Response to Referee #1

Review of the Manuscript 'Warm and moist atmospheric flow caused a record minimum July sea ice extent of the Arctic in 2020' by Ling et al. submitted to The Cryosphere.

Summary:

The authors are exploring the atmospheric conditions during spring that might have led to the low sea-ice extent in July of 2020. In their analysis the authors focus on the transport and convergence of moist and warm air masses and associated changes in the surface energy balance. Using a cyclone tracking algorithm, they connect the increased energy transport in spring 2020 to anomalies in the cyclone activity. Thereby, the study follows up on a range of previous studies, which identified the spring atmospheric conditions to be the major driver of a low summer sea-ice extent. While the topic is very relevant and interesting, the analysis lacks explanations and potentially also extensions.

General comments:

The analysis is rather comprehensive but the methods and supporting information are not always clear, hence, it is hard to arrive at the drawn conclusions. One of the main problems is that the study area contains a lot of land points, but the focus of interest is sea-ice variability. Why did the authors choose this study area and did not e.g. exclude land points or even focus on the area that showed the largest SIE anomalies in 2020 from Fig. 1.? Another point is the cyclone detection and conclusions drawn. It is not clear how robust the results are.

Response:

a. First of all, thanks a lot for the advice on this manuscript which helps us to improve the research. In the present study, we use a range of latitudes and longitudes to define a relatively regular study area (60° E-165° E, 70° N-82° N) for the convenience of plotting. However, in the analysis, the retrieved values are averaged over the oceanic grids by applying a land mask and SIE is defined as areas that have an ice concentration of at least 15% (Fig. 6 and 7).

b. The nature that extratropical cyclones are characterized by great complexity (asymmetric structure, differ rather more in size, multi-centers and occur in very diverse synoptic situations) indicate that there is no single commonly agreed upon scientific definition of an extratropical cyclone, and there exists a range of ideas and concepts regarding how to identify and track them (Murray and Simmonds, 1991; Serreze et al., 1993; Serreze, 1994; Sinclair, 1994; Pinto et al., 2005; Wang et al., 2006; Wernli and Schwierz, 2006). Methods differ in a number of aspects including variables used, tracking parameters, and post-processing. Different approaches each have their strengths and weaknesses, hence one cannot "judge" the algorithms to be "right" or "incorrect" (Neu et al., 2013). In the present study, we use a revised automatic cyclone identification and tracking algorithm developed originally by Serreze et al. (1993) to diagnose and track the cyclones from the 6-hourly mean sea level pressure (MSLP) data (Serreze et al., 1993; Serreze, 1995; Serreze et al., 1997; Wu et al., 2006a; Wang et al., 2013). The method was used by the National Oceanic and Atmospheric Administration–Cooperative Institute for Research in Environmental Sciences (NOAA–CIRES) Climate Diagnostics Center (CDC) to diagnose storm tracks for the period 1948 and 2004. Besides, Neu et al. (2013) conducted an intercomparison experiment involving 15 commonly used detection and tracking algorithms for extratropical cyclones. The results revealed that cyclone characteristics that are robust between different schemes and our algorithm agrees well with the others in terms of spatial distribution, interannual variability, and geographical linear patterns of the cyclones. To some extent, these facts give credence to the method utilized in this study.

Specific comments:

1) Figure 1: It is not possible to see the colored line indicating the July SIE of 2021 (red) and not possible to distinguish between the others (green, gray). Please choose different colors or a thicker linewidth, as this figure is important for the following analysis.

Response: Fig. 1 has been modified to aid the interpretation. We plot spatial patterns of SIC anomalies and the SIEs in different panels instead of superimposing the contour lines onto the shading. Besides, the study area is also outlined in Fig.1 (Fig. 1 in the revised manuscript).

2) Section 2.2.3: Crawford et al, 2021 (https://doi.org/10.1175/mwr-d-20-0417.1) have investigated the dependence of spatial and temporal resolution on a realistic detection of cyclone

tracks in ERA-5. How does your algorithm differ from theirs? Do you experience an unrealistic break up of cyclones? The cyclone tracks in Fig. 9 are hard to identify and many end up over land (while you are interested in what happens over the ice), which makes me wonder how robust your whole analysis on the cyclone tracks is. Maybe backwards trajectories would be easier to interpret?

