## Response to Editor

Dear Editor David Schroeder：

Thank you very much for handling and review of our manuscript. In the latest revised manuscript, we have addressed and answered all the comments and questions raised by the reviewers. Besides, we checked the manuscript carefully for typos and refined some of the descriptions

The revisions associated with the method, analysis, and figures are summarized as follows:

1) Method: Utilizing composite analysis, we conduct maps of the top 5 years with the lowest SIE in the study area and those 5 years with the highest SIE. Based on the result, we address the concern of Referee #2 about the similarities of the atmospheric conditions between spring 2020 and that of previous studies. We also explain the source and deviation of the sea-ice growth model as suggested by Referee #4.

2) Analyses: We have revised the discussion according to the composite analysis. Moreover, we added discussion about the moistening and warming that occurred over land, as well as their potential impacts on the Arctic Ocean system, to address the comments raised by Referee #3. The errors about measurements for clouds are also clarified.

3) Figures: We have replotted figures using a more commonly used projection with central longitude 0º. Several details of the figures and the captions have been also improved.

Please find below our point-by-point response (blue text) to the comments (black text) by the reviewers.

Best regards

Yu Liang and co-authors

## Response to Referee #2

### Summary

Liang et al. addressed my concerns that I have raised in the first round of revision of their manuscript, which I appreciate. However, based on my careful examination of the authors response to Reviewer#1 and the revised manuscript I still have concerns, which need clarifications before I can recommend the paper for publication.

### Major concerns

R.1. Reviewer #1 pointed out two seemingly relevant papers (Kapsch et al. 2019 and Horvath et al. 2021), however, to me, either of the papers show similar geopotential height/SLP structure as the authors of the present manuscript (their Fig. 1 SLP pattern). Although both papers suggest that low-pressure anomaly in the central Arctic in spring may favor anomalous melt onset, either of the SOM nodes show the particular case in 2020 SLP, as shown in Fig.1 of Liang et al. Some similarities can be recognized looking at node 18 of Fig 3 in Horvath et al. 2021, but similarities solely in the central Arctic with the lower latitudes showing very different SLP structures, which can result in different moisture/energy transports too.

While one single year's seasonal mean (2020 April-May-June) SLP may not be expected to perfectly match any of the nodes in the SOM using daily data, the authors should be more specific in line 182-183 when stating that their results are 'similar' to the SOM analysis of either Kapsch et al. 2019 or Horvath et al. 2021.

Thus, the authors should notice and discuss the potential causes of these differences, which probably can be related to methodological differences. Namely, the authors of the present manuscript use simple anomalies of seasonal mean 2020 spring/early summer. I wonder if the authors will find similar AMJ atmospheric patterns by using a melt-onset-date-based composite of SLP and moisture/energy transport spatial patterns in the Arctic during 1979-2019. Ending with 2019, because adding 2020 to the composite would alter the results, I am curious of. Based on 41 years of data, maybe the difference between those 6 years (approx. 1.5 std dev) with the earliest and those 6 with the latest melt onset (e.g., averaged over the study area) will show similar SLP and moisture/energy transports to the ones seen in 2020 spring/early summer (this similarity may be quantified using spatial correlation or the significance test of the composite maps based on a two-sample t test).

Response:

Thanks a lot for the detailed advice on this manuscript. Following the suggestion, we constructed the composite maps of the top 5 years with the lowest SIE in the study area and those 5 years with the highest SIE. The difference of the SLP fields, moisture flux, and total energy flux between them are shown in the figure below (Fig. 1 in the response letter). Note that difference maps of melt-onset-date-based composite share similar patterns. Compared to years with higher SIE in the study area, there exists a low-pressure anomaly centered over the study area which extended southwards from the Barents-Kara Seas to the middle part of Eurasia when SIE is low. This low-pressure center is reflected in the SLP anomalies pattern in spring 2020 with a broader extent covering the central Arctic. These kinds of SLP modes favor anomalous high moisture and energy air from Eurasia into the Arctic through the Kara Sea and the Laptev Sea, which agree well with the great transport of moist and warm air mass through the entry. However, in the difference map, the moisture and energy flux partly traveled northward crossing the pole while in spring 2020 the fluxes prevailed in the study area. The discrepancy can be attributed to the significant high-pressure level center with a broad extent in Siberia and the low-pressure center near the pole, indicating that spring 2020 had an unusual atmospheric condition that drove an exceptional form of moisture and energy transport. We carefully checked the SLP pattern of Spring (April to June) 2020 with those SOM nodes in Kapsch et al. (2019) and Horvath et al. (2021). Similarities can be detected between node 18 of Fig 3 in Horvath et al. (2021), with a slight difference in the Bering Sea and the Labrador Sea. **Based on the composite analysis, it seems that the difference between atmospheric circulation in 2020 with those SOM nodes is not sensitive to the method used. It underscores the unusual atmospheric condition in spring 2020.** We revised the statement when mentioning the similarities in the revised manuscript (Line 188-192, 200 in the revised manuscript).

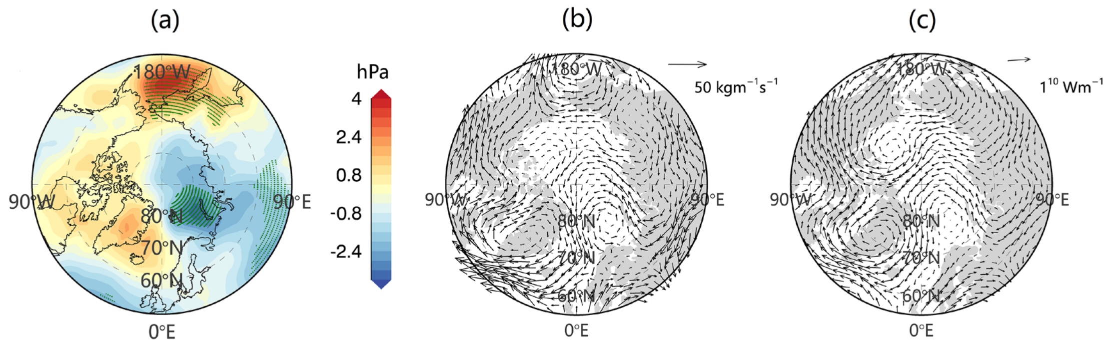


**Figure 1.** Difference maps of (a) SLP fields, (b) moisture flux, and (c) total energy flux between 5 years with the lowest SIE in the study area and those 5 highest years with the SIE. Stipplings represent the grids with the difference significant at the 90% confidence level (two-sample t-test).

