Responses to Referee #1

Dear Authors,

Upon re-evaluation, I appreciate the efforts made in revising the paper. However, I must first acknowledge that during my initial review, I overlooked the following published works by the authors:

Liang, H., & Su, J. (2021). Variability in sea ice melt onset in the Arctic Northeast Passage: Seesaw of the Laptev Sea and the East Siberian Sea. Journal of Geophysical Research: Oceans, 126, e2020JC016985. https://doi.org/10.1029/2020JC016985
Liang, H., and W. Zhou, 2023: Arctic Sea Ice Melt Onset in the Laptev Sea and East Siberian Sea in Association with the Arctic Oscillation and Barents Oscillation. J. Climate, 36, 6363–6373, https://doi.org/10.1175/JCLI-D-22-0791.1

Both of these earlier works, as well as the current study, draw connections between melt onset modes and the atmospheric state. They highlight the roles of wind, surface temperature, and total water vapor in shaping melt onset patterns in the region.

At this stage, I find insufficient evidence of significant scientific novelty or advancement in the presented study compared to the authors' previous works. Given the importance of contributing new insights or methodologies to scientific knowledge, I recommend rejecting the manuscript in its current form.

Always grateful for all the constructive comments from the reviewer.

Here, we give a brief statement about the potential value of this study:

This study supposes the metric of Melt Advance in the Laptev Sea and East Siberian Sea, which can be defined on the same date each year and has the potential to be used in the practical seasonal prediction of summer sea ice cover instead of the average melt onset. In addition, this study gives detailed accounts of dynamic and thermodynamic processes related to different Melt Advance scenarios in this region, which implicates considerable interannual and interdecadal variability in springtime conditions.

Responses to Referee #2

I am looking at this manuscript after a first round of revisions and did not review the original manuscript; therefore, I am focusing my attention on the comments from the previous reviewers and how whether the manuscript changes adequately address those concerns. The short answer: I think this paper could be a valuable contribution to The Cryosphere, but I also think there are still necessary revisions, both to bolster the most novel aspect and to refine the physical interpretations improved upon following the first round of revisions.

Thank you for the constructive comments. We have addressed the key issues below (hopefully) and made improvements to the manuscripts accordingly.

ISSUE A: Is it novel enough? Both reviewers commented on the melt advance metric being a positive aspect of this paper – since it is a different way of presenting melt onset data. But whether this metric provides notable gains in predictability was questioned.

I don't see notable strides in understanding of the physical drivers of melt onset in the Laptev and East Siberian Seas. Two previous papers by the lead author already cover this extensively (Liang and Su, 2021; Liang and Zhou 2023), as does other literature (e.g., Mortin et al., 2016; Horvath et al., 2021). Therefore, I think the "novelty" aspect of this paper hinges on the potential for melt advance to be a better predictive metric than average melt onset for summer sea ice conditions.

Digging deeper into that aspect, the authors make a solid argument that it is better to have a metric that can be reliably defined on the same date every year. Otherwise, there is uncertainty over when a prediction can be made. Therefore, if melt advance on May 31 (or some other date) performs at least as well as average date of melt onset, the authors can justify melt advance as superior. The problem is that the authors do not compare the predictive skill of melt advance to the predictive skill of the average day of melt onset. Instead, they compare to the skill of May SIC. That's not a bad comparison, to be clear, and I'd keep it. **However, I think they also need to compare melt advance's skill to the skill of the average day of melt onset in order to make the point.**

R: We now compare the prediction skill of melt advance with that of the average melt onset. The results show that they have similar predictive abilities, suggesting that melt advance could be commonly used in the practical prediction of summer sea ice cover.

