We kindly thank the reviewer for their very insightful and helpful comments. Below we include line-by-line responses to each comment raised by the reviewer. Where applicable, we indicate how the revised manuscript has been/will be modified to address the comment/proposed change.

Major comments :

The definition of the “SSH curl” index is not convincing. Why do the authors not use a simple SSH difference instead? The various links in the ocean feedback mechanism should be described in more detail. The authors could compute the time evolution of the volume of dense water in the formation area, or the water mass transformation, to more clearly demonstrate an increase in dense water formation.

A: This is a good point. To fix this, we have instead switched to using the magnitude of the SSH gradient which we think is more straightforward to interpret. We have also added a short summary of our revised explanation of the ocean feedback hypothesis to improve the discussion of the ocean feedback hypothesis.

The authors should describe how they compute the temperature index of the boundary current. What are the horizontal and vertical extents of the section? Does the section encompass both the Barents Sea Branch and the Fram Strait Branch? The authors could also map the temperature trend at each point or within sub-regions of the vertical section to determine which layer is most affected by the trend.

A: Thank you, however, we are a bit confused by this comment. If referring to Figure 5, we note that it is the mean temperature (averaged over the surface of the section) and we provide the extent in table 1. The extent is from 81.5 to 83N which covers both branches of the boundary current. However, to clarify this, we have made reference to table 1 in the caption of Figure 5.

The list of references is short and should be completed. Here are some examples. For the observation in Saint Anna Trough, the authors should at least cite Schauer et al 2002 (DSR). For the discussion of the origin of the trend in the heat transport at BSO (Wang et al. GRL, 2019). Cai et al. 2022 ERL, for the role of the surface heat fluxes on the Barents Sea warming. Some papers of Arthun could also be cited. Beszczynska-Möller et al. 2012 for the trend in temperature at Fram Strait. RuizCastillo et al. 2023 (JGR) for the structure of the Arctic Boundary Current North of Severnaya Zemlya. Richards et al. 2022 (JGR) for the trend in the temperature of the Atlantic Water core in the Eurasian basin.

A: Thank you for including these references. We have attempted to incorporate them into our discussion where possible.

The discussion could be more detailed. In particular, the authors find that the trend in the heat transport at BSO is entirely due to warming temperature. How does this result compare with previous study of the heat transport at BSO?

A: Thank you, we have lengthened the portion of the discussion which addresses the trend in heat transport by discussing comparison to prior works like Wang et al., 2019; Skagseth et al., 2020; Skagseth et al., 2010.

Minor Comments:

l.11:" we present the first observational evidence": sounds a bit odd since the authors are using a model.

A: We have changed the wording to better reflect this. It now reads: 'we present empirical evidence for a revised version of the "ocean feedback" hypothesis'

L.18: "cooling machine" add a reference.

A: We have added appropriate references.

L.19: "into denser cooler Barents Shelf Water". Add a reference.

A: We have added appropriate references.

L.45: already said l.23-24.

A: we have removed the redundant line at L45.

L. 62: "coupled model": Could the authors be more specific: ice-ocean coupled model?

A: Thank you. Yes, we meant ice-ocean coupled. This has been added.

L.63 references for "World Ocean Database, ICOADS 3.0 and NOAA nighttime L3 Sea surface temperatures.

A: Thank you, we have now added these references.

L.63. The authors might specify the number of profiles assimilated in the Barents Sea and St Anna Trough.

A: This is a very good point. We have added a line specifying the approximate number of casts assimilated from WOD.

L.64 Are the ERA5 surface fluxes imposed? Or is the ERA5 surface air temperature imposed and the turbulent fluxes recomputed based on the SST of the SODA model?

A: This is a good point. SODA4 initially imposes the surface fluxes from ERA5 but then

recalculates them to adjust for consistency with SSTs and remove bias in the ERA5 output. In the manuscript, we add a line referring the reader to Chepurin et al., 2025 which describes the process.

L. 72. Since the authors compute difference between the first and the last period why do they not use 2 periods of the same length (i.e 1980-1989 for the first period and 2015-2024 for the second one).

A: This is a good point. We have opted to use only the 2020-2024 period due to the strength of the trend in heat content and heat transport, which produces significant changes in heat content or heat flux, even over a 5 year period (heat transport through BSO would be ~1.2 TW greater in the 2020s than the late 2010s based on the 0.23 TW/year trend). We felt that by not including the second half of the 2010s, the differences more appropriately showcased the magnitude of changes in the Barents Sea in recent years. We do not feel that this hinders the interpretation of our results, as we still show time series of the heat convergence and heat transport over the full period, and the focus of our difference plots is to showcase the spatial pattern of the Ocean Feedback mechanism and correlations between each component, and the correlations themselves are computed over the entire time series as well.

L.77: STA. not defined.

A: This has been fixed.

L.78-81: clarify your definition of “SSH curl” or use a simple SSH difference.

