Dear sirs,

We would like to thank the reviewers for their comments to the manuscript.
We resubmit three documents:

- The answer to the reviewers (the present document)
- The reviewed manuscript with the track changes
- The reviewed manuscript without the track changes

In the answers to the reviewers, we also provide the part of the manuscript that has been modified with the corresponding line numbers. The line numbers refer to the manuscript without track changes.

During the revision process, we have noticed that the significant wave height in the ALES-reprocessed Sentinel 3a and 3b datasets does not contain real values, but only “nans”. This is a problem since, following the ALES’ documentation, we have used the condition "SWH<11m" to discard the outliers from the ALES dataset. Because the "nans" never satisfy this condition, we have not included Sentinel 3a and 3b in our analysis.

To address the problem, we have modified the manuscript and deleted all the references to the Sentinel 3a and 3b datasets. Moreover, we have removed the Sentinel 3a and 3b from the conventional altimetry dataset to allow for a fair comparison with the ALES-reprocessed dataset, and we have produced a new version of Figures C1, C2, C3, C4 and C5. However, neglecting Sentinel 3a and 3b does not alter the results of our conclusions probably due to the short overlap of these two satellite missions with the period covered in our study (circa thirteen months for Sentinel 3a and circa one month for Sentinel 3b).

Reviewer 1

I would like to thank the authors for providing a satisfying revisions. I have only a couple of minor points left.

1.
Concerning my two former major comments, I think they are mostly answered. It is good that the authors are now transparent in describing the way in which they define the effective degrees of freedom and the uncertainties, although I must say I am not fully familiar with it.

I am still a bit puzzled by the quality of the fitting of the variogram in some stations compared to others, for example in Figure A2 the fitting of Honnigsvag and Vardo (last two locations) showing the same lags as the fitting of Stavanger, Bergen, Maloy, Alesund and Kristiansund in the second line. But then, compared for example to the much stronger autocorrelation seen in the Appendix B, probably this suboptimal fitting is not that problematic and does not modified the conclusions very much, as the authors say. I’d appreciate a comment of the authors for my own knowledge about that, but I am not asking a revision on this point.

The experimental semi-variograms do indeed reveal different structures in different areas. There are oscillations in some Northern stations due to residual oscillations in the signal, which come out superimposed with the overall exponential structure of the semi-variogram. The Honningsvåg and Vardø stations also exhibit a slower time scale of about 12 months or more.
In both cases the amplitude of these slow signals is much smaller than the variance (about 10%) so we are confident that a more complex semi-variogram fit (nested exponentials + trigonometric functions) would not have changed the results much.

What I do ask, instead, is to be clear about what references have been used to apply this methodology (or methodologies) concerning Appendix A. No references are cited right now.

It is true. We have now added the following reference:

Wackernagel (2003). Multivariate Geostatistics (3rd ed.). Berlin: Springer Verlag.

to Section 3.1 of the manuscript, where we first introduce the semi-variogram as a way to compute the effective number of degrees of freedom of a time series.

And, secondly, I think the caption of the figures is wrong. For example in Figure A1 you write: “variogram of the difference between the detrended and deseasoned SLA estimated from the ALES-retracker satellite altimetry (empty circles)”. But this is not a variogram of the differences, it’s a variogram of the the detrended and deseasoned SLA, isn’t it? The only variogram “of the differences” is in Figure A3. Please double check.

We would like to thank the reviewer for noticing the error in the captions of Figures A1 and A2. Yes, they show the semi-variograms of the detrended and deseasoned SLA estimated from the ALES-retracked satellite altimetry (Fig. A1) and from the tide gauges (Fig. A2). We have corrected the captions in the revised version of the manuscript.

2.

Concerning my previous comment “Nevertheless I don't get one point: why such strict thresholds and not more simply iterating the two distances for each tide gauge selecting each time the distances that yield the best correlation?”, the added paragraph looks still confusing to me.

So, let’s take the distance “along coast”. You write “we test different combinations...between 20 and 200 km”. Which different combinations? Every 20 km? Every 50?