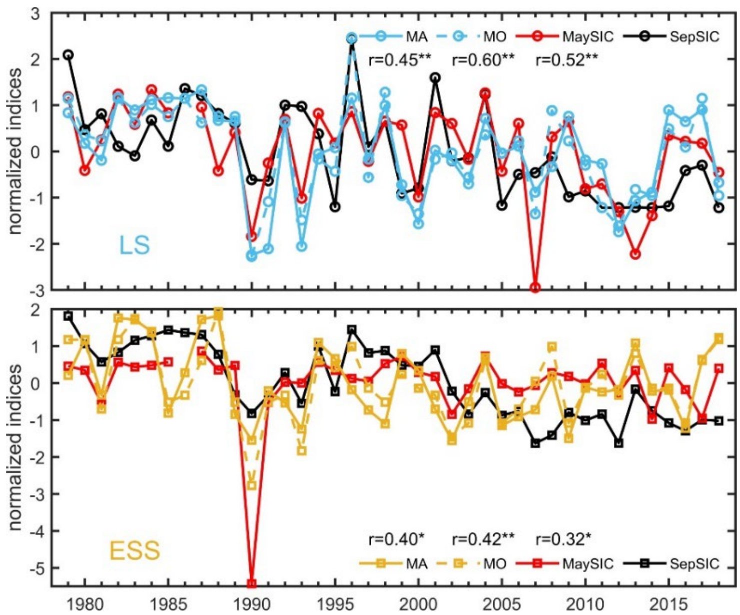

Fig. 7. *Sea-ice surface Melt Advance, average Melt Onset, and Sea Ice Concentration (SIC) in May and September in the Laptev Sea (top) and East Siberian Sea (bottom) from 1979 to 2018. The May and September sea ice cover are denoted by the areal percentage relative to the whole sea. All variables have been normalized. For better visualization, Melt Advance is multiplied by -1. Correlation coefficients with double asterisks denote 99% confidence, while those with a single asterisk denote 90% confidence.*

ISSUE B: Have reviewer concerns about the physically interpretations been adequately addressed? Reviewer 1 wanted more detailed analysis of the surface energy balance – namely, including cloud cover in the analysis and looking at the individual components of the SEB more closely. They specifically wanted turbulent heat fluxes in the main body of the paper. Both reviewers expressed doubts about some of the physical interpretations.

The easy part is that the authors have included more detail from the ERA-5 data. It's primarily in the supplemental, but it is there. The fact that cloud cover in spring is not all that variable, but total column water vapor is, aligns with my expectations. Several places that reviewers noted as problematic have been improved. There are four points, though, that I think still need revision.

1) For the polynya and the LS-faster case, there is a problem with the explanation that westerly winds would enhance polynya presence. If we look back at Figure 1 of Krumpen et al. (2011), they show several polynya areas in a U-shaped within the Laptev Sea (between Severnaya Zemlya and the New Siberian Islands). A westerly wind would indeed be offshore for the west side of this U-shape (i.e., the Eastern Severnaya Zemlya polynya, the Northeast Taymyr polynya, and the Taymyr polynya). However, for the New Siberian polynya, on the east side of that U shape, a westerly wind would be blowing against the fast ice locked in around the New Siberian Islands. That said, I agree that in the current manuscript, Figure 3

does show negative anomalies in SIC for the entirety of the polynya area for the LS-faster case. But the westerly wind explanation is not entirely satisfactory.

R: Yes. The westerly may not fully explain the changes in sea ice and polynya. We have revised the section related to polynya opening in the LS-faster-scenario.

Text: *While the westerlies may not fully account for the reduced sea ice in the LS, this circulation includes an offshore wind component in the western LS, resulting in increased polynya opening. This includes polynyas such as the Northeast Taymyr polynya, the Taymyr polynya, and the Anabar-Lena polynya (Krumpen et al., 2011).*

2) Later, on Lines 254-256, the authors talk about the how the circulation anomaly "pushes sea ice against the southern coast". That's true enough in the western half of the Laptev Sea, but in the eastern half, it's really against the fast ice (although still preventing polynya formation).

R: We regard this as a more precise description.

Text: *pushes sea ice against the southern coast in the western half of the LS, and against the fast ice zone in the eastern half, thereby preventing polynya formation.*

3) Figure 4 shows that no anomaly extends beyond the interannual standard deviation for the ESS for the ESS-faster, LS-faster, or fast scenarios. The authors spend ample time discussing differences in the ESS for the ESS-faster scenario, but if the anomalies are no bigger than normal noise, that makes the analysis seem like over-interpretation. This limitation needs to be at least acknowledged. Perhaps there is more than one atmospheric set up that can lead to the LS-faster or ESS-faster regimes. Perhaps it's too random or there's too much noise from measurement errors to see clearer signals.

(This issue less problematic for slow v. fast, where we're looking at climate change rather than internal variability, and the LS shows more notable differences.)