Response:

a. Crawford et al. (2021) aimed to test the sensitivity of the cyclone tracking method to the spatial and temporal resolution of ERA5 sea level pressure fields. The cyclone detection and tracking algorithm they used was introduced by Crawford and Serreze (2016) and builds on the method originally designed by Serreze et al. (1993). Coincidentally, we use a revised automatic cyclone identification and tracking algorithm which is based on the same scheme developed by Serreze et al. (1993). The main difference is that they explicitly identify multicenter cyclones (a single cyclone may contain two or more distinct but closely related minima in SLP) as well as splitting and merging events. While in our algorithm, the exact center is determined as the grid with the largest local Laplacian of SLP when multiple cyclone center candidates are found within a radius of 600 km.

b. In the present study, the ERA5 6-hour SLP fields were interpolated to a 50-km version of the NSIDC EASE-grid, prior to the application of the algorithm. As suggested by Crawford et al. (2021), we used a common search distance (7×7 array of grid points) when detecting minima in sea level pressure to avoid unrealistic break up of cyclones.

c. Some studies corroborated the fact that synoptic cyclones play a crucial role in regulating the poleward moisture and energy fluxes (Jakobson and Vihma, 2010; Dufour et al., 2016; Villamil-Otero et al., 2018). As a cyclone represents a dynamical process and concerning its fundamental nature in holding moisture and energy, all poleward cyclones may play a non-negligible role in transporting energy and water vapor to the Arctic in the form of a relay. Thus, rather than just confining cyclones that occurred over the ice, we take all northward cyclones into account when inspecting the underlying effects of the cyclones on the meridional transport of energy/moisture in spring 2020. As a consequence, some cyclones in Fig.9 had their tracks end over land. Tracking cyclones backwards from their lysis to form trajectories may be more straightforward in this study. However, to our knowledge, there exist no backward-tracking schemes of an extratropical cyclone. It can be an innovative path for future research.

3) Line 157: A low-pressure anomaly over the central Arctic dominates the spring of 2020. Similar anomalies were detected in spring of years with a low summer sea ice in Kapsch et al., 2019 (https://doi.org/10.1007/s00382-018-4279-z) and Horvath et al., 2021 (https://doi.org/10.1007/s00382-021-05776-y). Both of the studies pointed out that a similar pattern was associated with summers of low sea ice and an early melt onset in the Kara/East Siberian Sea. You should relate to these studies, as your findings for 2020 are a confirmation their findings.

Response: We have read the recommended literature and related these two studies in our study when discussing the atmospheric pattern in spring 2020 (Line 182-185 in the revised manuscript).

4) Fig. 6: the total convergence of energy is heavily smoothed. Why using a different temporal resolution for the different variables? Please clarify. A higher spatial resolution can also give an idea about the persistence of atmospheric circulation patterns that lead to the enhanced energy transport, which was found to be of importance for the summer sea ice in previous studies.

Response: We have no idea why the hourly REA5 convergence of total energy flux fields are rather noisy (Figure below, grey dashed line), the reasoning behind it demands further evaluations. Hence it is a compromise to utilize the monthly mean of the convergence fields.

