The author should clearly state what time scales these results applies to. In the data and method section they should describe how the data are processed before going into the analysis. I find some information for the ssh and hydrography data (for atmospheric data little information is given). It is necessary to provide more details on this, e.g how are the data de-seasoned, are the results sensitive to the choice of method.

We thank the reviewer for pointing this out. Additions to clearly point out the time scales considered, in both Section 1 and 4, will make the paper easier to grasp and also express novelty. See also further comments below. We now also extend Section 2 to include more information on how the ssh, hydrographic and air-sea heat flux data have been processed before the analysis.

A main conclusion of the paper is that a simplified model of ocean heat convergence, with only upstream temperature measurements at the inflow to the Nordic Seas as input, is able to reproduce key aspects of the decadal variability of the Nordic Seas. The authors briefly mention that the residual could be related to changes in vertical heat flux or volume flux, but none of these are investigated. Their argument based on the decadal comparison of surface fluxes to exclude the heat fluxes in their forward model integration seems weakly justified. Adding to this, Mork et al. (2015) concluded that air-sea heat fluxes explained about half of the interannual (year-to-year) variability in heat content tendency. Further, from the hydrographic data the authors could check their assumption of a similar AW temperature and outflow temperature. Since a number of papers already have pointed to the importance of temperature anomalies from the North Atlantic propagating through the Norwegian Sea, I think more conclusive results on what mechanism explaining the residual variability (i.e. not explained by inflow-T) would make the paper more novel. E.g. by extending the model to include some of the above points?

Answer: We thank the Reviewer for these comments. The decadal comparison of surface fluxes essentially rules out the role for air-sea fluxes for driving the heat content anomalies. This comparison should not be compared to the study by Mork et al. (2015) because of the different time scales, interannual vs. decadal. The latter is the focus of our study, where the decadal comparison shows a minor role for air-sea fluxes, which further suggests that on these time scales advection of temperature anomalies by ocean currents from the North Atlantic dominates the heat budget. The decadal time scales considered also adds to the novelty.

We have decided to not explore how well the mean AW temperature is related to the outflow temperature, defined at the northern boundary of our domain located roughly in the Lofoten Basin. For variations with time scales of a few years this should be a reasonable assumption, since at low frequencies the temperature anomalies in the AW tend to have large horizontal scales.

In response to questions from both referees on the residual in the conceptual model, we have expanded the discussion on the relative importance of anomalies in volume flow and temperature. We have changed some text on page 8 and 9: We now state that observations of Mork and Skagseth (2010), Berx et al. (2013), Bringedal et al. (2018), and Østerhus et al. (2019) suggest an increase of the Atlantic inflow to the Nordic Seas from the mid 1990:s to the early 2000:s. Further, we now write that this increase in volume transport can qualitatively explain why the observed temperatures are higher than those in the conceptual model (which is forced only by temperature variations) in the early 2000:s.

Third, the authors find that the correlation between the ssh and heat content is low (provide number). The authors have used a fixed depth 657m to calculate the heat content. However, as the Atlantic Layer in the Lofoten Basin extend deeper than this, and is time-varying, the authors should assure that their results are not a sensitive to the choice of heat content integration depth. This could be tested calculating the heat content down to e.g. 1000m. Also, regarding the interpretation of the baroclinic trans- port function (Fig2b) as a strengthening of the Baroclinic Front Branch at the expense of the Slope Branch. It seems that the positive anomaly is quite far from the slope. The core of this anomaly seems to match the Lofoten Eddy (that varies both in strength and position). Can you exclude that this is not the signature of the Lofoten Eddy that is smoothed in the hydrographic data set?

Answer: We thank the author for this suggestion concerning the sensitivity to the integration depth. We already had a sentence about the integration depth (end of Section 2.2), but this only concerned the spatial pattern of the heat content, and not the correlation with the SSH. After testing the correlation to the heat content integrated to 1000m, it does not seem to be sensitive to the choice of depth. We have now extended the discussion on the sensitivity to integration depth.

