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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.

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Overall assessment

This manuscript treats variations of ocean heat content and sea surface height in the Atlantic domain of the Nordic Seas. It has clear illustrations and includes thorough analyses of the data sets presented as well as a relevant conceptual model. I therefore feel that it has the potential to become an important addition to the literature on the Nordic Seas. There are, however, parts of the manuscript that seem weak. I therefore feel that major revisions are required before the manuscript can be accepted for publication in Ocean Science. Below, I first address my two main concerns with the manuscript and then list some details.

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Discussion paper

The causal link between ocean heat content and sea level height

The manuscript links changes in sea surface height, SSH, to changes in ocean heat content below each square meter, H, i.e. to steric height changes. This seems to be one of the main conclusions of the manuscript and is stated explicitly several times:

1. “the trend in SSH is to a first approximation caused by a uniform warming of the AW” (page 4, line 19).

2. “the steric height changes related to the variation in heat content is the main reason for the observed decadal changes in SSH trends” (page 10, line 31-32).

3. “the most plausible cause of changes in SSH and heat content decadal trends is a change of temperature of the Atlantic source waters entering the Nordic Seas over the Greenland–Scotland Ridge” (page 11, line 3-4).

4. “the main reason for the shift in decadal trends in the SSH is the steric height changes related to heat content.” (page 11, line 11-12).

That warming of ocean water causes expansion and thus increasing steric height is a well established fact, as long as salinity changes do not compensate too much. In the Atlantic water entering the Norwegian Sea, salinity variations have usually been parallel to temperature variations. So, there is compensation, but only partial. Thus, a warming of the Atlantic water is expected to give increased steric height. There is nothing new in that, so this cannot be one of the main conclusions of the manuscript. But, what then are the authors claiming? In spite of the many statements of this causal link listed above, it is not clear to me more precisely what they are claiming and how they justify their claim. The only justification I find for claiming that steric height changes (i.e., expansions/contractions) are the main cause of the SSH changes is Figure 4 and the discussion on it. This figure does show a qualitative correspondence between SSH and H for the defined domain, for the period 1993-2002 (although not really after that or on shorter time scales). To claim that steric height changes are the “main” cause of the SSH changes needs a quantitative justification as well, however. I therefore find it strange that there is no calculation of the steric height changes associated with the heat content changes. This should be easy to calculate from their hydrographic data set. Why does the right panel in Figure 2 show potential energy rather than steric height. It may well be that the potential energy "largely mirrors the trend in steric height (not shown)" (page 4, line 12), but this choice makes it difficult to make a quantitative comparison between the two panels in Figure 2 and verify that steric height changes are the "main" cause of the SSH changes. Personally, I doubt that there is a quantitative justification for this claim. Using the two trend lines for the 1993-2002 period in Figure 4, the ratio between SSH change and H change is: ΔSSH/ΔH $\approx$ 4·10-11 m3 J-1. I don't have the hydrographic data set used by Broomé et al., but using CTD data from a standard section in the Faroe-Shetland Channel, I found a high correlation (R > 0.97) between ΔSSH calculated as steric height and ΔH, but regression analyses gave ΔSSH/ΔH < 2·10-11 m3 J-1, i.e. only around half of that in Figure 4 or less. For a vertically homogeneous water column, it is easily seen that the ratio between steric height and energy changes is ΔSSH/ΔH $\approx$ α/cp where α is the isobaric expansivity and cp the specific heat per volume. Since α increases strongly with temperature, a ratio as high as implied in Figure 4, requires considerably warmer water than generally found in the (depth averaged) specified domain. But, more fundamentally: If Figure 4 is the justification, then the authors must imply that SSH changes in the specified domain are mainly caused by expansions/contractions within this domain. Why link SSH in the region to heat content in the region, otherwise? As argued above, there is some (not overwhelming) qualitative support for that but no quantitative justification. I doubt, however, that this can be their claim. Most of the water that was within the domain in 2002, was outside it in 1993 (probably west of the Iceland-Scotland Ridge). Thus, much of the expansion caused by warming from 1993 to 2002 will have occurred outside of the domain, perhaps in the southeastern boundary of the SPNA. This interpretation would be consistent with the statement in bullet point 3 above but, if they are really claiming that the SSH changes in the specified domain are mainly caused by expansions/-contractions upstream of the domain, then why use the local heat content in the domain (Figure 4)? Why not discuss heat content over a wider region upstream of the domain (which would be warmer and therefore have a ΔSSH/ΔH ratio more consistent with Figure 4)? But, then it would of course also be necessary to evaluate the effect of circulation changes (e.g., subpolar gyre). It is well known that steric effects (thermal expansion) are an important component of recent global sea level rise (e.g. IPCC). That does not imply that the warming in a small region, as the one treated here, is the main cause of sea level rise in that region as apparently claimed. As argued above, the results presented in this manuscript rather imply the opposite. One might argue that this is a question of semantics. As defined by Eq. (1), the steric height is a mathematical construct with a value depending on the reference density, Eq. (3). Establishing a mathematical relationship with another parameter (ocean heat in the specified region) is of course fully justified. The problem arises when words like “mechanism”, “cause”, and “reason” are used because they imply a causal physical relationship. From a physical point of view, the statement “the main reason for the shift in decadal trends in the SSH is the steric height changes related to heat content” (page 11, line 11-12) must mean: “the main reason for the shift in decadal trends in the SSH is the expansion/Âňcontraction due to temperature changes”. When SSH (which is a physical parameter; not a mathematical construct) is linked to steric height, it is linked to the physical mechanism of expansion/contraction and it has to be clearly stated where this mechanism operates. And justified based on that. The question of steric height variation in the Nordic Seas has been addressed by various authors as referred to in the manuscript. Nevertheless, I feel that the data presented in this manuscript may contribute to this topic. For that purpose, the authors need, however, to be more precise. If they want to maintain a strong causal link between steric height and SSH, they need to specify where the associated expansions/Âňcontractions have occurred and they must justify their claim quantitatively as well as qualitatively.