I suggest that it would be safer to say even in the title, that "Warm and moist atmospheric flow" not caused rather either contributed to or preceded "a record minimum July sea ice extent of the Arctic in 2020", because as the authors also mention in the discussion, the high-pressure in July 2020 also contributed to sea ice melt, so in my view, conditions well aligned for setting a new record minimum in July sea ice. This is not reflected in the current last sentence of the Abstract, while the authors acknowledge it in the discussion (line 485-487).

Response: Thanks, we used a new title "Contribution of warm and moist atmospheric flow to a record minimum July sea ice extent of the Arctic in 2020" for this paper as other factors such as the high-pressure in July 2020 contributed to the minimum SIE as well. Besides, in the last sentence of the Abstract, the role of the anticyclonic atmospheric circulation pattern has been reflected by the expression "large-scale atmospheric circulation" (low-pressure and high pressure).

Furthermore, the low-pressure anomaly in the central Arctic is insignificant compared with the climatology and the high-pressure anomaly over Siberia is more anomalous, thus might have played a larger role than the low pressure in the central Arctic in setting the stage for July extreme melt; in light of the composites, it will be easier to decide. In lines 176-180 please clarify, which centers are statistically significant, not all of those that are currently mentioned in the text. In the discussion also, line 461 needs to be refined, probably in accordance with the suggested composite analysis.

Response: Indeed, the low-pressure anomaly in the central Arctic exceeds one standard deviation, while the high-pressure anomaly over Siberia and the low-pressure region in the Barents Sea have magnitudes larger than 1.5 standard deviations. Based on the analysis, the low-pressure center in the central Arctic, albeit with a smaller magnitude, also plays an important role in the sea ice melt in the study region. That is to say, it acted together with the high-pressure anomaly over Siberia to generate strong winds in the study area. These winds divert the moisture and energy fluxes to prevail in the Laptev and East Siberian Seas after they entered the Arctic. To show the magnitude of the low-pressure anomaly in the central Arctic, we have revised show the values where anomalies exceed one standard deviation in Fig.2 in the manuscript. However, in the difference map (Fig. 1 in the response letter), the high-pressure anomaly over Siberia is not discernible and the low-pressure center near the pole is not conspicuous, which again highlighted the unique atmospheric condition in spring 2020. The related description has been clarified in the revised manuscript (Line 188-192, 200 in the revised manuscript).

R.2. I am unsure what the authors mean by 'average melt days' in the caption of Fig. 6. Based on the text, it should refer to that the authors averaged the melt onset days in all grid points corresponding to the study area in a given year. If so, please clarify the figure caption, e.g., instead of 'the averaged melt date of the study area' say 'day of the year corresponding to the melt onset in each year averaged over the study area'. Is the melt onset in 2020 outside 1.5 standard deviation from the mean of all melt onset dates?

Response: Yes, Fig. 6b shows the melt onset date in each year averaged over the study area during the period 1979-2020. The caption of Fig. 6 has been accordingly. Yes, the melt onset in 2020 lies outside 1.5 standard deviations from the mean averaged over the study area.

**Minor concerns**

R.3. Figure 3 lacks the hatching indicating statistically significant grid points.

Response: Modified accordingly, we have added dots to denote anomalies that exceed two standard deviations (Fig. 3 in the revised manuscript).

R.4. The authors may consider replotting their figures with using a more commonly used projection with central longitude 0º instead of the currently used ones centered over Greenland. I think this would make the figures easier to read and compare with other papers on similar topics.

Response: Modified as suggested.

## Response to Referee #3

**Summary:**

Liang et al. examine the processes that may have contributed to the extreme Arctic sea ice loss event in the summer of 2020. Analysis using ERA5 and PIOMAS products shows enhanced poleward moisture and energy transport likely generated increased downwelling surface radiative fluxes that melted the ice. The study then links this meridional transport to increased cyclone activity and intensity during spring of that year.

**General comments:**

When considering preconditioning of the ice in spring prior to the melt season, several studies have made linkages to poleward energy and moisture transport, but it is interesting to see this applied to 2020, which may be an important year for understanding extreme sea ice loss events. The discussion that places 2020 in the context of the last 4 decades, for example in Figure 6 and 10, is particularly interesting. The goals, methods, and logic are overall valid and suitable. The presentation of the results and conclusions could use some minor revision to improve the clarity and to help tie everything together.

It seems that good improvements were made in the previous round of revisions. Therefore, I only have recommendations for minor revisions detailed below. These comments are mainly about clarifying certain aspects of the figures or changing the figures to make it easier to follow the connections between them. The main comment is that the interpretation of Figure 4 may be overstated and the counterargument using evaporation might not be sufficient since much of the strongest warming and moistening may also occur over land or at the land-sea interface.

Response: Thank you for your general comment. We believe that the interpretation of Figure 4 is not overstated. The same vertical profile as Fig. 4 in the manuscript but excluding the land grids by using an ocean mask is shown in the figure below. Note that the latitude 65-66° N has no ocean grids thus were labeled as no data. Firstly, significantly elevated temperature and higher moisture content distributed widely from 67° N to 80° N over the ocean and extend from the surface into the mid-troposphere. That is to say, even though strong warming and moistening also occur over land or coast, there exist unusual conditions with higher moisture content and warming within the Arctic atmospheric column over the ice cover region. Then we clarified the source of this extra water vapor (local evaporation or transport from lower latitudes). The below normal evaporation indicates that the enhanced moisture contributing to the moister atmosphere is primarily provided by atmospheric transport from remote areas. Secondly, warming and moistening over land could also contribute to ice melting through several mechanisms, especially for the heat input by river runoff. We have added some discussions on this issue and more explanations about Fig. 4 in Section 3 (Line 221-228 in the revised manuscript).

