Reply to Reviewer #1

The authors presented a study of MHWs in the Barents Sea, which is very interesting since the MHWs were less studied in the high-latitudes. However, the study cannot be published at the current form for the following major and minor comments:
Reply: We would like to thank the reviewer for all the constructive comments, which we think helped improve the manuscript.

Major comments:
(a) surface and bottom MHWs
It is very interesting to see the bottom MHWs in this study. My impression is that it might be better to have their focus on the comparisons of surface and bottom MHWs, and even change the title of the manuscript.
However, It is not clear whether the MHWs in the section using ROMS data are for the surface or bottom. It might be good to compare both surface and bottom MHWs just as using the TOPAZ data,
Reply: The ROMS model data were used to calculate MHW statistics for both surface and bottom. The text will be rewritten to make this clearer.

(b) Baseline comparison
I don't think this is interesting, since the results are very intuitive at least qualitatively.
I don't think this is meaningful either, since this is just a way for scientists to redefine MHWs for a later period of baseline, but the ecosystem may not be able to get used to the new baseline quickly unless the authors can provide the physical evidence.
Reply: While we agree that the outcomes of the comparison of baselines are intuitive, the comparison was motivated by the different response times of the different components of the marine ecosystem. However, we will perform new analysis using different baselines with less intuitive outcomes, as also suggested by reviewer #2.

(c) MHW algorithm
It is not clear whether the MHWs were diagnosed from the entire time series from 1991 to 2021, or rather diagnosed year by year from January 1 to December 31, since the statement in L83-84 is not consistent with that in Olive et al (2018). See my detailed comments for L83-84.
Reply: We will rewrite this statement to clarify that we followed the procedures as outlined by Hobday et al. (2016) using the algorithm as provided by Oliver et al. (2018).

(d) Heat budget analysis
I am glad to see the heat budgets were used to explain the changes in MHWs, but budgets should be closed. The increase of influx from the Atlantic may not be necessary in favor to MHWs, if the outflux is considered. Also, it is not clear whether theses heat fluxes can be used to explain both surface and bottom MHWs.
Reply: Thank you for pointing out that this part was not clear enough. We will use existing literature to better substantiate the conclusions drawn from the heat budget analysis.

Minor comments:
L13-14, It is not clear for "surface and bottom expressions"
Reply: We will rephrase the text to more clearly state that our analysis include both surface and bottom MHW.

L18-19, the recent studies on the MHWs in the Arctic (Hu et al. 2020 and Huang et al. 2021) are worth citing here.
Hu, S., Zhang, L., & Qian, S. (2020). Marine heatwaves in the Arctic region: Variation in different ice covers. Geophysical Research Letters, 47, e2020GL089329. https://doi.org/10.1029/2020GL089329
Huang, B., Z. Wang, X. Yin, A. Arguez, G. Graham, C. Liu, T. Smith, H.-M. Zhang, 2021: Prolonged Marine Heatwaves in the Arctic: 1982-2020. Geophys. Res. Lett., 48, e2021GL095590, https://doi.org/10.1029/2021GL095590.
Reply: Thank you for the recommendations. We will consider including these references.

L23, I always have difficulty to understand why 90^{th} percentile was selected as an MHW criterion, since from statistics point of view the 90^{th} percentile is really too low.
Reply: We chose the 90^{th} percentile based on previous published literature, and also because our study is investigating MHWs in general, but we do agree that in more specialized studies other criteria may be more appropriate.

L24, the impact of baseline is clear but this does not mean we need to change the baseline since the ecosystem may need time to adjust the changes in baseline.
Reply: This is exactly why we chose to compare the two baselines in the first place. Some parts of the marine ecosystem may still be adapted to the climate in the previous climatological average period (i.e., mid-1900s) and therefore experience MHW as compared to the 1961-1990 baseline and not the 1991-2020 baseline.

L29, Likewise, the removing of the linear trend does not make sense, since our scientists can remove the warming trend but the ecosystem cannot and it at least needs time to adjust the warming trend. It has not been unknown how long it will take for the ecosystem to get used to the new base line or warming trend.
Reply: The rationale here is that in some cases the slow and gradual climate change in itself is the biggest risk factor, whereas in other cases it is the instant shock of a short-lived anomaly that is the biggest risk factor. Removing the linear trend may reveal whether the shocks have become more severe. But this sentence is anyway referring to literature and previous studies, and we have now decided **not to** remove the linear trend when doing the MHW analysis.

L56, it may be better to say something why CTD data is used to your assessment.
Reply: We have changed "*assessing the performance of the two models*" with "*assessing the **quality** of the two models*" and added "***before we use the model results to calculate MHW statistics***" at the end of the sentence.