The conceptual model

The conceptual model in Sect. 3.2 is an appropriate component of the manuscript and helps justify the three last main findings as summarized on page 11. It raises a few questions, however:

Firstly, why use 700 m for the depth of the AW here (page 9, line 6), when 657 m is used elsewhere in the manuscript ?

Secondly, the last part of Eq. (14) defines τ in terms of the volume inside the chosen Atlantic domain, which you must have calculated (from Figure 1 and the arguments on page 8, line 10-12, it seems to be $\approx$ 5·1011 m2 · 657 m) and the volume transport. Using 5 Sv (page 8, line 10), this gives $\tau \approx 2$ years. I understand why you chose to use higher values, but it might be appropriate to include a sentence or two to justify this.

Thirdly – and most importantly – the arguments on page 8 for neglecting transport variations relative to temperature variations seem weak. With the uncertainties involved, the ratio 0.3/0.4 is hardly different from 1. Also, it would have been more appropriate to consider the ratio between the two driving terms in Eq. (12) rather than in Eq. (14). Then Eq. (15) would have ∆T' instead of Ti', which I assume would make the ratio closer to (or above ?) unity. To utilize this, you would, of course, need time series of volume transport in addition to temperature. From page 8, lines 28-30, you might already have this available from altimetry, but, if not, Figure 10 in Østerhus et al. (2019) provides a time series of Atlantic inflow to the Arctic Mediterranean and most of that enters between Iceland and Scotland i.e. into your Atlantic domain (Figure 9 in Østerhus et al. (2019)). As stated in your manuscript (page 8, line 27-28) this transport is highly stable on decadal time scales, but the observations do indicate an increase of at least 0.5 Sv from the mid-1990s to the early 2000s, i.e. in the period where you observe the largest increase in heat content. A back-of-the-envelope calculation indicates that including such an increase (followed by constant transport or the time series in Østerhus et al. (2019)) might give a considerably better fit than the one seen in the lower panel of Figure 6. In connection with this, the two sentences "Equation (14) is based on the reasonable assumption that the low-frequency ocean heat convergence is dominated by changes of the AW circulation" (page 8, line 4-5) and "...variations in temperature are slightly more important than variations in volume flow" (page 8, line 14) seem contradictory.

Details

Including both "time-varying" and "trends" in the title seems a bit of an overkill and makes the title ambiguous. Do you mean "time-varying trends" ? Perhaps rephrase the title.

In the text, the Nordic Seas are sometimes treated as plural (are/have) and sometimes as singular (is/has). I prefer plural, but in any case, choose one.

Page 1, line 5: can "slowdown" -> "weakening"

Page 1, line 21: "Chafik and Rossby (2019)" -> "(Chafik and Rossby, 2019)"

Page 2, line 8: "have" -> "has"

Page 2, line 13: "carry" -> "carries"

Page 2, line 24: "stagnant" -> "slowly-increasing"

Page 2, line 26: "variations" -> "variation"

Page 2, line 32-33: Do you actually use "Absolute" (rather than SLA) altimetry data ?

Page 3, line 6: Non-standard reference

Page 4, line 2: "have" -> "has"

Page 4, line 8: "dynamic sea surface height" -> "sea surface height"

Page 4, line 22: "extent that" -> "extent than that"

Page 4, line 29: "mean flow heat transport" ?????????

Page 5, line 17: "seasonal variation in heat content" -> "seasonal variation in heat content and wind forcing"

Page 6, line 19: "average" -> "averaged"

Page 6, line 29: "show" -> "shows"

Page 7, line 1: "data is" -> "data are"

Page 7, line 13-17: Defining the overbar parameters as "time-mean" (line 13) is inconsistent with Eq. (11) (line 15) before you neglect second order terms (line 17). I suggest to move this assumption up. Then Eq. (11) follows naturally and is not a "choice".

Page 11, line 18: "seem" -> "seems"

Page 11, line 18: "maintain" -> "maintains"

Page 14, line 16: Non-standard reference

Figure 3 and Figure 8: It is nowhere stated, how statistical significance is estimated, specifically whether it takes serial correlation into account. If it does take this into account, this should be stated (e.g. in the figure captions). If it does not, the significance should be re-calculated and the figure modified or the dots in Figure 3 and circles in Figure 8 should be removed as well as any reference to statistical significance in the captions and text.

The two panels in Figure 6 are labeled a) and b). In other two-panel figures, you use left/right or upper/lower. Be consistent.

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