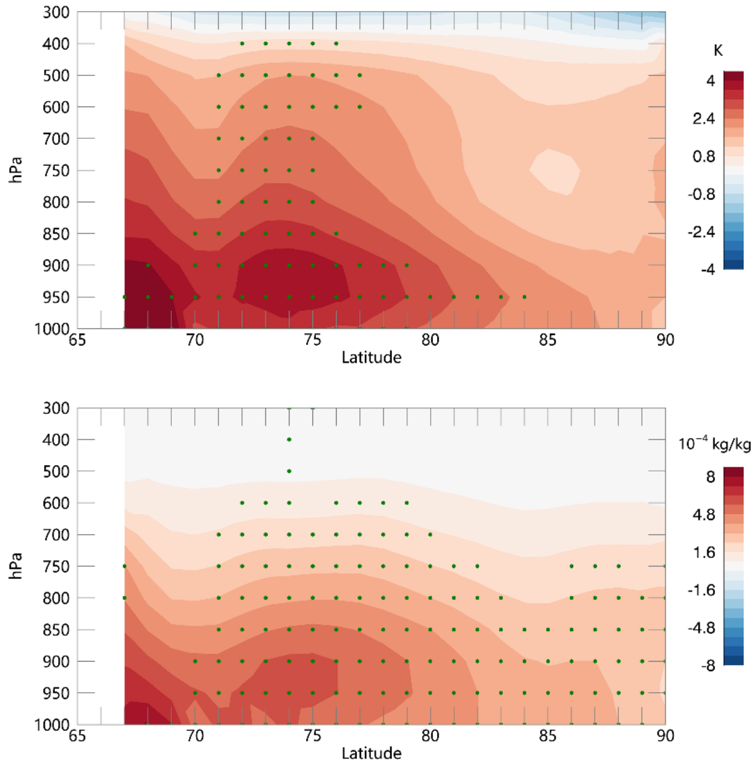


**Figure 2.** Same as Fig.4 in the manuscript, with data retrieved over oceanic grids.

**Specific comments:**

Line 210-211: Is this reference for Figure 4?

Response: No, this sentence mentioned the great convergence of energy and moisture, which refers to Fig. 3c and d.

Lines 213-222: The centers of the strongest warming and moistening in Figure 4 not only coincided with the regions of strong sea ice loss but over land, too. Many of the fluxes over land in Figure 5 are opposite to those over the ocean and their effect on the temperature and specific humidity profiles may be different as well. Are there substantial changes in the anomaly patterns after detrending?

Response: As stated above, strong warming and moistening events occurred over both land and the ocean. They could contribute to the sea ice loss in the study area in July 2020. The intrusion of moisture and energy leads to warming and damping of the atmosphere in the spring months hence changing its radiative characteristics. The net longwave radiation and turbulent fluxes show opposite signs over some parts of the land and the ocean. Yet, enhanced surface fluxes occurred in the study area with considerable ice loss, which is the focus of this study. Additionally, there are no substantial changes in the anomaly patterns after detrending, see Fig. 3 below. The spatial distributions of the anomalies agree well with those related to climatology with little change in the magnitudes.

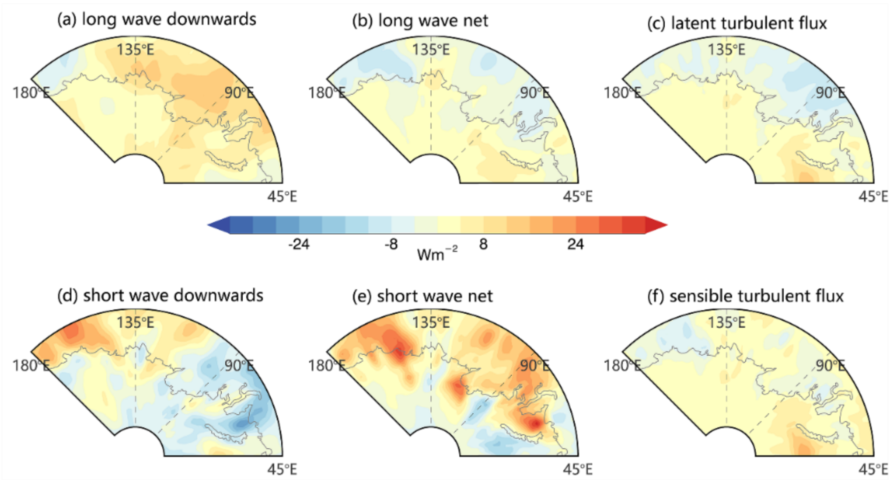


**Figure 3.** Anomalies of surface (a) downwelling and (b) net longwave radiation, (d) downwelling and (e) net shortwave radiation, as well as sensible (c) and latent (f) heat fluxes. The anomalies are relative to the trends with monthly resolution from the years 1979-2020 and averaged over the spring months (April–June) of 2020.

Line 276: "earlier than usual"

Response: Corrected.

Section 4: Measurements for clouds and their radiative effects are difficult to come by in the Arctic and in reanalysis are largely model generated, leading to large uncertainties in radiative fluxes. Some discussion of possible biases would be beneficial.

Response: When evaluating the variations of different surface energy flux components, we referred to the conclusions of previous studies about the effect of cloud cover and the mechanisms. The net amount and the nature of the effect on surface radiations are complicated, depending on the level, height, and seasonality of the cloud. We did not utilize cloud datasets to conduct analysis in the present study, hence we added a few sentences in Section 4 to discuss possible biases of measurements for clouds briefly (Line 276-280 in the revised manuscript).

Figure 6. If correct, please add net to make the figure clearer.

Response: We have added “net” before “shortwave radiation” in the labels of Figure 6.

Line 305-306: “changes in thermodynamic forcing”

Response: Revised as suggested (Line 326 in the revised manuscript).

Figure 7. Could this be separated into the contribution from each component of the surface radiative fluxes?