About the signature of the Lofoten Eddy: the largest trend of the potential energy (as well as the steric height, now shown in the revised Fig. 2) in the central part of the Lofoten Basin indeed coincides with where the Lofoten Vortex resides. As suggested by the referee, the larger-scale positive trend seen in potential energy, steric height, and heat content (Skagseth and Mork, 2012) in time- and spatially-averaged hydrographic data sets may reflect an increase in intensity and number of mesoscale anticyclonic eddies in the Lofoten Basin (Köhl, 2007, Raj et al., 2015). These eddies have warm cores and locally yield positive anomalies in heat content, steric height and potential energy. Strengthening of the intensity of a generally dominating anticyclonic eddy, known as the Lofoten Eddy or Vortex (Søiland et al., 2016) can be important, but this appears to be linked to periods with a higher number of anticyclonic eddies entering the Lofoten Basin from continental slope. To point to this slightly different perspective of warming trend in the Lofoten Basin, we have added at the end of section (3.1):

“The broad-scale positive trend in heat content, steric height and potential energy in the Lofoten Basin, recorded in the time-averaged and space-interpolated hydrographic data, may reflect an increase in intensity and number of mesoscale anticyclonic eddies shed from the continental slope that propagate into the central basin (Köhl, 2007, Raj et al., 2015, Chafik et al.

2015). Higher influx of eddies from the slope can invigorate the long-lived dominating anticyclonic eddy (Köhl, 2007), known as the Lofoten Vortex, which has a strong local hydrographic signature and moves around in the central basin (Søiland et al., 2016). However, for the time-mean flow trend it does not matter if the warming in the Lofoten Basin is due to meso-scale eddy-induced or large-scale induced heat convergence."

Page 9. Regarding the connection to the upstream North Atlantic the authors interpret this as a disconnection between the North Atlantic and the Nordic Seas after 2005. The authors could also consider the interpretation that they follow with the Nordic Seas lagging the North Atlantic. That mean that the Nordic Seas in the year to come would experience a decreasing inflow temperature and subsequent decrease in ocean heat content. Please consider this.

Answer: We thank the Reviewer for this comment. Since our data stopped in 2016, we chose not to speculate about a possible lag. Instead, we focused on the possible mechanisms for this disconnection. However, we realize that doing as the reviewers suggests could have helped the interpretation of the disconnection. Analysis (not shown) of the recent years in fact indeed hints on a lag mechanism from the North Atlantic. We have now commented on this connection in the manuscript.

Minor comments:

The authors should limit the use of phrases as "key aspects", "definite similitude" with- out providing any statistical measure. When possible please quantify, and also prefer- able also include the effective number of freedom when claiming significance (e.g. Fig. 3).

Answer: We thank the reviewer for careful reading of our manuscript and take this into consideration in the revised version. The methods used to show significance are also added in the new version.

Clearly describe how the data are filtered before going into further analyses, how is annual cycle removed, in what way are results sensitive to methods.

Answer: This is extended in Section 2.

Page 8, line 23-24: Asbjørnesen et al (2018) used a volume-mean time-evolving tem- perature as reference.

Answer: It is true that Asbjørnsen et al. (2018) used a volume-mean time-varying temperature of the Norwegian Sea. However, they concluded that they got essentially the same results if they used 0 °C as a fixed reference temperature, which was used by Orvik and Skagseth (2005). To avoid possible misunderstandings we now instead site Orvik and Skagseth (2005) for the reference temperature.

In conclusion the authors repeat the main result both in the second paragraph and then "in the main findings". Once is better.

Answer: The Conclusions are revised. The list of “main findings” is removed, and the last item is moved to be included in the second paragraph. This paragraph will also be modified to better clarify the proposed answer to the question on remote or local forcing.

Conclusion two last paragraphs: I like the the point about the implication for the down- stream Atlantification. However, most of the remainder should go into introduction or discussion.

Answer: The paragraph starting at L20 is moved into section 3.1, to the discussion at the end where we have also added a paragraph on the Lofoten vortex (see above).

In general, when there are not strong arguments against it figures should show the same area.
Answer: Figures 1-3 and 5 are revised to show the same area.

Fig 1. Consider including depth contours on the map.
Answer: Done.

Figure 2b. This is probably not potential energy, but more a Baroclinic Transport func- tion. Change the title of figure. Define Sverdrup, Sv

Answer: The content of Figure 2 is changed and clarified. Figure 2 has been improved by also adding steric height trends. We now define Sv.

Figure 4. The time axes are different for a and b.

Answer: That is correct and was intentional. The hydrographic data used only covers the period from 1993-2016, and we still wanted to show the full altimetry data available. This could have been clearly stated in the figure caption. In the new version, the figure is modified so that the time axes are the same, but with the last data points empty for the hydrographic data, to avoid confusion.

Figure 6. Why do you use a 24-month running mean here?

Answer: A 24-month running is used to reflect inter-decadal variability; the focus of this study. For consistency, the same filter length is now also used in Fig. 4.

Figure 8. The figure caption is not clear. Assure clarity.

Answer: The caption is now revised. We thank the Reviewer for careful inspection.