., 2022; Shields et al., 2022). In polar regions specifically, studies have demonstrated that AR characteristics and impacts are largely consistent across different reanalysis datasets—both in the Arctic (Zhang et al., 2023) and the Antarctic (Wille et al., 2019; Wille et al., 2021).

As noted in Section 2.1 of our manuscript, we selected the 85th percentile IVT-based AR index applied to ERA5 because ERA5 provides higher spatial resolution (0.25° × 0.25°) and has been shown to offer improved accuracy for AR detection and tracking compared to the coarser-resolution MERRA-2 (0.5° × 0.625°), particularly in polar regions (Zhang et al., 2023). Given this, we believe our current approach using ERA5 offers a more reliable assessment of AR features in the Arctic.

It is helpful if you could provide quantitative assessments on how the AR induced anomalous SEB, for example LWD 20 W/m2, contributes to sea ice melting or glacier.
Response: We appreciate the Reviewer's suggestion to quantitatively assess the contribution of AR-induced SEB anomalies—such as the example of a +20 W/m² LWD anomaly—to sea ice or glacier melting. This is an important point, and we have added the corresponding statement to provide a first-order estimate of the potential melt energy implied by the anomalous SEB. We have added this estimate in Section 4.3 of the revised manuscript to better contextualize the physical implications of the observed SEB anomalies in Lines 698-703:

"To provide context, we note that and additional +20 W m^{-2} sustained over a 10-day AR period over one season corresponds to a cumulative energy input of approximately 17.3 MJ m^{-2}. Assuming all of this energy goes into melting ice and using the latent heat of fusion for ice (~334 kJ kg^{-1}), this would be sufficient to melt roughly 5.2 cm of ice (52 kg m^{-2} of sea ice). While this is a simplified calculation and does not account for other energy sinks such as sensible heat flux, conduction, or the energy required to melt snow first, it does suggest that AR-induced anomalies can provide meaningful energy for localized melt, particularly near sea ice margins or glacier ablation zones."

The conclusion section seems a repeat of the discussion section. It has compactly to re-summarize the major results of this study firstly, then the meaning of the finding and influence, including the lacking and next step.
Response: We thank the Reviewer for this thoughtful suggestion. In response, we have revised the Conclusion section to be more concise and focused. It now summarizes the key findings, highlights their broader implications for Arctic climate, with the lacking and next step detailed stated in the Discussion section.

Based on these comments, the manuscript is needed a major revision.
Response: We appreciate the Reviewer's detailed and constructive feedback. In this second-round revision, we have carefully addressed all comments and incorporated the suggested changes throughout the manuscript. We believe these revisions have substantially improved both the clarity and overall quality of the study.

References:
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