Response: In terms of numerical estimation alone, the changes of SIT can be separated into the components caused by anomalies in surface net longwave radiation, net shortwave radiation, sensible heat flux, and latent heat flux. The results are depicted in Fig. 4 below. However, these components may interact and feedback each other, and sea ice cannot identify different radiation fluxes. Thus, we prefer to show the changes in ice thickness caused by the overall anomalies of these surface radiative fluxes.

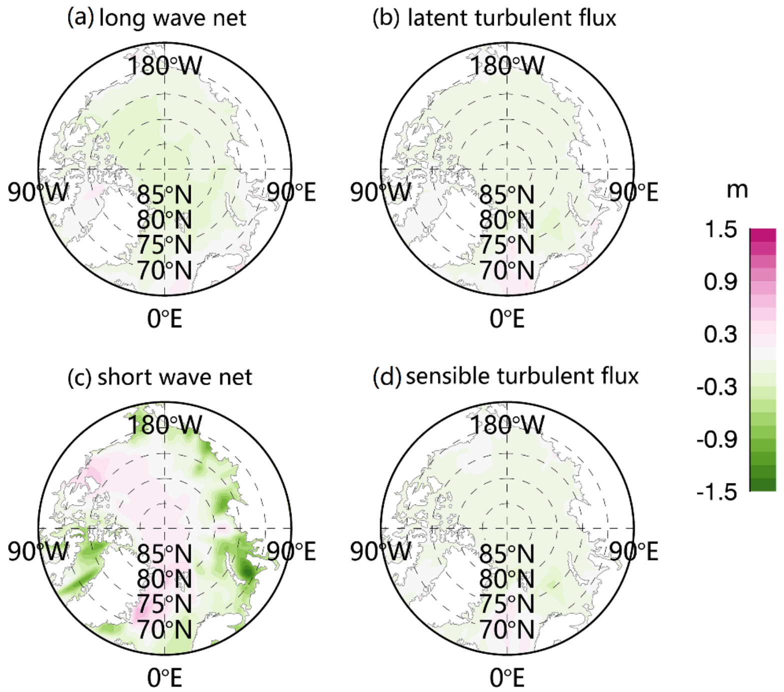


**Figure 4.** Changes of SIT caused by anomalies of surface (a) net longwave radiation, (b) latent heat flux, (c) net shortwave radiation, and (d) sensible heat flux during spring (April-June) 2020 which is estimated by a sea-ice growth model.

Line 315: Looks like the word "moisture" was unfinished.

Response: Corrected as suggested (Line 337 in the revised manuscript).

Figure 8: Could this figure show the fluxes and CAI in 2020 and the trends, like is done in other figures? It seems this could serve the same purpose and also add to the discussion.

Response: We clarify that Fig. 8 represents the longitudinal distributions of the climatological northward total energy and moisture flux across 60° N, and the CAI of cyclones in spring during 1979-2020. Unlike other figures which highlighted the anomalies in 2020 relative to the climatology of the years 1979-2020, Fig. 8 shows the averaged state of the moisture/energy flux and cyclone parameters. It serves to confirm the significant role of cyclone activity in contributing to the poleward advection of energy and moisture. The spatial patterns of anomalies of fluxes and cyclone index are already delineated in Fig. 3 and Fig. 10.

Figure 9: From the caption the description for the blue lines is unclear. Also, how would this look with a polar stereo projection, so that it is easier to compare with Figure 3? This analysis might be

aided by Harrington et al. (2021) where they use isotope tagging to trace sources and transport of moisture into the Arctic.

Response: We have revised the caption to make it clear. Fig.9 in the polar stereo projection is depicted in Fig.5 below. We still would prefer to remain the original version with a geographical projection for detailed visualization of the cyclone tracks around the study area. Additionally, thanks a lot for the recommended literature which broadened our horizons, yet the isotope tagging is far beyond the scope of this study, we may make a try in future research.

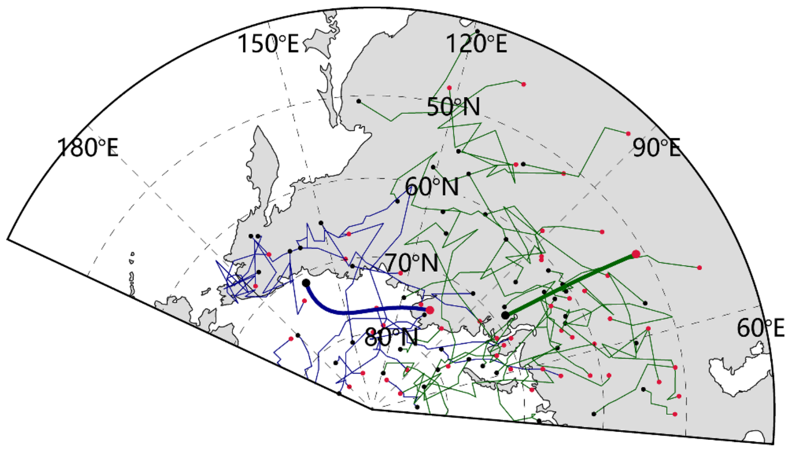


**Figure 5.** Same as Fig.9 in the manuscript, with cyclones and their typical trajectories plotted on a polar stereographic projection.

Figure 10: In the caption, please make note of how the dates on the x-axis correspond with dates in the running window.

Response: As suggested, we declared that the values of the x-axis correspond to the central years of each running window in the caption of Fig. 10 (Line 411 in the revised manuscript).

Lines 421-428: The wording in this paragraph makes it seem like the dynamical effects are very strong then it switches to being unexceptional and it is difficult to follow.

Response: In this paragraph, we aimed to discuss ice motion in response to the circulation patterns and synoptic cyclones more in detail following your suggestions in the first round of review. The focus of this study lies on the thermodynamic mechanisms leading to the noteworthy sea ice surface melt in the study area in July 2020. We evaluated the sea ice outflow through the Fram Strait which possibly contributes to the sea ice loss. However, results showed that the sea ice area flux in spring

2020 was not unexceptional which excludes dynamical transport of sea ice to be the primary contributor to the occurrence of July minimum SIE. We reorganized these sentences to make it clearer (Line 437-444 in the revised manuscript).