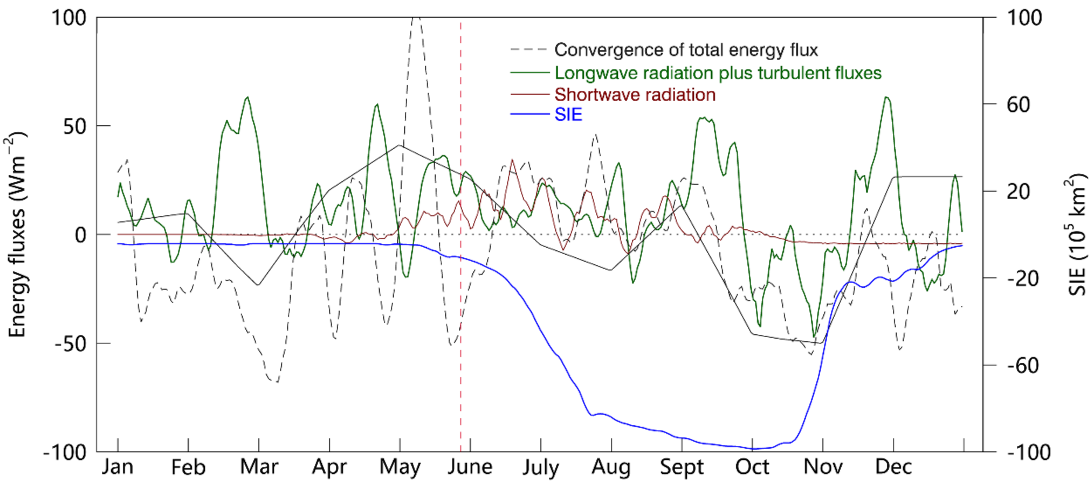


Figure 6. Time series of SIE, the anomalies of atmospheric energy transport convergence and surface energy

fluxes over the study area (indicated by the green polygon in Fig. 3c and d) during 2020. The blue curve represents the SIE. The red line denotes the anomalies of net solar radiation. The green line corresponds to the anomalies of the sum of the downwelling thermal radiation and the turbulent (latent plus sensible) flux. The vertical pink line denotes the average melt day (May 28) in 2020, provided by NASA. The anomalies are relative to the climatology of the years 1979-2020.

5) Line 46: '… various disciplines.' – like which? Line 87: Schweiger et al outlined a less than 0.1m difference and a high pattern correlation. How different are the data sets over the area of interest?

Response:

a. The scientific studies about the causes of Arctic sea ice shrinkage encompass various disciplines, including atmospheric (Deser et al., 2000; Wu et al., 2006b; Wang et al., 2009; Ogi et al., 2016) and oceanic (Årthun et al., 2012; Miles et al., 2014; Zhang, 2015; Årthun and Eldevik, 2016) sciences. This paper aimed to assess the impact of the variations in atmospheric transport of total energy and moisture on sea ice loss in spring 2020, the second paragraph in the introduction, therefore, discusses the current understanding of relevant mechanisms (Line 38-54 in the revised manuscript).

b. In the Laptev and East Siberian Seas, ICESat and PIOMAS ice thickness fields show a close agreement with the spatial pattern of ice thickness. The PIOMAS thickness fields are about 0.2-0.7 m small than that of ICESat for February–March (Schweiger et al., 2011). The description of the PIOMAS fields in the study area has been added in the revised version (Line 104-106 in the revised manuscript).

6) Line 119: You claim that the results of your energy flux estimates are similar to those of ERA-5. If the moisture flux exists in ERA-5, why estimating it?

Response: Indeed, the ERA5 fields of the total energy and moisture flux experienced an update and some corrections during the period we processed the data. To ensure that our research continues, we calculate the moisture flux when the corresponding field from ERA5 was in an upgrade state. Once the ERA5 field is prepared, we compared our estimated results with ERA5 and decided to directly utilize the ERA5 fields of the total energy and moisture flux.

7) Fig. 5: Might be an optical illusion due to the projection, but for me it seems that the study area slightly differs from the one indicated in Fig. 3. It seems that there are more land points in Fig. 5.

However, see comment on excluding land points from the analysis.

Response: All the fields obtained from ERA5 have a spatial resolution of 1.0° ×1.0° in longitude and latitude. For better illustration of the Arctic region, the anomalies of different meteorological variables as well as cyclone characteristics are displayed on a polar stereographic projection (Figs. 1, 2, 3, 5, 7, 10, and 11). The spatial patterns of variations of surface radiative and turbulent fluxes shown in Fig.5 are not the optical illusion but indeed a true phenomenon exists in the data. The figure attached below shows the same anomalies, which have coincident patterns with Fig.5. Besides, the plotted view of Fig.5 is slightly larger than the study area in order to display (has been clarified in the caption of Fig.5). In the analysis, the retrieved values are averaged over the oceanic grids within the study area by applying a land mask and SIE is defined as areas that have an ice concentration of at least 15% (Fig. 6 and 7).