A: We have switched to using the SSH gradient in place of the SSH curl to reduce confusion.

L.80 “1000m” specify what does it mean in term of water mass for the Fram Strait Transport. Does the Fram Strait section encompass the EGC? Could the authors be more specific about the method used to compute the heat transport? Furthermore, the transports through Fram Strait could be compared with observations (Beszczynska-Möller et al. 2012).

A: We have included a line which specified our choice of cutoff at 1000m. With regard to the Fram Strait transport, this could mean that the total transport through Fram Strait is underestimated, but the trend would be unaffected. While we agree with the reviewer that comparison to observations would be helpful, we found that including a direct comparison would be a bit outside the focus of the paper since the discussion of Fram Strait is intended to motivate our discussion with regard to the importance of the trend through STA to the boundary current, rather than being it’s main focus. This is further complicated by our need to use a different set of boundaries for the Fram Strait section than is used in observations (since we exclude the EGC) which hinders direct comparison. However, we do add a short discussion, noting a temperature trend through Fram Strait, which is smaller in magnitude but of the same direction as that identified by Beszczynska-Möller et al. 2012, to show that our findings are still somewhat consistent with observations in the region.

L.102: It seems that a time derivative is missing in the formulae.

A: We are somewhat confused as to what the reviewer means. This is simply a rewriting of Green's/Gauss' Theorem, which states that the divergence of a quantity (in this case heat) over a given volume is equal to the flux across all surfaces entering that volume (in this case, it is the sum of the heat transports across all the boundaries D_i). This quantity should be time-independent so long as all surfaces are considered.

L. 107. Could the authors indicate the NB section in figure 1.

A: Thank you, the NB section has been added to Figure 1.

L. 111: The title of the section is a bit general

A: We have renamed to "decadal changes in hydrography".

L. 116: replace "greather" by greater.

A: This has been changed.

L.115. Without any analysis of the surface heat fluxes and heat transports at the boundaries, it is premature to conclude that the increase in heat content is due to warmer inflows.

A: This claim has been removed.

L. 120. Fig 4a is cited before figure 3.

A: The sentence referencing Figure 4a has been moved to fix this.

L. 122: Are the Kola section data included in the world ocean data?

A: Yes, both the Kola section and Bear Island section data are included in the world ocean database.

L. Figure S2. Cited before figure S1.

A: The supp. Figures have been reordered to address this.

L. 120-132.I suggest to move this section at the head of the paragraph (as a validation of the model).

A: We are somewhat confused as to what the reviewer is referring to. Line 120-132 seems to refer to an entire paragraph, which does discuss the validation of the model already.

L.136: replace fig 4b,c by fig.5

A: this change has been made.

L. 140 . Beszczynska-Möller et al. 2012 suggested that the temperature of the AW core displays a trend in Fram Strait. Is this trend present in the SODA model? It might be easier to compare equivalent quantities. If the heat transport at St Anna Trough is dominated by the temperature variations, it would be easier to directly compare the variations of temperature at St Anna Trough and in boundary current.

A: Thank you. As noted above, we have added a brief discussion about the trend in temperature through Fram Strait.

Figure 2. Could the authors add standard deviations ?

A: This is a good point. We have now added standard deviations to this figure.

L.147 The authors could include a reference to fig. 4b.

A: This reference has been added.

L.149: The authors could include a reference to fig. 4c

A: This reference has been added.

L. 154: accommodate.

A: This has been fixed.

L. 155: A time series of the surface fluxes would help.

A: While we agree with the reviewer that a time series of heat fluxes would provide further understanding of the trends in heat flux, we have chosen to omit this to preserve the focus of the paper. A proper discussion of the time series of heat fluxes should involve defining regions within the Barents Sea so that we can see the time series in different locations (for example the southwest vs. the northeast) and see how the site of heat loss migrates northward. We felt that defining these locations and providing information with regards to how we chose them would introduce unnecessary length and complexity to the paper given that the change in heat fluxes are not one of the primary key points (instead the main key points focus on the temperature-driven increase in heat transport, heat transport through STA, and the ocean feedback mechanism).

Figure 4: Why do the time series stop in 2020?

A: Thank you for pointing out this oversight on our part. We have fixed the time series to now show the full record (1980-2024).

Figure 6: Indicate that positive values correspond to a heat loss for the ocean.

A: This has been indicated.

Figure 7: a mean state of the mixed layer depth could help better understand the changes in MLD.

A: While we agree with the reviewer that the time mean MLD would help the reader better understand the changes, we show MLD not to illustrate the actual changes with MLD but instead to further substantiate the idea that there is dense water formation since we see the mixed layer is deepening and stratification at the base is eroding, providing some evidence of dense water formation. However, it may be the case that instead these two panels should be removed if the reviewer thinks they do not contribute significantly to the discussion. For now, we have kept them but can remove them if the reviewer prefers.