Paprtiz et al. (2020) might compliment discussion of poleward transport of air masses into the Arctic and their impacts on the thermodynamically-driven melting and the structure of the Arctic atmosphere.

Response: Papritz (2020) complements the dominating view that warm extremes in the high Arctic are essentially the result of the transport of already warm air masses from midlatitudes. However, they pointed out that poleward transport of already warm air masses contributes around 20% of all extremely warm air masses. Their findings emphasize the predominantly rather high-latitudinal origin of air masses and the relevance of processes including subsidence-induced adiabatic compression, as well as diabatic warming associated with air-sea heat exchanges. We cited this paper, and added some discusstions to fupport our conclusions (Line 45 in the revised manuscript).

## Response to Referee #4

This paper focuses on the record low Arctic sea ice extent in July 2020 and explores the causes of this extreme phenomenon. The authors conclude that the moisture and energy convergence due to the low pressure anomalies occurred in the early summer is responsible for the minimum sea ice extent by regulating local radiation changes. Meanwhile, synoptic cyclones guided by the atmospheric flow further enhance water vapor transport to the Arctic and thus accelerates the sea ice melting.

Overall, this is an article that keeps up with the current changes of the Arctic climate system and has a novel perspective to help us understand the contributors of polar climate change, especially the synoptic factors. However, I do have some concerns about the methods, analysis and so many details still need to be improved. I hope my comments below can help to further improve this manuscript.

**Major comments:**

1.Line 57-60: This is a quite crucial sentence in the whole structure that pinpoints why the authors are concerned about the April-June circulation and the July Arctic sea ice minimum in 2020. So it would be great to show a graph (e.g. a time series of sea ice) to compare the difference in sea ice change between 2020 and 2012, and thus visually highlight the necessity of analyzing sea ice changes during this time period.

Response: We sincerely appreciate the valuable comments on the manuscript. As suggested, we provided a figure to show the time series of daily SIE in 2020, 2021, and the average over 1979-2020. The third paragraph of the Introduction has been revised accordingly (Fig. 1a in the revised manuscript).

2. Line 144-148: What is the original function before neglecting small radiative fluxes? I'm confused by equation (3). It looks very similar to equation (1) of Eisenman et al. (2007), in which turbulent surface fluxes of sensible and latent heat are neglected. Please do explain the source and derivation of the equation (3).

Response: The original function before neglecting small radiative fluxes is written as follows:

$$-\triangle h = \frac{\triangle t}{\rho L}[H_{\downarrow} + LE_{\downarrow} + \varepsilon_w LW_{\downarrow} + (1-\alpha_w)SW_{\downarrow} + F_{l\uparrow} + F_{w\uparrow}] \quad (1)$$

where $\triangle h$ represents sea-ice growth, $\triangle t$ represents the time step, $\rho$ represents the density of sea ice (917 kg/m3), $L$ represents the latent heat of fusion for sea ice (333.4 kJ/kg), $H_{\downarrow}$ represents sensible heat, $LE_{\downarrow}$ represents latent heat, $LW_{\downarrow}$ represents incoming longwave radiation with $\varepsilon_w$ longwave emissivity, $SW_{\downarrow}$ represents incoming shortwave radiation with the surface albedo ($\alpha_w$) depending on the surface characteristics, $F_{l\uparrow}$ represents the heat flux emitted from the surface, and $F_{w\uparrow}$ represents the conductive heat flux at the ice-ocean interface. This function of the sea-ice growth model originally came from Parkinson and Washington (1979), wherein the exact derivation process can be found. This equation is similar to equation (1) in Eisenman et al. (2007). Eisenman et al. (2007) neglect the surface fluxes of sensible and latent heat as they think the turbulent fluxes are much smaller than the radiative components. Nevertheless, in this study, the anomalies of sensible and latent heat fluxes in the study area are not that small (Fig. 5c and f in the revised manuscript). Besides, the focus of the present study is the effect of atmospheric forcing on sea ice, thus the heat flux emitted from the surface (it is a small one during summer) and oceanic heat flux at the ice-ocean interface are neglected. The source of the equation was explained in Section 2.2.2 (Line 140-150 in the revised manuscript).

**Minor comments:**

1.Line 60, 81: July SIE are averaged over two periods of 2000-2020 (Line 60) and 2001-2020 in the figure1 caption (Line 81). Please check and unify it into the same time range in the whole text. A very simple query, why not use the period of satellite data (1979-2020)?

Response: Yes, the time ranges in the original version of the manuscript are not consistent. We aimed to emphasize the severe sea ice cover loss in July 2020 as the Arctic sea ice has been declined since the satellite era, thereby having relatively small SIEs in the recent decades. We have reproduced Fig. 1 to display the average SIE of the whole satellite records (1979-2020) for uniformity and revised the descriptions accordingly (Fig. 1c in the revised manuscript).

2.Line 80:(1) in fig 1a, to the east of the study area there is still a distinct negative SIC signals, would you arrive at different conclusions if the area is extended to 60E-180W, 70-82N? (2) “grey line” in the figure caption indicates blue line in fig 1b? I guess a similar issue related to figure 1 was raised in the first round of review, but it was not seriously revised, so now the figure does not match the caption. Please rewrite the caption of the diagram, stating clearly what is shown in Figures 1a and 1b and what each line corresponds to.

Response:

(1). We arrive at the same conclusions if the area is extended to 60°E-180°E, 70° N -82°N. First of all, from Fig. 3b and d in the manuscript, we see that enhanced transport of total energy prevailed and converged in the region east of the study area where considerable SIC reduction can be observed. There also exist positive anomalies of downwelling longwave radiation, net shortwave radiation, as well as turbulent fluxes in this region (Fig. 5 in the manuscript). Additionally, we plot Fig. 4 and Fig.6 in an extended study area (60° E-180° E, 70° N-82° N), see figures below. The spatial patterns in Fig. 4 have not changed substantially. What's more, the time evolution and relationship shown in Fig.6 still hold when extending to 180E. In the present study, we selected the study area as 60° E-165° E, 70° N-82° N where the most prominent sea ice cover reduction occurred in July 2020.

