We kindly thank the reviewers for their additional comments and thoughtful consideration regarding our manuscript. We think that these comments have significantly strengthened our manuscript. We have done our best to respond to each of the comments posed by the reviewers, with responses shown below in blue. We note the responses to each reviewer in bold.

Reviewer 1

Second review of the manuscript "Increased ocean heat transport to the central Arctic despite a well working Barents Sea Cooling Machine", by S. Eisner et al.
The authors have taken into account most of the comments provided in the first round of review, which has improved the manuscript and its readability. There are still some issues, however, that are unclear and that relate to some of the fundamental aspects of the study. Most notably the proposed feedback mechanism, which still require further clarification.
The proposed feedback mechanism, as visualized in Fig. 10, has some logical flaws (steps C and D). Increased densification of the water masses in the northeastern Barents Sea will cause a depression in the SSH, because the water column will contract (higher density means the same mass will occupy less volume). And if the SSH in the northern Barents Sea is depressed, it will create a barotropic pressure gradient that will induce a velocity component with higher SSH to the right. In this case, the response will be an anticyclonic circulation in the St. Anna Trough (Fig. 7; box D in Fig. 10). But that would mean a velocity component pointing southward in the eastern St. Anna Trough and pointing northward in the western St. Anna Trough, i.e., a weakening of the circulation, not a strengthening. Rather, as stated in Smedsrud et al. (2013), a densification of the water masses in the northeastern Barents Sea, will cause an accelerated density-driven current downslope into the St. Anna Trough. This will act to enhance the already significant baroclinic component of the outflow. That accelerated outflow, in turn, will by continuity act to increase the throughflow through the Barents Sea. Or at least, that is one plausible consequence, while another plausible consequence is just an increased overturning or lateral circulation within the St. Anna Trough. But correlation analysis in Smedsrud et al. (2013) pointed to at least a contribution to the former (i.e., increased throughflow in the Barents Sea). The interpretation of the results needs to be clarified to make the conclusions in this regard logically sound.

Thank you for this very detailed comment. In general, after closer examination we agree with the statement of the reviewer, with a few minor changes. Upon closer examination, we have modified our description of the feedback mechanism and provided additional figures and text to support this modification to try to create a more logically consistent feedback. For the indicated changes, see the abstract, sections 3.4, 3.5, and the discussion section as well as the newly updated Figure 10, and the newly added Figures S5,S6, and S7 which depict the salinity changes, the time mean SSH field and the trend in SSH. The next paragraph gives a brief summary of our modified depiction of the ocean feedback mechanism based on our additional analysis.

After looking at the time mean SSH, there is a persistent low in the Barents Sea and a persistent high in the Kara Sea (likely originating from fresh river outflows). However, when we look at the long-term change in SSH there is an SSH depression in both the Barents Sea and Kara sea that drives the anomalous flow that the reviewer describes. Our interpretation is that the time mean state depicts the 'typical' functioning of the ocean feedback which operates over the 1-4 year timescales we mention and is supported by the correlation plots (Figs. 8,9). So, this produces a time-mean picture of the ocean feedback as shown in the new Figure 10. However, while the long-term increases in salinity and density would support an enhancement of this feedback as time progresses, the changes to SSH acts to weaken the feedback, producing a picture of an ocean feedback which works in the time mean sense but has potentially weakened or at least shifted over time because of the shift in the direction of the SSH gradient (compare Figures S6 and S7 or S6 and 7a,c). This would also explain why there is no increase in the volume transport through BSO or STA over the 40 year period because the feedback has weakened. We have updated the manuscript to try to better reflect this story, primarily in the abstract, sections 3.4 and 3.5 and the discussion section.

Specific comments:
L7-8: The reply to my comment to this statement is not satisfactory. Because you state that the waters in the northeastern Barents Sea have become colder and denser due to the northward migration of the maximum ocean-atmosphere heatfluxes. But in the sentence prior, you state that the positive trend in the heat transport at both ends of the Barents Sea is entirely due to increasing temperature. And everywhere in the manuscript, you show that the temperature has indeed increased: in the inflow through the BSO and downstream in the Kola section (Fig. 3), in the water flowing out through the St. Anna Trough (Figs. 4, S1), and on average the heat content everywhere in the Barents Sea has increased (Fig. 4), and the overall temperature of the Barents Sea has increased (Fig. 2). Nowhere is it shown that the temperature has decreased, except that you have shown that the heat loss has increased. Thus, the temperature in the northeastern Barents Sea has decreased relatively to the temperature of the inflowing AW in the BSO, but in absolute terms the temperature seems to have increased throughout the whole region, which points toward the salinity as the main culprit for the increased density.

