First Reviewer

Comment on egusphere-2022-324
Anonymous Referee 1

Referee comment on "Rapid Sea Ice Changes in the Future Barents Sea" by Ole Rieke et al., EGUsphere, https://doi.org/10.5194/e
2022-324-RC1, 2022

I have been asked to review the manuscript "Rapid Sea ICe CHanges in the Future Barents Sea" by Rieke et al. The topic of the work is important for current climate resarch, as the Arctic is constantly changing alongside global warming. In particular, the Barents Sea is a part of the region representing an important connection between the Arctic basins and the remainder of the Eurasian shelves, and lower latitudes. Whereas the data basis for analysis appears state-of-the-art and the analytical approach overall sound there are several details that I suggest be clarified before publication. The method of relating different derived quantities to changes in sea-ice cover is not clearly explained. The calculation of heat transport has, in the past, been challenged, and at least a few sentences of explanation why the approach used here is valid would be in order. Thus, I suggest to publish this manuscript, subject to the modest corrections outlined in my comments. Note that I submit the comments as PDF comments in the form of a comment summary.

We thank the reviewer for all the valuable feedback on our paper. We have thouroughly revisited our manuscript following the reviewer's comments and adjusted parts of the text and figures accordingly. The reviewer's main concern was the calculation of ocean heat transport. Ocean heat transport through individual sections (such as the BSO) must be calculated relative to a reference temperature (Tref), which is in principle arbitrary (Schauer and Beszczynska-Möller, 2009). "In our calculation of OHT, Tref = 0 °C. This is commonly used for OHT calculations in the Barents Sea (Årthun et al., 2012; Smedsrud et al., 2013; Koenigk and Brodeau, 2014; Li et al., 2017). We have calculated OHT using other choices of Tref and find that our results (specifically the magnitude of present and future OHT trends and their link to RICEs) are not sensitive to this (Fig. 1).
Please find below a detailed response to each comment.

- Line 13: could also cite the current IPCC report: Meredith, M., M. Sommerkorn, S. Cassotta, C. Derksen, A. Ekaykin, A. Hollowed, G. Kofinas, A. Mackintosh, J. Melbourne-Thomas, M.M.C. Muelbert, G. Ottersen, H. Pritchard, and E.A.G. Schuur, 2019: Polar Regions. In: IPCC Special Report on the Ocean and Cryosphere in a Changing Climate [H.-O. Pörtner, D.C. Roberts, V. Masson-Delmotte, P. Zhai, M. Tignor, E. Poloczanska, K. Mintenbeck, A. Alegría, M. Nicolai, A. Okem, J. Petzold, B. Rama, N.M. Weyer (eds.)]. In press.)
  We have included the citation of the IPCC report.

- Line 17: scenarios
  Typo fixed.

- Line 56: Please specify what reange of choices you explored (e.g. one standard deviation, three standard deviations...).
  We have explored the dependency on our results based on very different thresholds, using criteria based on the internal distribution (1-2.5 standard deviations) and on external criteria (5-10 times the observed ice decline since 1979). A threshold lower than this does not justify the RICEs being "extreme" events and has thus not been explored. We now mention this in the text.

- Line 56: .
  Missing dot inserted.

- Figure 1 Caption: (SIA).
  Inserted

- Line 61: This appears to be an arbitrary choice. Seawater at typical Barents Sea salinities in winter freezes around -2 deg C. Please elaborate a bit more how sensitive your results are to this choice of reference temperature. Refer to / cite the following publication: Schauer, U. and Beszczynska-Möller, A.: Problems with