(2). The mistake in the caption of Fig. 1 has been corrected. We also have replotted Fig.1 following the suggestion and rewritten the caption of Fig.1 to (Line 80 in the revised manuscript).

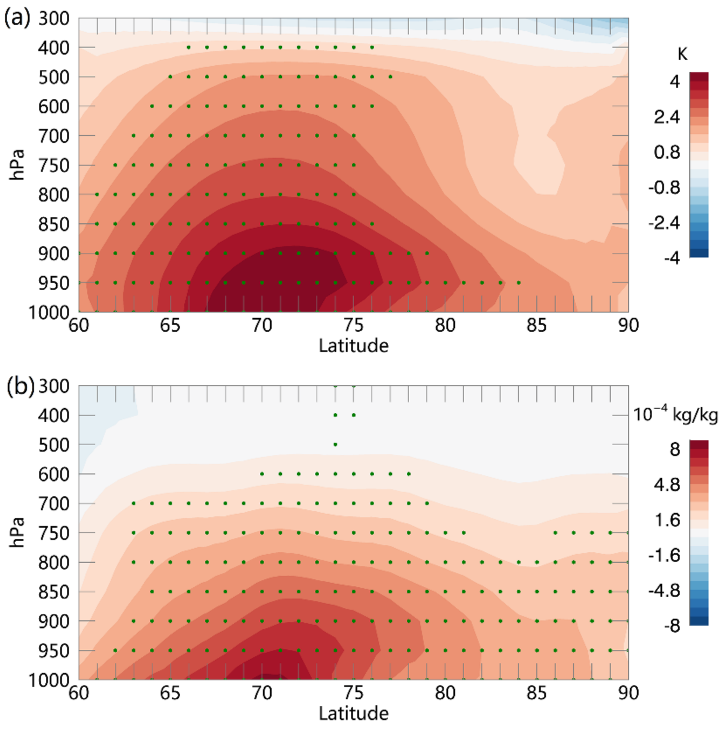


**Figure 6.** Same as Fig.4 in the manuscript, just with an extended study area spanning 60° E-165° E, 60° N-90° N.

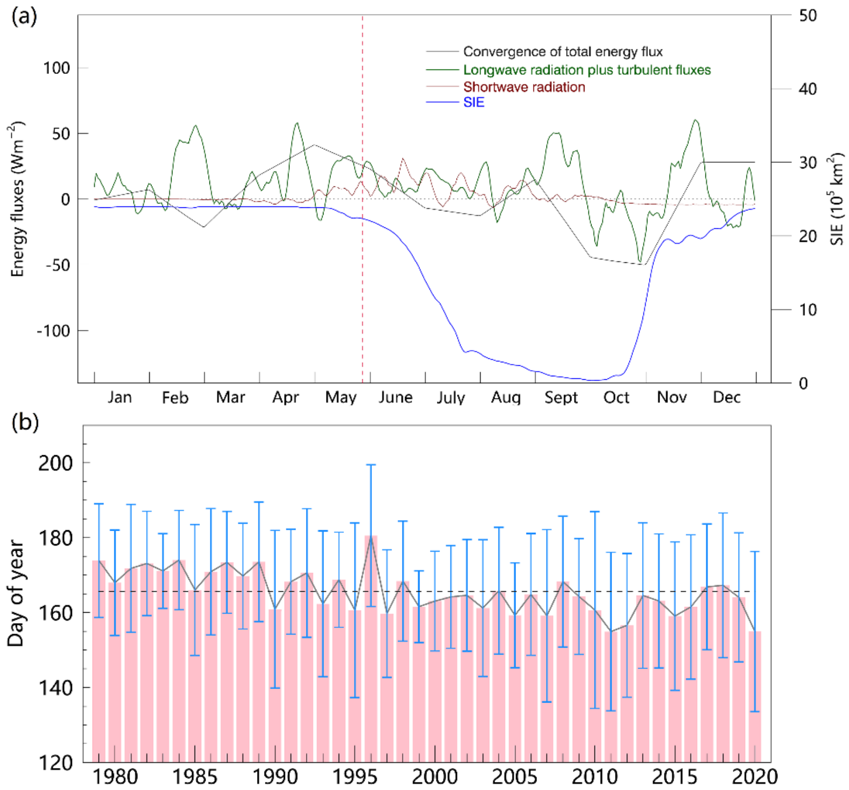


**Figure 7.** Same as Fig.6 in the manuscript, just with an extended study area spanning 60° E-165° E, 60° N-90° N.

3.Line 105-106: “are about 0.2-0.7m small than that of...” change to “are about 0.2-0.7m thinner than that of...”. It would be better to move this sentence to the front part of this paragraph.

Response: Revised as suggested (Line 103 in the revised manuscript). The sentence along with the previous one serves to illustrate the error of sea ice thickness fields from PIOMAS in the study area. We believe that it is more appropriate to introduce the PIOMAS data generally firstly. Therefore, we did not move this sentence to the front part of this paragraph.

4.Line 116: please add ERA5 data citation.

Response: We added the ERA5 data citation as suggested (Line 114 in the revised manuscript).

5.Line 135: “n0 is the lowest pressure level” change to “n0 is the bottom pressure level”.

Response: Revised as suggested (Line 130 in the revised manuscript).

6.Line 195-196: “larger than the 1979-2020 climatology”, please show values of climatology.

Response: We have added values of climatology for the zonal mean of the meridional total energy flux and water vapor flux (Line 204 in the revised manuscript).

7.Line 198-200: please show the calculation range of latitude and longitude of Kara Sea.

Response: Here we merely declare the rough location where the maximal magnitude of the total energy and moisture flux convergence anomaly occurred.

8.Line 245-247: cloud you please check the cloud cover and show the figure?

