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 noth- ing 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 quanti- tative 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: ∆SSH/∆H ≈ 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 ∆SSH calculated as steric height and ∆H, but regression analyses gave ∆SSH/∆H < 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 ∆SSH/∆H ≈ 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 in- terpretation 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 ∆SSH/∆H 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 ef- fects (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 ar- gued 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/Ânˇcontraction 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/Ânˇcontractions have occurred and they must justify their claim quantitatively as well as qualitatively.

Answer: We thank the referee for a detailed and insightful discussion on the physical link between ocean heat content and sea level.

The reviewer is correct in pointing out that it is well established that ocean warming causes increasing (steric) sea level height. We have modified the manuscript to more clearly convey that the novel results on this subject concerns decadal time scales and the importance of advection of temperature anomalies from the North Atlantic, which appears to be dominating over local air-sea fluxes on decadal timescales. Air-sea heat fluxes are important for inter-annual ocean temperature anomalies in the eastern Nordic Seas as have been shown in several previous studies.

The reviewer is correct in stating that the sea level cannot uniquely be divided in a steric height component and bottom pressure component as one has to select a somewhat arbitrary reference density. However, when one considers changes in the sea level, the steric changes can be tied to the observed changes in density, and this measure of steric height changes is essentially insensitive to the reference density (as long as it is taken to be a typical mean ocean value). In section 2.2, we now describe how the sea level can be partitioned into a steric height and a bottom pressure component and that changes in steric height are causally linked to the vertically-integrated changes in density.

This new and more careful derivation and discussion in section 2.2 should clarify that the steric height changes are tied to the local vertically-integrated changes in density (or buoyancy), which in turn can be due either to local air-sea fluxes or due to oceanic buoyancy convergence caused for instance by advection of upstream water with anomalous buoyancy. We believe that this answers the referee's question about the role of expansion caused by warming in the North Atlantic south of the Nordic Seas: this steric height signal is tied to the buoyancy anomaly of water advected northward.

The referee asks whether the thermal expansion due to the heat content changes can explain the altimetric sea level changes in Figure 4, and provide some back of the envelope estimates. We have now added the steric height variations calculated from the hydrographic data (EN4 data set provided by the UK Met Office) in Figure 4. This shows that the steric height and ocean heat content variations in the hydrographic data are strongly correlated (as expected) and that the steric height variations explain main parts of the inter-decadal variations in the altimetric sea level. We now also cite Chafik et al. (2019), who show that inter-decadal variation in the surface wind stress explain some of the sea level variation in the eastern Nordic Seas.

Also in Figure 2, we now show the trend in steric height calculated from the hydrographic data.

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 ≈ 5·1011 m2 · 657 m) and the volume transport. Using 5 Sv (page 8, line 10), this gives τ≈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 vari- ations 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 Øster- hus 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 indi- cates 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.

Answer:

Conceptual model

First point: 657 m is the depth in the hydrographic data base closest to 700. We now state this.

Second point: There is some degree of arbitrariness in defining the limits of the Atlantic Volume in the conceptual model. Taking the box indicated in Fig. 1 gives an area about $6x10^{11}$ m^2, which with H~700 m and a transport of 5 Sv, one obtains a time scale of 2.6 ≈ 3 years. On page 8 after discussing the residence time scale we have added a sentence which clarifies that, by taking these specific number for the Atlantic Water properties the residence time-scale defined in Eq. (14) gives value comparable to the ones estimated by for example Kozalka et al. (2013)

Third point: We agree with the referee that our scaling arguments based on the conceptual model do not firmly show that temperature anomalies are dominating over volume transport anomalies for driving Atlantic Water temperature anomalies in the Nordic Seas. Instead, given the qualitative nature of scaling arguments it is reasonable to conclude that temperature- and transport-anomalies are potentially of equal importance. On P8 L19 we have therefore changed "can be more important for ocean heat convergence" to "can be equally important for ocean heat convergence". However the important point (P8 L20), which we want to make clear, is that for heat transport through a section, volume variations completely dominates over temperature variations (Asbjørnsen et al., 2018). We stress this difference between heat transport and heat convergence since it is important for rationalising the simple scenario where only forcing from temperature anomalies in the conceptual model is considered.

To give a more detailed view 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. And 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) between in the early 2000:s.

Finally, we think the referee's point concerning contradictory statement in relation to Eq. (14) can be solved by writing: "Equation (14) is based on the reasonable assumption that the low-frequency ocean heat convergence is dominated by changes of the AW circulation, *rather than air-sea heat fluxes*"

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.
Answer: Yes, we do mean "time-varying trends", but did not realize this wasn't clear. The title will be changed to: "Mechanisms of the time-varying trends in sea surface height and heat content in the Nordic Seas"

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.

Answer: We thank the Reviewer for pointing this out.

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

Answer: Done.

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

Answer: Done.

Page 2, line 8: “have” -> “has”

Answer: Done.

Page 2, line 13: “carry” -> “carries”

Answer: Done.

Page 2, line 24: “stagnant” -> “slowly-increasing”

Answer: Done.

Page 2, line 26: “variations” -> “variation”

Answer: Done.

Page 2, line 32-33: Do you actually use “Absolute” (rather than SLA) altimetry data ?
Answer: Yes.

Page 3, line 6: Non-standard reference

Answer: Fixed.

Page 4, line 2: “have” -> “has”

Answer: Done.

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

Answer: OK.

Page 4, line 22: “extent that” -> “extent than that”

Answer: Thank you. Done.

Page 4, line 29: “mean flow heat transport” ?????????
Answer: Changed to: “Heat transport by the mean flow”

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

Answer: OK.

Page 6, line 19: “average” -> “averaged”

Answer: Done.

Page 6, line 29: “show” -> “shows” Page 7, line 1: “data is” -> “data are”

Answer: Done.

Page 7, line 13-17: Defining the overbar parameters as “time-mean” (line 13) is incon- sistent 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”.

Answer: This is adjusted.

Page 11, line 18: “seem” -> “seems”

Answer: Done.

Page 11, line 18: “maintain” -> “maintains”

Answer: Done.

Page 14, line 16: Non-standard reference

Answer: Fixed.

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 ac- count, 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.

Answer: For Fig. 3, two-sided p-values are calculated using the Wald test, which is provided by the SciPy linregress routine (Oliphant, 2007). The significance test in Fig. 8 will be revised to take into account the effective number of degrees of freedom.

*Oliphant, T. E. (2007). Python for scientific computing. Computing in Science & Engineering, 9(3), 10–20. doi: 10.1109/MCSE.2007.58

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.

Answer: We thank the reviewer for this. Done.