Thank you for this comment. This is absolutely correct. After looking into this further, we found that indeed the average salinity of waters in the Barents Sea had increased over the 40 year period. Along with the increase in temperature, this is of course consistent with the fingerprint of "Atlantification". This gives an altered picture of the feedback mechanism which relies not on the increased heat loss in the northeastern Barents Sea but instead on higher density associated with saline waters. To highlight this, we have changed the line in the abstract and further clarified our new, modified interpretation of the ocean feedback in the abstract. We also further clarify this in Section 3.4 and the discussion to note that the denser waters are driven by salinity changes since the average temperature is increasing.

Figures:
Figure 7: I am still baffled by the panel a) stating that there is a full unit (kg/m3) difference in the density between the 1980s and the 2020s for the full water column in, e.g., the middle part of

STA, where the water depth is >500 m and the water column consists to a large degree of Atlantic Water from the Fram Strait branch.

This is a very good point and certainly this is a very notable change. However, we think that this makes sense given the changes in salinity observed over the period. As the reviewer helpfully pointed out, the increased density appears to be the result of increases in salinity, which we further show in our supplemental figure (S5). In this figure, there are salinity changes over the 40 year period that are the order of 0.8-1.2 psu. In some locations, where significant freshwater input has occurred (such as the Kara Sea or in the vicinity of landmasses, where there may be glacial runoff or melting of landfast ice), these changes can reach nearly 2 psu in SODA4. We used TEOS-10 to do a quick estimate of the effect of these salinity changes on density for water masses with typical Atlantic Water/Fram Strait properties (0-2 C, >34 psu). When we do this estimation, we find that for water at 0-2 C, a change from 35 to 34 psu results in ~0.8 to 1 kg m-3 change in density. So, if the changes in salinity are realistic, we think that this could feasibly explain the dramatic density changes observed. We explain this in the newly revised section 3.4 and also highlight our lack of ability to assess the reasonableness of salinity anomalies very far away from Bear Island and Kola sections or in regions of very sparse observations. Therefore, we note in this section that in these extremely northeasterly regions, the most we can say is that the density changes are consistent with the SODA4 salinity anomalies. We then of course note that we can have far more confidence in the anomalies closer to the observational sections where we have directly assessed consistency with observations. THis discussion is in the newly updated Section 3.4.

Figure 8: I still think the unit on the x-axis in the left panel (m/km) must be wrong, and that there is a 10-3 missing. Because a gradient of up to 16 m difference in SSH per km makes no sense, and in Figure 7, the maximum value is on the order of 0.01 m. Please check again.

We thank the reviewer for pointing this out. We apparently forgot to correct the axes amidst switching from the previous “SSH curl” to the current SSH gradient. Indeed, the axes should say 10-3. This has been corrected.

Figure 10: I am still not convinced that your steps C and D form a logical or plausible part of a feedback loop.

We thank the reviewer for this comment. Based on our revised interpretation which we describe in the response to the general comment, we have revised the diagram of the feedback loop to better represent our updated story, which we describe in the response to the general comment. In this new diagram, we emphasize the role of salinity and the persistent SSH high in the Kara Sea in maintaining the feedback loop, which acts on 1-4 year timescales. We note that even though there is continued warming, the feedback requires increased salinity and a sufficient baseline amount of surface cooling to maintain the density increase. As mentioned in the response to the general comment, we also included lines in the discussion and in section 3.5 to clarify these points further.

Reviewer 2

I am sorry but I was a bit confusing in my previous review. I still believe the authors should provide a clearer description of the temperature trend in the ACBC core. Since the ACBC section includes both the Barents Sea Branch and the Fram Strait Branch, could the authors calculate the temperature trends for each branch separately? How do both trends contribute to the overall temperature trend observed in the ACBC core. In the model, the temperature in the Fram Strait also exhibits a trend, which is approximately half the magnitude of the SAT trend. Could the authors discuss the respective impacts of these temperature trends—both in the Fram Strait and in the SAT—on the observed changes in the ACBC core. Additionally, in the discussion section, the authors should also add some lines to discuss the novelty of their results regarding the temperature trend in the ACBC core. This could include a comparison with previous studies, such as Richards et al. (2022, JGR).