Response: We checked the cloud cover in 2020 using the fraction of cloud cover on pressure levels from ERA5 as suggested. Fig.8 below demonstrates the anomalies of total cloud cover in the atmospheric column. It can be seen that increased cloud formation in some parts of the study area, especially in the Kara Sea, could lead to a below-normal downward component of the solar radiation (Fig. 5d in the manuscript). Despite there exist discussions about the radiative effects of cloud (Doyle et al., 2011; Serreze and Barry, 2011), measurements for clouds are largely model generated, leading to large uncertainties in reanalysis and in quantifying radiative fluxes. Besides, the net amount and the nature of the effect on surface radiations is complicated, depending on the level, height, and seasonality of the cloud (Gimeno et al., 2019). Thereby, the analysis of cloud cover requires more cautions and details, we consider referring to the existing results rather than putting the cloud cover anomalies figure in the manuscript (Line 257 in the revised manuscript).

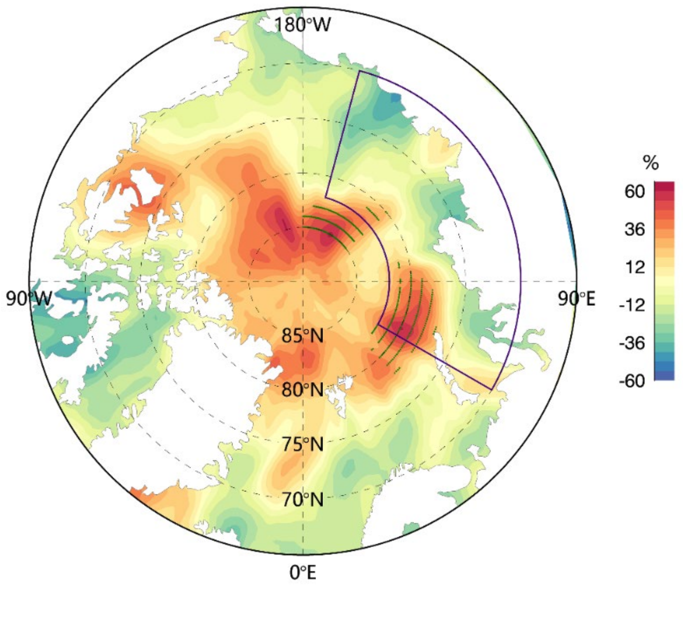


**Figure 8.** Spatial patterns of total cloud fraction anomalies in the atmospheric column from April to June 2020. The anomalies are computed as the difference between the averaged fields of the three months (April-June) and the

corresponding climatology over the past four decades (1979-2020). Stipplings represent the values where the anomaly exceeds 1.5 standard deviation

9.Line 286: in figure 6a, Is there a large difference between the two time series of longwave radiation and longwave radiation plus turbulence? How about their correlation coefficients with convergence of total energy flux/sea ice? In figure 6b, it would be better to draw Y axis starting from 150 days and change the axis string from "Day of year" to specific month.

Response: No large difference exists between the two time series of longwave radiation and longwave radiation plus turbulence, especially in January to June, see dashed green line in Fig. 9 below. Based on the calculation, their correlation coefficients with sea ice extent are quite low. Fig. 6 in the manuscript depicts the time evolution of the 2020 event. We intend to explain that the convergence of the total energy and moisture flux has a significant imprint on downward thermal radiation plus turbulent fluxes, which aids to initiate an earlier than usual melt in 2020. It is meaningless to calculate the correlation coefficients between the longwave radiation plus turbulence and SIE series as their lead-lag relationship can be affected by the feedback mechanism. After the melt commenced, the formation of open water decreased the surface albedo, which in turn acted to increase the absorption of solar radiation. This is confirmed by a significant correlation between SIE and net solar radiation from April to June 2020 (R = -0.78).

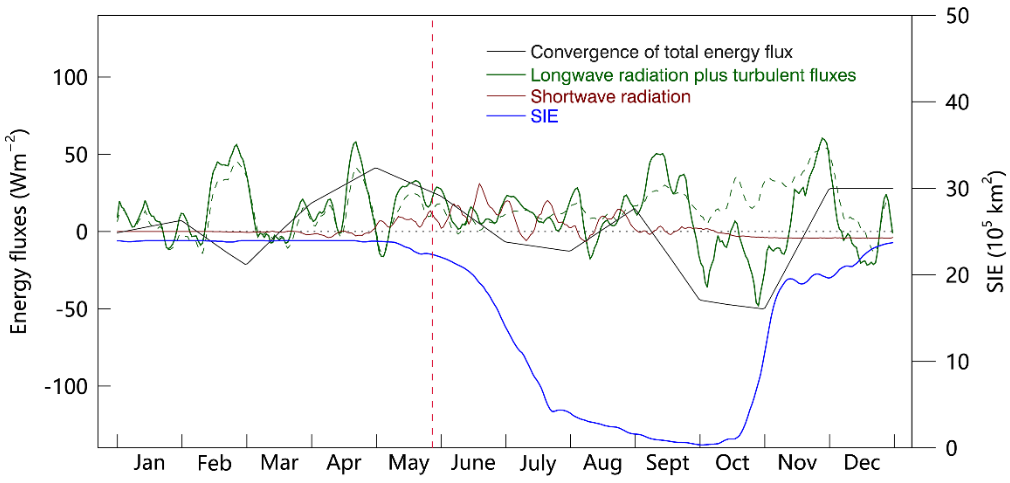


**Figure 9.** Same as Fig.6a in the manuscript, with longwave radiation in the dashed green line.

We think that it is better to preserve the original version of Fig. 6b. As suggested, we replotted Fig. 6b with Y ranging from 150 and labeled the axis with months, see Fig.10 below. However, some bars which denote the standard deviations are broken. Besides, the Y ranges in only two months, it is clearer to use the string "Day of year" same as the unit of the dataset.

