The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-159-RC1, 2021

Comment on tc-2021-159

Anonymous Referee #1

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-RC1, 2021

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.

Specific Comments:

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.

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?

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.

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.

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?

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?

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.

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).

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).

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.

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.

Technical corrections:

Line 102: 'replacing ERA-Interim'

Line 175: remove parenthesis behind Kara Sea.

Line 188: 'unusual conditions with higher'

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

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

Line 282: 'we identify and track cyclone'

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.