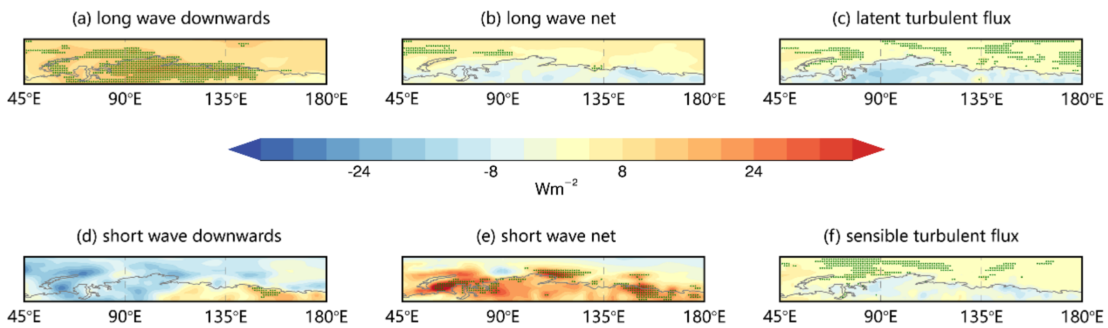


Figure 5. Anomalies of surface (a) downwelling and (b) net longwave radiation, (d) downwelling and (e) net shortwave radiation, as well as sensible (c) and latent (f) heat fluxes. The anomalies are relative to the climatology with monthly resolution from the years 1979-2020 and averaged over the spring months (April–June) of 2020. The stippled grids denote those with values where the anomaly exceeds 2 standard deviations.

8) Line 284: I don't see how calculating the cyclones from ERA-Interim gives more credibility in the methods and results. It might be worse to take a more independent reanalysis for such credibility check. Again, a discussion on the method and previous findings is necessary (see point Section 2.2.3).

Response: We sincerely appreciate the valuable comments on the credence check of the cyclone identifying and tracking algorithm. In a previous study of our team, the retrieved cyclones from ERA-Interim SLP fields are used to discuss the impact of cyclones on the sea ice area flux through the Baffin Bay with respect to dynamical processes (Liang et al., 2021). For convenience, we compared the cyclone systems diagnosed from the ERA5 SLP with those from ERA-Interim, which may be problematic. We have deleted these sentences in the revised manuscript and referred to the results of the intercomparison between 15 different algorithms in Neu et al. (2013) (Line 322-326 in the revised manuscript).

9) Line 361: Ice motion in response to the circulation patterns and cyclones should be discussed a bit more in detail, as it is an important process. It also should be related to previous studies. There have also been other studies, elaborating on some of the processes that lead to an earlier melt onset (e.g. increased liquid precipitation).

Response: The main driver of sea ice motion is the surface wind, which can explain more than 70% of the variance of the ice velocity in the central Arctic Ocean. Sea ice tends to move with a speed of about 2% of the surface wind and about 45° to the right of the wind (Thorndike and Colony, 1982). That is to say, variations in large-scale circulation patterns and cyclones would inevitably change the ice drift pattern. We discussed the ice motion and other dynamical processes in more detail in the revised paper and add the related references (Line 420-436 in the revised manuscript). Besides, the literature elaborating the consistent mechanisms with the present study which lead to an earlier melt onset has added to the discussion part of Fig.6 (Line 282-285 in the revised manuscript).

10) Line 415: A very relevant study related to an early melt onset in years of low summer sea ice in the study area is also Mortin et al., 2016 (https://doi.org/10.1002/2016GL069330) as well as several studies by Stroeve et al.

Response: We have read the recommended literature and referred to them when discussing Fig.6 in our study (Line 282-285 in the revised manuscript).