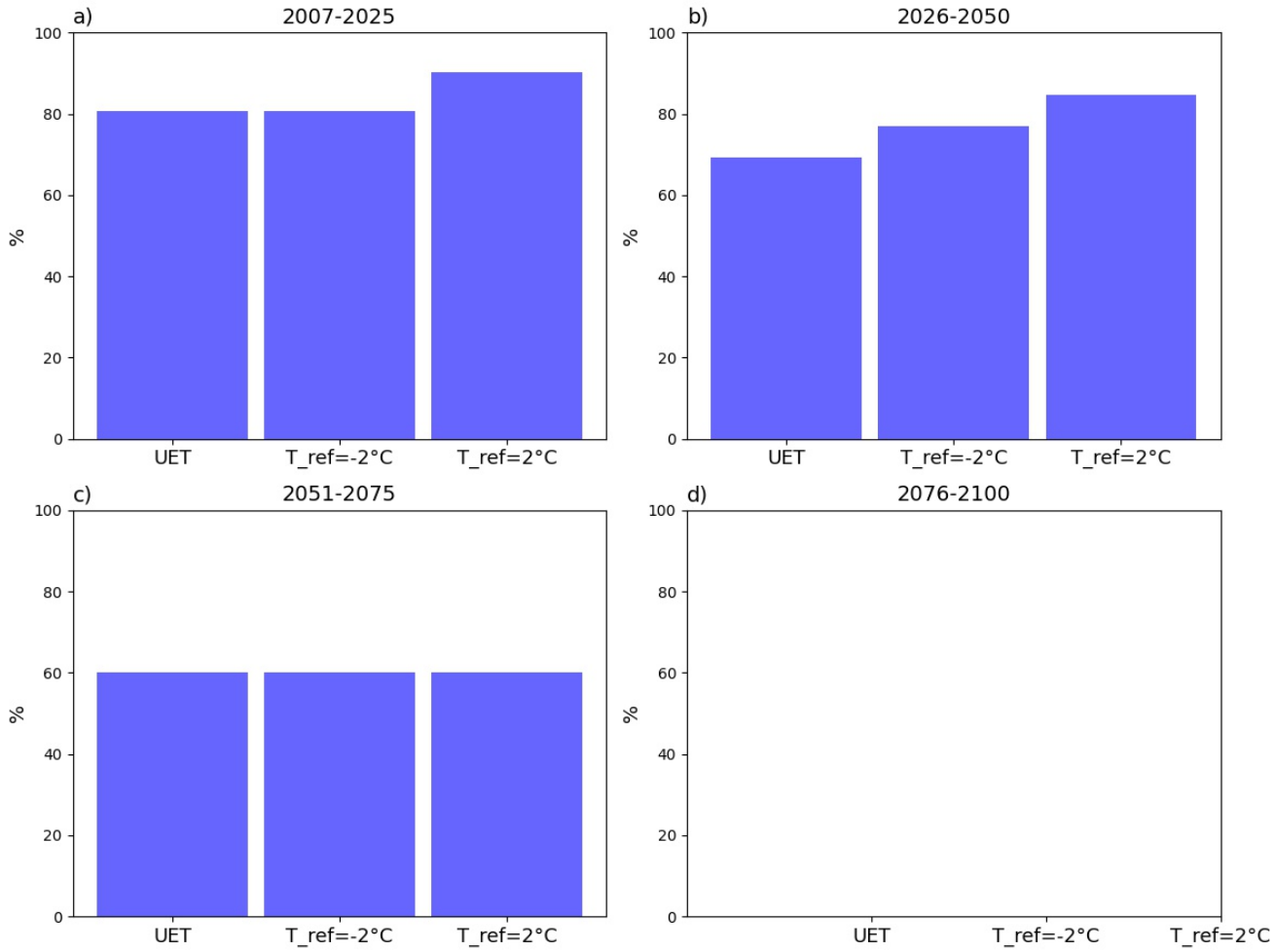

Figure 1: Fraction of RICEs that can be related to trends in ocean heat transport using different reference temperatures. OHT based on the model output variable UET uses a reference temperature of 0 °C.

estimation and interpretation of oceanic heat transport – conceptual remarks for the case of Fram Strait in the Arctic Ocean, Ocean Sci., 5, 487–494, https://doi.org/10.5194/os-5-487-2009, 2009.
As mentioned above, we have calculated OHT trends with different reference temperatures and find that our results are not sensitive to this choice (Fig. 1). The use of reference temperature is now discussed in the text, including a reference to Schauer and Beszczynska-Möller (2009).