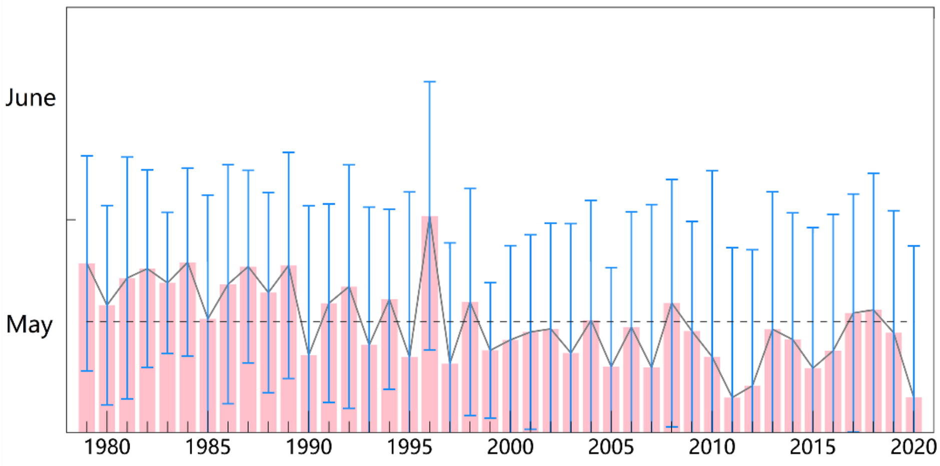


**Figure 10.** Same as Fig.6b in the manuscript, with different plot range and axis labels.

10.Line 272: "response" indicates there is an underlying causality, which is not the case here. Although the two time series match well during Mar-May, the situation is completely different in February or September. Given that sea ice cannot identify different radiation fluxes, it would be more convincing to additionally check net surface heat flux to explore how does the convergence of total energy flux affect net surface heat flux.

Response: Firstly, the word "response" has been replaced with "change" as suggested for a more precise description. Indeed, in February and September 2020, the increased downwelling longwave radiation plus turbulent fluxes was accompanied by moisture convergence a few days ago even though no energy convergence events occurred. In Fig. 6a, only the series of the convergence of the total energy is shown due to its substantial positive anomalies in spring 2020 (on which we focused) compared with moisture convergence. Combined with the response to comment 9, Fig. 6 will be easier to comprehend. What's more, given that sea ice cannot identify different radiation fluxes, we check the net surface fluxes to explore the effect of these anomalies later in the manuscript with results shown in Fig. 7.

11.Line 296: W/m-2 change to W/m2 or W*m-2

Response: Corrected as suggested (Line 317 in the revised manuscript).

12.Line 298: equation (4) change to equation (3)

Response: Corrected as suggested (Line 353 in the revised manuscript).

13.Line 310: in figure 7a, SIT anomaly in Kara Sea is about -1.5m, what is the climatology? Could you please show the climatology plot?

Response: Here, we cannot show the climatology plot. Fig. 7a demonstrates the ice thickness change due to the extra surface energy other than the anomalies related to the climatology of the past four decades. It serves to quantify the thermodynamic impact of atmospheric energy of spring 2020 on the sea ice melt.

14.Line 359-360: the cyclones are grouped into two categories and they are both generated from the land as the thick lines indicate. While in the previous part, there are three main entry channels of the energy and moisture of the North Atlantic, North Pacific, and the Labrador Seas. Could you give some explanations for the mismatch between these two?

Response: Through Fig.8, we detect three main entry channels of total energy and moisture for **the whole Arctic**, where more cyclones with greater intensity propagated toward the Arctic. The strong correlations suggest the significant role of cyclone activity in contributing to the poleward advection of energy and moisture. Next, we examine the **cyclones in the study area** to answer whether cyclones occurred here have contributed to the great transport in spring 2020. Besides, thick lines correspond to the trajectories of two clusters while the actual cyclones are represented by thin lines (which have genesis over the ocean or land).

15.Line 391: in figure 10b, c, What are the definitions of track density and intensity, respectively? Based on the Zhang et al. (2004), CAI is a composite index that measures intensity, number and duration. How to understand the weak track density and CAI near the Beaufort Sea, but the strongest intensity?

Response: The density of tracks denotes the number of distinct cyclones occurring in a particular region during spring. The intensity is referred to as the difference between the SLP of the cyclone center and the climatological monthly mean SLP at corresponding grid points. These definitions were stated in Section 2.2.3. Fig. 10b in the manuscript delineates the anomalies of the density of tracks in spring 2020. Note that **one cyclone track consists of many centers** identified in sequential time steps. Fig. 11 below shows the anomalies in the number of cyclone centers in spring 2020. CAI is defined as the sum of the intensity over all cyclone centers in a particular region during the spring months. As estimated, the intensity has a magnitude of ~10hPa while the **number of centers ranges from 0~50** (which determines the magnitude of CAI value). There are fewer centers in the region near the Beaufort Sea, thereby the CAI there was small despite the strong intensity.

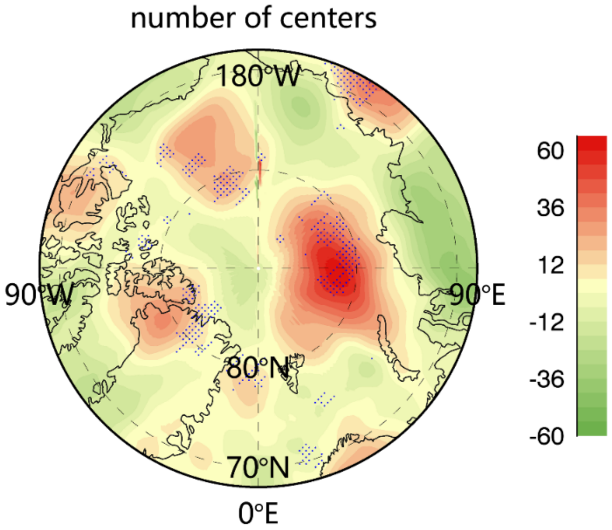


**Figure 11.** Anomalies of the number of cyclone centers in spring 2020.

16.the study area is indicated by pink scalloped frame in figure 1 and green frame in figure 3. Please align it in the full text and revise the description accordingly.

Response: We have revised all relevant figures along with texts and used the same color to depict the boundary of the study area (Fig. 1 and Fig. 3 in the revised manuscript).

17.I understand what the authors trying to express by saying "formation of open water", but it might be better to change it to like “open water is present”.

Response: We have replaced the inappropriate expressions (Line 327 in the revised manuscript).

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