11) Line 427: It should be mentioned much earlier, probably in the introduction, that the September SIE was not a record in 2021. It might be interesting for the reader to know why this study explores the July SIE instead of the September SIE.

Response: We clarified the fact that the September SIE of 2021 did not hit the record earlier in the introduction (Line 55-56 in the manuscript). In 2020 however, Arctic sea ice experienced the lowest July extent recorded since 1979, which is ~21% lower than the average July SIE over the period 2000-2020. This study aims to disentangle the mechanisms that drive this extreme event. For extension, we added a paragraph to discuss the connection between July and September Arctic sea ice extent in 2020 (Line 480-493 in the revised manuscript).

Technical corrections:

1) Line 102: ‘replacing ERA-Interim’

Response: We replaced the sentence with “ERA5 represents a new reanalysis product which improves on its predecessor (ERA-Interim). It benefits from a decade of developments in model physics, core dynamics, and data assimilation.” (Line 119-120 in the revised manuscript).

2) Line 175: remove parenthesis behind Kara Sea.

Response: Revised as suggested.

3) Line 188: ‘unusual conditions with higher’

Response: Revised as suggested (Line 213 in the revised manuscript).

4) Fig. 1, 2, 3, 7, 10, 11: It might be worse to use one projection (including latitude range) for the plots.

Response: As the regions of interest in this study are located in the Arctic, I think it is better to plot the geographical patterns of different variables on a polar stereographic projection. Moreover, all figures that demonstrate spatial distributions have the same projection for the sake of uniformity of presentation.

5) Fig. 4: Line 200: ‘spanning the with significant’ – something missing here. The whole caption would benefit from a revision.

Response: The expression has been changed into “spanning the regions with significant” (Line 226 in the revised manuscript).

6) Line 282: ‘we identify and track cyclone’

Response: Corrected as suggested (Line 320 in the revised manuscript).

7) In many places there is no space between text and the following parentheses, e.g. Line 205, 256, 279, 283, 398, 419, 421 … In general, it would be good to check for spelling and language related issues.

Response: Following the suggestion, the space between text and the following parentheses have been added. We also further polished the revised manuscript and remove the inappropriate expressions.

Response to Referee #2

Review of the Manuscript ‘Warm and moist atmospheric flow caused a record minimum July sea ice extent of the Arctic in 2020‘ by Ling et al. submitted to The Cryosphere.

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.

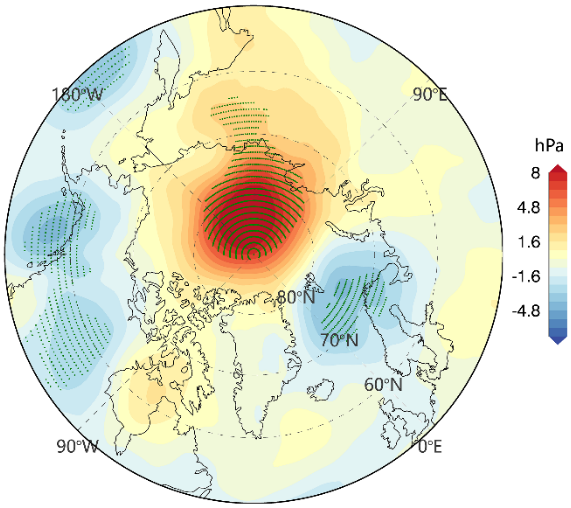
Especially, I suggest that the authors discuss more thoroughly the current understanding of oceanic and atmospheric drivers of summer sea ice melt, especially the physical mechanisms, as their objective in this paper is to reveal the underlying mechanisms leading to the record melt in July 2020. For 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.