Then you write: “we have performed a sensitivity test on the distance from the tide gauge allowing it to range between 15 and 400 km: as before, we have found little difference in the final results.”

Then you write: “Indeed, the maximum values of the linear correlation coefficients occur for distances along the coast that range between 140 and 200 km, with them being 200 km at 13 out of 22 tide gauges.”. So, if you get the best correlation using 200 km (the maximum value of your threshold), does the correlation get better if you use, say, 300 km?

What I mean is that all these paragraphs put together are confusing for the reader. I appreciate you want to keep your methodology, but could you write it in a coincise way in one single paragraph, maybe with a formula that helps the reader to reproduce the procedure?

We thank the reviewer for the suggestion. We have shorten the paragraph describing the procedure we used to produce the monthly-averaged sea-level time series from the ALES-reprocessed satellite altimetry dataset. The paragraph now reads:

(Lines 261-273) *During the process, we verify that the selected altimetry observations represent the sea-level variability at each tide gauge location. More precisely, since tide gauges represent the sea-level variability along a stretch of the coast, we monthly average all the altimetry observations within a certain radius “r” from the tide gauge and a certain distance “d” from the coast (Fig. 3). We try different combinations of d and r by allowing the first to range between 5 and 20 km, with steps of 2.5 km, and the second between 20 and 200 km, with steps of 15 km. Then, we pick the combination that maximizes the linear correlation coefficient between the detrended and deseasoned SLA measured by satellite altimetry and by the tide gauge (as, for example, in Cipollini et al., 2017). To set the maximum values of d and r at 20 and 200 km respectively, we have first performed a sensitivity test and noted that larger values of d and r return slightly higher linear correlation coefficients (especially in northern Norway), but do not alter the main results of this study. At the same time, a maximum distance of 20 km from the coast and of 200 km from the tide gauge ensures that all the selected altimetry points are located over the continental shelf and that we can better capture the spatial variability of the seasonal cycle of the sea level and of the sea-level trend.*

Moreover, we have added a sketch (new Fig. 3) to help the reader better understand the procedure.

3.
Concerning my previous comment related to the sentence “These results suggests that the detrended and deseasoned SLA in the south vary over smaller spatial scales
compared to the north.” I do not find the answer fully satisfactory. You have added a general comment, (which I comment later), but you have not answered on whether
you think that the location of the tide gauge plays a role. Please look for example at the values shown in Figure 5, concerning the tide gauge 12. Why is the standard
deviation of the correlation coefficients and of the RMSD very high at that point? Is it anything to do with the fact that this tide gauge is located in a fjord or not? In other
words: do you think that the spatial scales of the detrended and deseasoned SLA are fully independent of the conditions in which the tide gauge is located? (open sea, vs
fjord, vs ....)

We would like to thank the reviewer for the comment. The location of the tide gauges can indeed be an issue since the quality of satellite altimetry measurements can decrease in sheltered coastal areas, such as fjords, where patches of calm water prevent the area illuminated by the satellite altimeter to have a uniform backscatter coefficient. We expect this issue to explain, for example, the high standard deviation of the linear correlation coefficient and of the RMSD at Trondheim, which is located in the Trondheim fjord, and in Oslo and Oscarborg which, instead, are located in the Oslofjord.

We have now modified the manuscript by removing the two paragraphs that introduced and described Figure 5 in the submitted version of the manuscript with the following one:

(Lines 375-385) *Figure 6 supports our previous conclusions on the relationship between satellite altimetry and the tide gauges at Trondheim, Oslo and Oscarborg. In Figure 6, we show, for each tide gauge, the standard deviation of the linear correlation coefficient and of the RMSDs over all the possible combinations of the distance from the coast and from the tide gauge to measure the geometrical uncertainty of the SLA estimates from satellite altimetry. We find that, at Trondheim, both the linear correlation coefficient and the RMSD depend more on the size of the selection window when compared to other regions of the Norwegian coast. Similarly, at Oslo and Oscarborg, we note an anomalously high standard deviation of the linear correlation coefficient only. We expect anomalously high values of the standard deviation of the linear correlation coefficients and RMSDs because these three tide gauges are in sheltered areas (Trondheim in the Trondheim fjord, whereas Oslo and Oscarborg the Oslofjord) which can favour the formation of patches of calm water and negatively affect the quality of the satellite altimetry observations.*