- Figure 2: I know this may seem picky, but if you used the same colours for the nLE, n2C and nObs as in the legend it would be intuitively easier to associate the "n" to the correct set of data.
Good idea! We have changed the colours following the reviewer's suggestion.

- Line 107: Looking at Figure 4, I see that the distances increase from (a) to (b) but in (c) only CESM-LE increases further. Perhaps rephrase and state "during the first half of the 21st century"?
We have rewritten this part and hope it is more understandable now.

- Line 116: Again, please specifiy briefly what choice of thresholds you tried.
We have investigated thresholds ranging from one to three standard deviations. This does not qualitatively change the results in terms of the relative importance of the drivers, but only the absolute numbers. One standard deviation is sufficient to indicate a clear relation between the driver and the SIA trend, and provides large enough numbers to investigate the evolution of the drivers over time. This has been added to the text.

- Line 118: I don't understand how you obtained those % numbers – did you calculate some sort of explained variance? How did you "related" the trends quantitatively? Deserves a couple of extra sentences.
  These numbers simply describe the fraction of events where the respective trend of a driving mechanism exceeds our threshold of one standard deviation. We have clarified this in the text.

- Line 121: See my comment above – explain a bit more why the approach of calculating OHT is physically meaningful or why the result is not sensitive to the choice of reference temperature.
  See statement on OHT above.

- Line 124: There are a lot of acronyms in this manuscrpt, which makes it difficult to understand for the reader who is not an expert on sea ice in CMIP6 models. Consider writing at least some of these acronyms out in full – for example, "OHT", "ITE", "ITN", "SHF" could be used in figures (defined in corresponding figure caption) but written in full in the text.
  Following the reviewer's suggestion, we have removed some of the acronyms in the text. For example, acronyms of Barents Sea Opening, ocean heat transport and surface heat fluxes are now restricted to figures following the reviewer's suggestion.

- Figure 4 caption: See comment above.
  Acronyms now explained in figure captions.

- Line 141: Does that mean you just counted the number of times that RICE were related to anomalous trends in SHF (i.e. where the trens in SHF went outside one standard deviation)? Is that the same way you get the percentages in Figure 4?
  Yes, that is correct. See the reply above. We have clarified this in the text.

- Line 149: This would suggest that most of the BSO heat transports are volume-transport-driven, rather than driven by changes in temperature of the advected water. That devserves another sentence... Again, I refer to my comment on heat transports above.
  We find indeed that the wind influences the volume (and hence the heat) transport through BSO. This is consistent with previous studies that have found volume transport to be most important to heat transport variability on interannual to decadal time scales (e.g., Muilwijk et al., 2018; Årthun et al., 2019). This is now stated in the manuscript.

- Line 158: Do you mean that the sea ice loss occured only until 2030? Or does the low average SIA in the model occurs after a strong ice loss that alltogether occur before 2030? Please rephrase to clarify...
  We have clarified this in the text.

- Line 193: See my comments on OHT above...
  See statement on OHT above.

- Line 208: If sea ice change in the northern Barents Sea and the Eurasian basin near the continental slope is a result of Atlantification or, rather, a adriver of it may be an open question (or it's both) – I'd rephrase this as something like "a visible change associated with future Atlantification".
  We have rephrased this as suggested.

- Data availability statement: I would expect a proper citation of data sources here, i.e. cited here or in the main text and put in the reference list. At least the ERA5 and the SIC observations should be citable (with a doi) and available via a repository. Not sure about the model output. In fact, when following your link for NSIDC, there is a clear indication of "Citing these data": " Walsh, J. E., W. L. Chapman, F. Fetterer, and J. S. Stewart. 2019. Gridded Monthly Sea Ice Extent and Concentration, 1850 Onward, Version 2. [Indicate subset used]. Boulder, Colorado USA. NSIDC: National Snow and Ice Data Center. doi: https://doi.org/10.7265/jj4s-tq79. [Date Accessed]. " Please check for the other data sources, as well.
  We have included the citations as suggested for the observational dataset and the reanalysis data. Model output is available via the given links, citation of the models and simulations used in this study is provided in the data and methods section.

References

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