Response: We sincerely appreciate the constructive and detailed comments by Referee #2. These comments helped us improve our manuscript, and provided important guidance for our future research.

a. The Introduction has been reorganized as suggested. Before analyzing the minimum sea ice extent in July 2020, we discussed thoroughly the current understanding of atmospheric drivers of sea ice melt, especially the relevant physical mechanisms and refer to the previous studies. Then we present the extreme event of sea ice loss in July 2020, and the scientific question of the present research is set up afterward (Line 29-77 in the revised manuscript).

b. Although the September SIE of 2020 did not shatter the previous lows to be a new record, September 2020 had the second-lowest SIE since 1979, stood at 3.82×10^{6} km^2, which is merely 12% higher than the lowest SIE. A prominent high-pressure anomaly dominated the Arctic in July-September 2020 (especially in July-August, the figure below). Previous studies elaborated that the recent summertime sea ice depletion is broadly associated with the anticyclonic atmospheric circulation pattern which can increase the downwelling longwave radiation above the ice by warming and moistening the lower troposphere (Ogi and Wallace, 2012; Ding et al., 2017). The combination of low-pressure anomaly persistent in April-June (favoring moisture and energy inflow) and anticyclonic atmospheric circulation pattern (leading to adiabatic warming) may contribute to the particularly low SIE of September 2020, the mechanisms of which would be the potential candidates for future studies. The present study is dedicated to elucidating that anomalous high inflow of total energy and moisture from lower latitudes to the Arctic in spring caused severe sea ice loss of July 2020. The above arguments about the anticyclonic atmospheric circulation pattern, therefore, have been added to the Discussion and Conclusions part when mentioning the September SIE of 2020 (Line 480-493 in the revised manuscript).



Supplementary Figure. Spatial patterns of sea level pressure anomalies (shading) during July to August 2020. The anomalies are computed as the difference between the averaged fields of the three months (April-June) and the corresponding climatology over the past four decades (1979-2020). Stipplings represent the values where the anomaly exceeds 1.5 standard deviations.

c. Several existing literature pointed out similar mechanisms with ours (Graversen et al., 2011; Vázquez et al., 2017; Kapsch et al., 2019; Horvath et al., 2021). Our results serve to augment more evidence to the mechanisms that drive sea ice loss through transporting moisture and energy into the Arctic via a case study in July 2020. Here we argued that the unusual atmospheric energy and moisture transport favored by large-scale circulation and cyclones in Spring 2020 effectively reduce ice extent under the circumstance of more thin, first-year ice, which is a novel result. These points distinguishing our research from previous literature has been declared in the revised version (Line 473-475 in the revised manuscript).

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.

Response: Indeed, sea ice extent (SIE) and sea ice concentration (SIC) are two similar parameters describing the areal coverage of sea ice, while the former denotes the “boundary” and the latter represents the “spatial fraction”. As suggested, we use SIE in the Introduction. SIC is used only once when showing the spatial pattern of sea ice cover anomalies to detect the regions where severe ice loss occurred in July 2020 (Fig. 1.). Besides, Fig. 1 has been modified to aid the interpretation following the suggestion. We plot spatial patterns of SIC anomalies and the SIEs in different panels instead of superimposing the contour lines onto the shading.

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

Response: We added the full names of the abbreviations in the figure captions.

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.

Response: Thanks for the insightful comments on the significance test. The significance of the figures and results is an essential issue when drawing a conclusion. Following the suggestion, we have added striplings to denote anomalies that are significant (e.g. greater than two standard deviations) in Fig 2, Fig. 4, and Fig.5. Accordingly, we polished the discussion part of these figures (Fig 2, Fig. 4, and Fig.5) for better clarification. The significance of the results shown in Fig.6, including the magnitudes of different anomalies have been stated literally in the paragraph of its analysis (Line 266-285 in the revised manuscript). Besides, we produce a bar plot of all 42 years' early melt date to distinguish the particularly early melt onset in 2020. The revised figures are replaced in the revised version.

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

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

Response: The decreasing trend detected in the averaged sea ice thickness of the study area in spring during the period 1979-2020 is significant at the 99% confidence level, using a Student's t-test. The significance testing method has been clarified in the revised paper clarified (Line 304 in the revised manuscript).

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)?

Response: By processing the data, we found that the low-frequency variations in the 10-yr trends are controlled by the large-scale circulation trends. A more thorough discussion of Fig.10a has been presented (Line 383-390 in the revised manuscript). However, this question indeed requires more comprehensive analysis and should be explored in greater detail in future studies.

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