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Interactive comment on "Mechanisms of the time-varying sea surface height and heat content trends in the eastern Nordic Seas" *by* Sara Broomé et al.

Anonymous Referee #1

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The paper investigates the variability of the of ssh and heat content in the "Atlantic" (Eastern) part of the Nordic Seas, the main connection of heat and salt to the Arctic Ocean. A main conclusion is that the decadal variability in this Atlantic domain can be explained by a model solely forced with the Atlantic inflow temperature variability and setting the time scale of the considered volume to some years. While this result might not appear surprising, the scaling discussion of the relative influence of the volume flux vs temperature effect on the heat content and ssh is interesting, novel and warrants publication. Also, the paper is well written and easy to follow. With this overall positive impression, there are some points listed below that I hope the author will consider.

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.

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?

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 transport 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?

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.

Minor comments:

The authors should limit the use of phrases as “key aspects”, “definite similitude” without providing any statistical measure. When possible please quantify, and also preferable also include the effective number of freedom when claiming significance (e.g. Fig. 3).

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.

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

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

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

In general, when there are not strong arguments against it figures should show the same area.

Fig 1. Consider including depth contours on the map.

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

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

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

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

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