L83-84, this is not consistent with the statement of Oliver et al. (2018): “Note that when calculating the annual statistics of events which occur across several years, the duration and intensity are assigned to the start year of that event.” Authors need to check and verify the consistency between the python code and the statement of Oliver et al. As the authors acknowledged that the frequency (maybe duration as well) may have been overestimated due to non-rational separation of MHWs across different years. More importantly, it is not clear whether MHWs are analyzed from the starting year (1991) to the ending year (2021). If yes, it is should be easy to fix the above problem. If not, I guess (based on the statement in the manuscript) the MHWs may have been analyzed every year from the January 1 to December 31. If this is the case, the MHWs may have been underestimated for those MHWs sustained from the end of year to the beginning of the next year. e.g. SSTs are above 90% over December 28-31 and January 1-4 of the next year, these SSTs may not be counted as an MHW if they are analyzed yearly, but should be counted as an MHW if they are analyzed for the entire period.
Reply: We will check with the python code and clarify this part of the Methods description, which will make our analysis clearer to the reader.

L98-100, it is easy to understand for the sea bottom MHWs if focuses are the ecosystem of the ocean bottom such as coral reefs. But this should be described much earlier in Introduction section and the Abstract.
Reply: We will enhance the Introduction part to allow for more details and more a comprehensive overview of the background (see also reply to comment by reviewer #2).

L103, should “Table 1” be Table 2? Descriptions are needed for Table 2.
Reply: Yes, we have corrected the text accordingly.

L111-115, it is not clear how these MHWs were diagnosed. Is it different from those based on Hobday et al. (2016) starting from L115?
Reply: The start of the sentence starting on L115 (referring to method by Hobday et al., 2016) has been deleted, because this information was unnecessary and only led to confusion (see also reply to reviewer #2).

Fig. 2., it is not very clear why the time series are from 2015 to 2018, as an example? How about the period from 1991-2014? Why the example of 2015-2018 was selected, and what are the implication for these MHWs. E.g. the connections from the 2015-16 El Niño event.
Reply: We believe the reviewer is referring to Fig. 3, which shows the duration and intensity of the MHW in 2016 in the four different regions. We have added the following to the first sentence leading up to Fig. 3 (L159-160):
*“To look for regional differences, we **chose to** investigate the 2016 MHW event, **which was the most severe MHW event detected in the Barents Sea as a whole,** in the four sub-regions depicted in figure 1”*

L129-130, since the negative trends of the bottom MHWs were not statistically significant, it might be safe to say "no significant trends were detected".
Reply: Changed as suggested

L168-169, the heat is not directly related to the influx, but to the convergence of influx and outflux. What is the change of the outflux from Barents Sea to the Arctic?
Reply: While we agree that the heat content of a volume is determined by the heat convergence within that volume, for the Barents Sea we may still assume that the heat content to a large degree is determined by the inflow from the southwest. This is because the outflow of oceanic heat to the northeast is almost negligible and the largest heat sink is heat lost to the atmosphere while the water is traversing the Barents Sea. It has been shown in several studies that the heat transport **from** the Barents Sea is small and that most of the oceanic heat is lost to the atmosphere in the Barents Sea (e.g., Gammelsrød et al., 2009; Lien & Trofimov, 2013; Smedsrud et al., 2013; Skagseth et al., 2020) and that increased inflow to the Barents Sea cause increased heat within the Barents Sea and reduced sea-ice cover (e.g., Onarheim et al., 2015; Lien et al., 2017).
We will rewrite the text and include references to existing literature to better substantiate our conclusions regarding the link between inflow and MHW events.

L172, What is the "turbulent heat", is it sensible, latent heat fluxes. How about solar radiation fluxes?
Reply: We have clarified this point by adding "*... the turbulent **(latent and sensible)** heat loss ...*" at both occasions where turbulent heat loss is mentioned.
Solar radiation fluxes are negligible during the DJF/winter period, due to the Polar Night at the latitude of the Barents Sea. We have also added this information explicitly.

Fig. 5., Can the heat flux analyses be applied to both surface and bottom MHWs? What results the differences between surface and bottom MHWs?
Reply: The surface heat fluxes affect the bottom MHW indirectly through vertical mixing during winter, while during summer the surface and bottom layers are usually separated by stratification. We will elaborate on this in the revised manuscript.

Table 3, "Number of marine heatwave events per year during the period 1961-2020" is very confusing and out of context. I think the same period of 1991-2021 should be analyzed and compared with different baseline periods of 1961-1990 and 1991-2020, which can also be compared with the results presented in section 3.1.
Reply: We have changed the title of the table to:
*"**Average frequency** of marine heatwaves +/- the decadal trend for two different baseline periods, 1961-1990 and 1991-2020."*

Tables 3-5, I assume these are for the surface MHWs, what about the bottom MHWs?
Reply: Tables 3-5 show results both for the surface and the bottom. We have now stated clearly in the first sentence of the paragraph that the calculations are done both for the surface and the bottom.

L237-238, Does this imply that the ice may be melted at the bottom while remained at the surface?
Reply: The sea ice affects the bottom only indirectly through the sinking of cold, brine-enriched water as a consequence of sea-ice formation at the surface. Fr clarification, we have added the following:
"*[...] sea-ice formation on nearby banks* ***and thus a reduction in the sinking of brine-enriched surface water****.*"

L238, is this "sea-ice cover" the surface ice or bottom ice cover?
Reply: There is no sea ice on the bottom. See also the reply to the comment above.