Thank you for clarifying this in more detail. Regarding the contribution of Fram Strait and STA trends to the ACBC, we have added an additional paragraph at the end of section 3.2 which places the trends in context with each other and outlines how the STA trend and ACBC trends are nearly double the magnitude of the trend through Fram Strait. We also have included lines in the discussion (at the end of the second paragraph) highlighting how this fits in with the literature (noting the implications for downstream heat transport and consistency with studies like Polyakov et al., 2025).

With regard to the trends in the individual branches, we think that this is certainly an important avenue to look at moving forward but think that it may provide confusion with regard to the central story. Our reasoning is that once the two branches “combine” into the ACBC there is significant exchange of heat between the two, often with water exiting from STA serving to cool waters in the Fram Strait branch. Therefore, despite a small trend in temperature of water entering Fram Strait, we may see a much larger increase in temperature of the downstream Fram Strait branch simply because the waters exiting STA are warmer and now less effectively cool the Fram Strait waters after combining at the shelfbreak. Beyond this, the mechanism for cooling between the two branches is poorly constrained in the reanalyses. A number of studies (such as Spall, 2013 JPO; Pnyushkov et al., 2018 OS) suggest that diapycnal mixing by mesoscale eddies may be significant along the shelfbreak, especially in the vicinity of where the two branches combine. Since the mesoscale is poorly constrained in nearly all arctic reanalyses including SODA4, it would be difficult to say if the heat exchange between the two branches is realistic and thus difficult to make sense of whether a trend in temperature in either the Fram Strait or Barents Sea Branch might mean. We do think however that with a fully mesoscale-resolving numerical model, these sorts of questions could certainly start to be addressed and would have important implications for understanding heat exchange between the two branches. To emphasize this, we have included a sentence at the end of the second paragraph in the discussion focusing on the need for future work to explore this avenue.

Specific comments :

L.83 Could the authors show in a figure (7c for example) the area over which the SSH gradient is computed.

THis is a good point. We have added a border to Figure 7c to indicate the area over which we evaluate the SSH gradient for the correlation plots.

Figure S1: Could the authors change the scale for the salinity so that the reader can see some patterns in the Barents Sea. Could they specify if the fields are vertically averaged? Or indicate the depth of the fields.

Thank you. We have tried to update the colorscale to make differences more visible. We have also indicated that the salinity differences are indeed vertically averaged.

L.116: I suppose that heat content is computed from the surface to the bottom? Could the authors specify? Could the authors show in figure 1 the limits of the Barents-Kara sea region over which they computed the heat content

Thank you for this comment. We have included an additional line to clarify how heat content is computed. Instead of adding a boundary to Figure 1, we explicitly noted that the total heat content is computed over the exact region depicted in Figure 4a (which shows the change in per grid cell heat content) in addition to the clarification about how it is depth integrated. We chose to do this instead of add the boundary to Figure 1 as it significantly overlaps with the northern boundary and BSO section and we felt it would be hard to actually identify the boundary in Figure 1 with all of these sections on top of it.

L:121-130. I was certainly unclear in my previous review, but I still believe that the section 3.1 should start with the validation of the model. Then the authors can present the new results brought by the model analysis. Or at least, first present the comparison of the mean SODA state with the World Ocean Atlas and then present the trend in the observations and the model.

Thank you for the clarification. Indeed this is a great point and the comparison with observations should certainly come first. We have now rearranged the paragraphs so that it begins first with the validation of SODA4 against Bear Island and Kola section, then World Ocean Atlas 2023. The paragraph discussing heat content and hydrography comes last in the section. To further reinforce this, we have reordered Figure 2 & 3 so that the times series of anomalies is now before the hydrographic profiles.

L.141: “the additional heat advected into the Barents Sea is being lost”: unclear. The authors did not compute any heat budget, therefore there is no indication of which part of the additional heat transport convergence is used to warm the Barents Sea and how much is lost to the atmosphere.

This is a good point and very true. We did not compute a heat budget so we cannot technically say how much heat is “lost”. Instead, the point here was to highlight that the trend through STA is less than half of that through BSO, indicating that the additional heat transported through BSO is in some way mitigated prior to exiting through STA. To clarify this, we make the statement more explicit by saying “at least half of the additional heat advected into the Barents Sea through BSO is mitigated prior to exiting through STA” even though as you rightfully point out, we cannot say where exactly the heat has gone.

