Linghan et al present an analysis of two summer atmospheric river (AR) events that coincide with two summers of extreme low Arctic sea ice extent. In addition to these two case studies the authors present a statistical analysis on atmospheric moisture related to AR’s and Arctic sea ice tendency and surface energy fluxes based on 40 years of ERA5 reanalysis data. The paper discusses a relevant topic that can improve the understanding of the drivers of Arctic sea ice variability, but in which way this research adds to previous research on Arctic sea ice extremes could be articulated more clearly. Some of the goals stated in the introduction are not met and therefore need rephrasing. While interesting results and nice visualisations of them are presented, some more explanation and interpretation of the results should clarify the point that the authors want to make. A more detailed discussion on how the methods and results of this study compare to previous studies related to sea ice extremes and atmospheric moisture input would help show the novelty of the presented study. I will elaborate on this with a few major comments, followed by minor comments.

**Major comments:**

- **Articulation of novelty of study**
  The introduction section starts well with a discussion of Arctic sea ice variability and the role that atmospheric rivers and their input of moisture towards the Arctic can play on these sea ice changes. However, the research gap that the authors try to fill is left very generic and unclear (r 36-40). It misses reflection on previous studies that describe the role between atmospheric moisture input and sea ice anomalies and how the approach used in this study is different. Previous studies have focused on convergence of atmospheric moisture or latent energy transport calculated from ERA5/Interim products and their impact through energy balance components on the Arctic surface temperatures and sea ice as well. Can the authors clarify in which way the atmospheric river approach used in this study is different to previous studies and what is the added value of using this approach?

- **Articulation of purpose of study**
  The goal of the study ‘to explore how AR’s influence Arctic sea ice variations’ (r 51) is very generic and needs some refinement. The authors nicely introduce what kind of results we can expect from this study (results from the 2 case studies and the statistical analysis over a long time period), but reflection on why this approach is chosen is missing. What is exactly the authors goal of the 2 case studies?
Contribution of dynamic and thermodynamic effect of AR on sea ice
In the introduction the authors mention ‘This study investigates the relative contribution of surface heat flux components and the relative importance of thermodynamic and dynamic processes in sea ice changes when ARs happen in the Arctic.’ While this is a very relevant aim, which to this reviewer’s knowledge has not been answered in the current literature yet, it is not clear in the current manuscript whether the authors answer this question with the presented results. The authors show that both wind anomalies as well as anomalies in the surface energy fluxes coincide with AR events, however analysis or quantification on their relative contribution is not presented. While the wind fields suggest that there might be sea ice motion, no sea ice motion data products are analysed to show whether these surface winds indeed resulted in sea ice motion.

Timescale statistical analysis
The authors show an extensive statistical analysis of concurrence of extreme atmospheric moisture content with the surface energy balance fluxes and sea ice changes. The explanation on choices that are being made for this analysis and what the main message is from this analysis are not clearly given. Previous studies have shown a delayed impact of moisture on the sea ice (e.g. Kapsch et al 2009, Hofsteenge et al 2022), which is not considered in this study used correlation analysis. Could the authors justify this choice and explain why the analysis will focus on short time scales? Adding significance to the maps of correlation coefficients would strengthen this analysis as well.

Discussion of results and consideration of related work
The authors have chosen to include interpretation and discussion of the results within the results section, which can work well for the presented study. However, further explanation of the presented results would improve the impact of the research. In particular, the results could be brought into context with previous studies more clearly. Some references that could be helpful to bring this paper into context are provided in a list below; it would be interesting if the authors could comment on whether they have any idea whether there is a delayed response of ARs on the sea ice. Lastly, the discussion of these results with previous papers on the role of atmospheric moisture or other factors driving the 2012 and 2022 sea ice minima could be improved. How do the results agree with previous research, and what findings are new or contrasting to the previous studies?

Minor/specific comments:

- 63: Add a reference to studies that used ERA5 successfully for AR detection
- 87-94: The authors mention implications of studying ARs for sea ice prediction: is this the motivation for this work? As reader I get confused which research gap the authors try to fill and how the analysis of the 2 cases studies and statistical analysis are used to answer the research question.
- 105: Move reference to Figure 1b forward (when referred to situation on Aug 5)
- 106-107: I don’t understand how you conclude that ‘surface winds push sea ice away from the ice edge towards the pole’ based? Could it also be sea ice melt that leaded to the sea ice concentration change? Can this be concluded from coincidence of negative and positive sea ice changes nearby indicating transport?
- 110-115: It is interesting to read that you see a stronger influence of the moisture input on the turbulent fluxes compared to the radiative terms, could you discuss further why there could be this difference from previous studies (eg Kapsch et al 2016, Graversen et al 2011, Francis et al 2005, Hofsteenge et al 2022). The role of clouds is mentioned here very briefly as well, and I would be curious to see what the relative role of clouds on the longwave and shortwave radiation components are and how that relates to cloud effects through moisture transport of previous studies
- 110: Magnitude of net longwave radiation is indeed small, but is positive in the area of the AR compared to negative to surrounding areas. This sign switch might indicate an
important switch in the role of LWnet in the energy balance?

- r.132: Similar as in r.106/107; could you explain how you conclude there is a sea ice anomaly through sea ice motion?
- 144-145: It is interesting to see the AR impact on the energy balance components of figure 2b. The energy balance seems always shortwave radiation dominated, leading to a small net energy flux during the night. However, during the AR event net radiation is much larger in the night because of the turbulent fluxes. Enlarging figure 2b would help to see whether the energy fluxes are negative or positive, which is hard to see now, while it there is a shift from usually net LW cooling to LW heating during ARs.
- 187: This is interesting, this cloud effect seems different compared to the 2012 case, since there is an impact on SWnet visible now. Could you discuss shortly the differences between the AR event in 2012 and 2022 and their impact on the sea ice?
- 188: Similarly to 106/107; on which results is based that there is sea ice motion rather than melt?
- 204-206: What explains this difference in response? It would be good if the authors point out that the starting ice concentration is different for both cases, only 0.3 for the 2012 case and 0.7 for the 2020 case. Therefore the 2012 seems a more rapid change, but both show a change of about 0.2 over a day.
- 210 (This AR event ..): This can be moved forward when the AR event used in this case study is described.
- 209 (For this event ... rapid sea ice decrease): How is quantified or concluded that moisture content is more important than wind speeds for the sea ice decrease?
- 225: Here the authors mention rapid sea ice melt, while previously effects of wind that cause the sea ice decrease, this seems inconsistent. Try to specify clearly if the results suggest melt or sea ice motion or whether is not able to distinguish between both.
- 230: Can the authors please conclude on the main difference between the two AR events and their impact on the sea ice
- 294: Here the authors mention rapid sea ice melt, while previously effects of wind that cause the sea ice decrease, this seems inconsistent. Try to specify clearly if the results suggest melt or sea ice motion or whether is not able to distinguish between both.
- 294: Can the authors explain shortly how ARs are identified in this catalog and how that differs from the method used in the manuscript?
- 306: Give a reference or explain why this is to be expected
- 329: Which correlations, positive or negative?
- 343: Please give possible explanation for the positive correlations that are found in figure 8b.
- 345-347: Is the partial sea ice cover more sensitive or are larger fluctuations in the sea ice concentration expected in the marginal ice zone and therefore stronger correlations?
- 360: Precipitation is mentioned here but not discussed in results
- 361 (‘Additionally, dynamic .. near ice margins’): This statement should be softened as it’s suggested from surface wind fields and not shown with sea ice motion products.

**Reference suggestions:**