R: Agree. It is necessary to discuss this signal-to-noise ratio issue in the analysis.

Text: *However, in this case, as depicted in Fig. 4, no anomaly extends beyond the interannual standard deviation for the LS and ESS, suggesting a risk of over-interpretation. One plausible explanation is that the normal state in this region tends to resemble the ESS-faster-scenario, as indicated by Fig. 1a and the higher climatological Melt Advance value in the ESS compared to the LS shown in Fig. 1c. Additionally, multiple atmospheric setups may lead to the ESS-faster-scenario, highlighting the considerable variability in springtime conditions. Hence, the low signal-to-noise ratio is understandable. It's worth noting that the LS exhibits more notable differences, consistent with its significant polynya activity.*

4) Figure 5 also has a statistical issue. The second and third plots in the bottom row for ESS are clearly affected by a single outlier with excessive leverage. The relationships would be different if that outlier were removed – perhaps substantially different.

R: Yes, we have redrawn Fig. 5 and taken into account the effects of outliers. It appears that the linear relationship remains largely unchanged. However, the correlation between MA and NSR in the ESS becomes less significant after removing a single outlier.

Text: *Note that the variability of SIC and NSR in the ESS is smaller than in the LS if the single outlier is removed from the ESS data (see the second and third plots in the bottom row of Fig. 5). Once more, this is related to the significant polynya activity observed in the LS.*

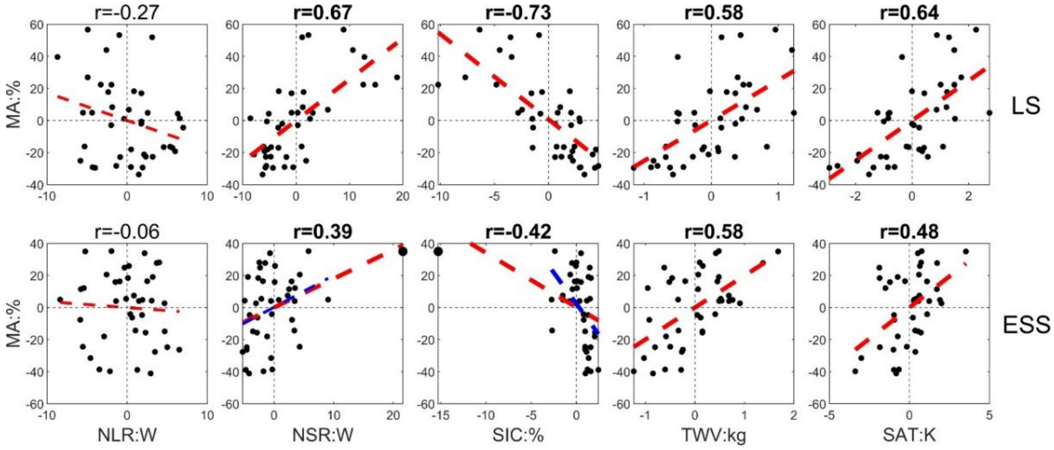

Fig. 5. *Scatter plots for the period 1979-2018, illustrating the relationship between the Melt Advance (MA) anomaly and region-mean anomalies of factors shown in Figs. 3 and 4. Thick dashed red lines represent linear fits above the 95% confidence level. Bold titles represent correlation above the 95% confidence level. In the second and third plots of the bottom row, thin and thick dashed blue lines respectively denote linear fits after the removal of an outlier, identified by a larger black dot.*

ISSUE C: Finally, this wasn't brought up by prior reviewers, but I don't think the Figure 7 schematic is worth it for the manuscript. The words in the text do a much better job summarizing conclusions and the schematic is difficulty to see and read. It's a cool idea, but there's just too much being incorporated for me to see much value. On a poster, where it could be bigger, I think it would work better.

R: We intend to summarize conclusions in that schematic. If it is difficult to read, it might lose its effectiveness. We have removed it for clarity.

References

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Liang, H. and Zhou, W.: Arctic Sea Ice Melt Onset in the Laptev Sea and East Siberian Sea in Association with the Arctic Oscillation and Barents Oscillation, Journal of Climate, 36, 6363-6373, 10.1175/jcli-d-22-0791.1, 2023.
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