We would like to add that a preliminary result, obtained during the revision process, suggest that the standard deviation of the linear correlation coefficients and of the RMSDs at each tide gauge location might also depend on the satellite mission under study. However, we could not disentagle any systematic effect of coast configuration or impact of a single mission. So we believe that there is a combined effect of the coast and of the differences between the selected missions. Being this only a preliminary result, we have preferred not to add it to the manuscript. Instead, we might further investigate this issue in a future work.

4.
Line 738: “a few thousands of kms”...you mean a few hundreds?

We would like to thank the reviewer for the comment. Yes, we meant “hundreds”, not “thousands”. We have modified the manuscript accordingly.

5.
Lines 741-742 “we reduce the noise in the SLA from altimetry which might result, for example, from the rough topography of Norway”. I don’t see why the rough
topography of the land would create noise in a sea level dataset? Please clarify, but I tend to believe that the reason why the “noise” is reduced is that by averaging over

long distances, expecially along a coherent coast, you include more altimetry tracks and therefore you improve the temporal sampling from altimetry. This has been demonstrated, for example, with the concept of the “Zone of Influence” in Oelsmann et al., 2021 (https://doi.org/10.5194/os-17-35-2021).

We thank the reviewer for the comment. It is true, as we average over more satellite altimetry observations, we increase the temporal sampling of our measurement, especially along a coherent coast as the Norwegian one. We have implemented the change.

(Lines 651-659) *Because the detrended and deseasoned SLA pattern is coherent over large distances along the Norwegian coast (see also Chafik et al., 2017), coastal altimetry observations located a few hundred kilometres apart can be representative of the sea-level variations occurring at a particular tide gauge location. This explains why we can average the SLA from altimetry over an area a few hundreds of kilometres wide around each tide gauge location to maximize the linear correlation coefficient between the detrended and deseasoned SLA from satellite altimetry and the tide gauges (Section 3.2). Moreover, it also partly explains the good agreement between satellite altimetry and tide gauges since, as we average over a large number of satellite altimetry observations, we increase the temporal sampling provided by altimetry and, therefore, we reduce the noise in the resulting SLA (Oelsmann et al., 2021).*

6.
Appendix C: this is a useful comparison, but please make sure to write in the lines that these results are not only influenced by the retracker of choice, but also by the set of geophysical corrections applied to the retracker, which surely have differences between OpenADB and your source of “conventional altimetry”. I appreciate the fact that this addition is in an Appendix and does not need to be the core of the article, since the use of ALES and its advantages with respect to conventional retracking have been validated in several other papers nowadays.

We would like to thank the reviewer for the comment. It is true the different geophysical corrections might also explain the difference between the ALES-retracked satellite altimetry dataset and the conventional altimetry. To address this comment, we have added a sentence to the text in Appendix C:

(Lines 824-825) *even though we should remember that the discrepancy between the two datasets might not only result from the different retrackers, but also from the different geophysical corrections applied to the data.*

Reviewer 2

1.
Line 24. A hyphen is not required here. Generally, the term “sea level” only needs to be hyphenated when it is used as an adjective before a noun.

We thank the reviewer for the comment. We have modified the manuscript accordingly.

2.
Line 25. I suggest: “Accurate estimation and attribution of…”

We thank the reviewer for the suggestion. We have modified the manuscript accordingly.

3.
Lines 24-29. I think that the argument for this study could be made more convincingly in this first introductory paragraph by emphasizing the societal impacts of

understanding local sea level. For example, the authors start the introduction by saying that "Sea level is considered a key indicator to monitor the earth's energy imbalance and climate change", but this point is not even relevant to the work presented here because it is global mean sea level that is a key climate indicator rather than local sea level. I would say that the motivation for this study is that changes in local sea level can considerably differ from global mean sea level, and hence understanding such local changes is crucial to the implementation of coastal adaptation plans.

