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Comment on tc-2021-159

Anonymous Referee #2

Referee comment on "Contribution of warm and moist atmospheric flow to a record minimum July sea ice extent of the Arctic in 2020" by Yu Liang et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-159-RC2, 2021

Review of **'Warm and moist atmospheric flow caused a record minimum July**

sea ice extent of the Arctic in 2020' by Liang et al.

Summary

Liang et al. aims to investigate the July 2020 extreme sea ice melt event in terms of physical mechanisms. They look at the prior late spring-early summer 2020 to explain that anomalous warm air intrusion and cyclone activity set up favorable conditions for sea ice melt in July 2020. I find the idea interesting and well suited for The Cryosphere journal and the methods generally appear sound, however the presentation of their results and the significance of the findings need a bit more elaboration before I could recommend the paper for publication.

Reviewer comments

R.1 I find the Introduction a bit hard to follow. The authors might consider reorganizing it a little bit via discussing the contents of the current second paragraph before starting to talk about the 2020 SIE extent and referring to Figure 1. From row 30 it reads like it is already the description of the Results. I understand the reasoning behind it; the authors want a succinct Introduction to go with their very specific and well-defined goal in the paper, however I think they could do better in setting up the research question.

Especially, I suggest that the authors discuss more thoroughly the current understanding of oceanic and atmospheric drivers of summer sea ice melt, especially the physical mechanisms, as their objective in this paper is to reveal the underlying mechanisms leading to the record melt in July 2020. For example, in the current introduction the authors only mention surface wind driven sea ice drift as dynamical forcing on sea ice, however in recent years anticyclonic circulation anomalies caused vertical motion (warming and moistening descending air) is also a key component of atmospheric forcing on sea ice (see e.g., Ding et al. 2019; Topal et al. 2020). This local atmosphere-sea-ice coupling mechanism is further linked to large-scale circulation changes and forcing from the tropics especially over the enhanced melt period between 2007 and 2012 (Screen and Deser 2020; Warner et al. 2020; Baxter et al. 2019). Therefore, the well-known thermodynamical factors causing sea ice melt may be better linked with known dynamical sources besides surface wind drift, which is far from being the only dynamics causing sea ice variations in the Arctic. In this way the authors may set up their research question a bit more connected to existing literature and highlight that their goal is to complement the existing knowledge of dynamical drivers of sea ice loss which can well be exemplified via a case study in July 2020.

June-August 2020 was dominated by a high-pressure anomaly in the Arctic, which could have acted in concert with the prevailing spring conditions to cause the sea ice extreme melt. I wonder if the authors could provide more discussion on how they distinguish their results or link together with previous literature either in the Introduction or in their Discussion part.

R.2. I would encourage the Authors to use either SIE or SIC in the Introduction, the current version has both of them. Also, in Figure 1, I do not see any gray lines, which would refer to the 2000-2020 SIC climatology. Maybe it would aid the interpretation of Fig. 1 if it had multiple panels instead of the contour lines. The authors might consider plotting the SIC climatologies with shading in Fig 1 b for example.

R.3. In general, in the figure captions it would be helpful not to use abbreviations.

R.4. In many cases, the significance of the anomalies are not clear. In Fig 2, Fig. 4 and Fig.5 it would be necessary to include significance as stippling for the anomalies.

In Fig. 6, I do not see the significance of the results (nor statistically or literally). For example, in lines 237-240, the energy convergence should start early March and peak in June in each year corresponding with solar irradiation seasonality. How are the results presented in Fig 6 differ from the climatology? e.g., a histogram of all 42 years' melt start date could help to point out that 2020 May melt start was statistically significantly earlier than usual. Polishing the discussion of Fig. 6 would be essential to help the reader arrive at the conclusions that the authors set forth.

L264: significant is what sense? If statistically, please provide the p value.

Also, when stating 99% significance, what was the applied significance testing method?

R.4. I think a more thorough discussion of Fig.10a would also improve the paper. Any hints on the seen low-frequency oscillation in the 10-yr trends? Can this be linked with large-scale circulation trends (not SLP, but winds or upper-level geopotential, e.g., 300hPa)?