L.144: Could the authors give all the temperature trends in °C/year for an easier comparison between the trends.

Thank you for this comment. The trend has been converted to °C/year.

L.151: “the true heat convergence” : averaged over the full length of the simulation? Could the authors specify? What is the standard deviation? The SODA value is compared with Smedsrud et al. (2010) estimate. Could the authors specify how this estimate is done. Over which period. Maybe the comparison between the 2 estimates could be done over the same period?

In order to address this comment, we have included clarification about the period over which we compute the “true heat convergence” and include the standard deviation as a +/- uncertainty. We also include a brief sentence describing how the Smedsrud et al., 2010 estimate is obtained. As far as we are aware, there is no clearly defined period for the Smedsrud et al., 2010 estimate because theirs is obtained by synthesizing all available prior estimates of volume transport and temperature through each of the major openings (BSO, northern boundary, Kara gate, etc). In some cases, this may include observations even from before the beginning of our reanalysis coverage, while in others, there may be very few, relatively recent observations, so it is difficult to say what the actual period of coverage would be in their case. We use the heat convergence over our full record to try to be as close as possible by using “all available data” in our reanalysis in the same way that they used all available prior observational estimates.

L.158: The net volume transport through BSO is the sum of an inflow in the southern part of the opening and an outflow in the northern part of BSO. Are the trends of these transports equal to 0.?

This is a great point and we thank the reviewer for raising it. We do not examine the trends in the individual inflowing and outflowing branches through the BSO, but we certainly agree that it is an important part of the story. The reason for not separating the BSO transport into its two branches is twofold: first, we think that it is slightly beyond the scope of our study, whose main focus is on the net heat transport (particularly with an eye to how this transport is affecting downstream heat transport in the central arctic) and the proposed ocean feedback mechanism, which both are based upon the net heat and volume transport through BSO. Secondly, we think there is a bit of complication with using SODA4 (or most reanalyses) to investigate the individual branches. From our understanding, the BSO recirculation is somewhat poorly constrained in a number of reanalyses and since the focus of the work is not on the recirculation, we do not want to introduce this added complexity of poorly constrained recirculations to confuse the reader and distract from the main story. However, we certainly agree that a discussion of the recirculation is important. For this reason, we have included a few lines at the end of this paragraph which point out that our focus on the net volume transport and findings that there is no trend in the net volume transport certainly does not preclude the existence of individual trends in the inflow or outflow.

L.162. is the first sentence useful? See comment L.141.

This is a good point. We have removed the first sentence to reduce confusion.

L.172: The title of the section 3.4 is maybe a bit misleading, indeed the authors do not really analyze dense water formation. They do not study the properties of the water masses (the dense water product is not defined), and their formation rate. They show that there are" cooler and denser waters in the northeastern Barents Sea" (as mentioned in the abstract).

Very good point. We have changed the title of this section to "Denser water is the Northeastern Barents Sea".

L173. The mechanism has already been defined in the introduction.

Indeed. This sentence was redundant and has been removed.

L.188 replace "SSH curl" by "SSH gradient".

Thank you. This has been replaced.

L.191: BSO inflow is not defined. I suppose that it is the net volume transport through BSO. Could the authors be more specific? What are the correlations between the SAT net volume transport and the BSO inflow and the BSO outflow respectively? Are these correlations worth discussing? Would a regression of the velocity (barotropic?) in the Barents Sea on the SAT net volume transport give some hints on the link between the SAT net volume transport and the volume transports in BSO?

Thank you, we have clarified by replacing 'inflow' and 'outflow' with 'net volume transport'. We think that the reviewer raises an interesting point about the separate BSO inflow and outflow components (one through the southern BSO and one through the northern BSO) and certainly this would be an interesting direction to investigate further. However, we think that this sort of analysis and discussion extends beyond the intended scope of the study. Since the primary goals are in understanding the trends in heat transport and to test the ocean feedback mechanism, looking at both the BSO and STA in terms of net transports is the primary focus since that was what was proposed in the Smedsrud et al., 2013 study. However, as we state here, we certainly agree that this would be a very interesting direction for future study, especially to better understand the newly revised ocean feedback hypothesis.

L.243 “continued” might be removed.

Thank you for this comment. ‘Continued’ has now been removed.