We would like to thank the reviewer for the comment. We have modified the first paragraph of the Introduction, which now reads:

(Lines 26-35) *Global mean sea level (GMSL) has been rising during the XX century and the beginning of the XXI century at a rate of approximately 1.5 mm year^{-1} (Frederikse et al., 2020). Its rise is projected to continue, and even accelerate, in the future (Hermans et al., 2021), thus posing significant stress on coastal communities around the globe (Nicholls, 2011). At a local scale, though, sea-level variations can largely deviate from the global average (Stammer et al., 2013). Therefore, accurate estimation and attribution of sea-level rise at regional scale are major challenges of climate research (Frederikse et al., 2018) with large societal benefit and impact due to the large human population living in coastal areas (e.g., Lichter et al., 2011). The Norwegian coast is no exception. While it appears little vulnerable to sea-level variations because of its steep topography and rocks resistant to erosion, it has a large number of coastal cities, most of which have undergone significant urban development in recent times (Simpson et al., 2015).*

4.
Lines 52-53. Here I would add a clarification along the lines of "typical of the open ocean for distances to the coast of about 3 km".

We thank the reviewer for the comment. We have added "for distances to the coast of up to 3 km circa" to the end of Line 53.

5.
Line 65. You do not really assess the "sea-level budget". I would suggest you reword this sentence.

We thank the reviewer for the suggestion. We have replaced "sea-level budget" with "the steric contribution to the sea-level variability in the region".

6.
Lines 104-105. Could you please specify here exactly which tidal corrections have been applied? This is important because, while tide gauges only sense the ocean tide and the ocean pole tide, altimeter data are also influenced by the solid earth tide, the load tide, the solid earth pole tide, etc.

We thank the reviewer for pointing out the lack of detail here, and we have added a specification of which tidal corrections have been done to the altimetry data, and which of these we apply to the tide gauge data in order for the data sets to be comparable.
The entire paragraph now reads:

(Lines 114-131) *Prior to distribution, several corrections have been applied to the satellite altimetry data. Among them, the geophysical corrections are of particular interest for the purpose of this study. Indeed, to validate the ALES-reprocessed altimetry against the Norwegian tide gauges, the same physical signal must be removed from both datasets. The geophysical corrections applied to the ALES-reprocessed altimetry data include the tidal and the dynamic atmospheric corrections (COSTA user manual, http://epic.awi.de/43972/1/User_Manual_COSTA_v1_0.pdf).The correction for ocean and pole tides has been performed using the EOT11a tidal model. The solid Earth related tides have also been subtracted from the orbital altitude but, as it leaves the altimetry data in sync with the tide gauges (which are based on the solid*

Earth), this correction has no further interest for this study. The dynamic atmospheric correction (DAC), available at https://www.aviso.altimetry.fr/index.php?id=1278, removes both the wind and the pressure contribution to the sea-level variability at timescales shorter than 20 days, and only the pressure contribution to the sea-level variability at longer timescales. The high-frequency component of the DAC is computed using the Mog2D-G High Resolution barotropic model (Carrère and Lyard, 2003), and it is removed because it would otherwise alias the altimetry data. The low-frequency component accounts for the static response of the sea-level to changes in pressure, a phenomenon also known as inverse barometer effect (IBE), and according to which a 1 hPa increase/decrease in sea-level pressure corresponds to a 1 cm decrease/increase in sea-level. To validate the ALES-reprocessed altimetry against the Norwegian tide gauges, the relevant physical signals at the relevant time scales must be removed from the tide gauge data (Section 2.2).

7.
Section 2.1. Altimetry measurements are influenced by GIA, albeit differently from tide gauge data. Yet there is no mention of any GIA corrections applied to the altimetry data, even though tide gauge records appear to have been corrected for GIA (both crustal uplift and gravity contributions).

We thank the reviewer for pointing out this error in our descriptions. We have indeed only corrected the TG data for the VLM part of GIA. The text has been modified accordingly on that point:

(Lines 170-174) *Along the Norwegian coast, the GIA affects the sea-level reading from the tide gauges because it induces a vertical land movement (VLM) and, to a lesser extent the sea level itself, because it modifies the earth's gravity field. The first effect has been corrected using both GNSS observations and levelling, whereas the second has not been corrected since the satellite altimetry data are also influenced by geoid changes (Simpson et al., 2017).*

8.
Lines 145-147. "lesser extent". In some regions, the gravity GIA contribution is larger than the VLM induced by GIA. Perhaps, you could clarify that in Norway VLM is the dominant GIA contribution.

We would like to thank the reviewer for the suggestion. We have now specified that our statement is true along the Norwegian coast (the modified text has already been shown in the reply to the previous comment).

9.
Lines 168-176. Could you please specify the temporal resolution of the hydrographic data?

We would like to thank the reviewer for the comment. We have now modified the paragraph accordingly:

(Lines 208-214) *We select the temperature and salinity profiles taken between January 2003 and December 2018 for them to overlap with the period covered by the ALES-reprocessed altimetry dataset. Data are irregularly sampled, being them mostly collected once every one or two weeks. To allow a comparison with the satellite altimetry dataset, we have monthly averaged the temperature and salinity profiles at each hydrographic station. We should note that the monthly-averaged time series of temperature and salinity contain missing values (Fig. 2). Bud has the largest number of missing values, with 76 gaps out of 192. It is followed by Indre Utsira and Ytre Utsira, with 44 and 41 gaps, respectively. The remaining hydrographic stations have less than 16 gaps each.*

10.
Line 195. While there is nothing wrong with this nonlinear regression model, the model can be easily written in linear form, which greatly simplifies the estimation of the

regression coefficients and their uncertainty: z(t) = a + b*t + c*cos(wa*t) + d*sin(wa*t) + e*cos(was*t) + f*sin(was*t), where "wa" and "was" are the annual and semiannual angular frequencies. I am not suggesting the authors redo the analysis with the linear model, but I thought it was worth pointing this option out.

We would like to thank the reviewer for the simplification of the formula. But the sines and cosines still make the equation nonlinear in t and justify the use of the Levenberg-Marquard optimisation.

11.
Line 203. "depend".

We apologize for the typo. We have corrected it in the revised version of the manuscript.

12.
Line 212. This equation appears three times in the manuscript. I would suggest you write this equation only once, possibly in the Appendix, number it, and then refer to it whenever you use it.

We thank the reviewer for the suggestion. We have now modified the manuscript: the formula that we use to compute the confidence intervals of the linear trends only appears in Appendix A (formula 7). We refer to this formula when we use it in the main text and in appendix B.

13.
Line 268. As written, Equation (3) is incorrect. In particular, it is missing a minus sign. Assuming a standard definition for the haline contraction coefficient (beta = 1/rho * drho/dS), the equation as it stands implies that sea level increases as salinity increases, which is not correct. I assume that this is just a typo and that the halosteric calculations have been done properly, otherwise the authors need to adjust their results.

We apologise for the mistake. We have now added the minus sign to Equation (3) in the text. Yes, it is a typo. The python script that we wrote to compute the halosteric component of the sea level includes the minus sign.

14.
Lines 281-284. I do not understand what the authors mean here. Could you please clarify?

We thank reviewer for the comment. We have modified the paragraph accordingly:

(Lines 314-318) *We do not use the harmonic analysis approach to estimate the sea linear trend and the seasonal cycle of the SLA and of the thermosteric, halosteric and steric components of the sea level at each hydrographic station. Instead, we use simple linear regression to estimate the linear trend and we compute the monthly climatology of each detrended time series to estimate the corresponding seasonal cycle. We prefer this procedure. Indeed, the seasonal cycle of the SLA, and of the thermosteric, halosteric and steric sea level might depart from the linear combination of the annual and the semi-annual cycles.*

15.
Line 395. The term used in Benveniste et al. (2020) is "fractional differences".

We have corrected the typo in the revised version of the manuscript.

16.

Lines 395-399. It is not clear how the authors compute the FDs. In Benveniste et al. (2020), "tau" is defined as the trend of the time series of sea-level differences. That is,
Benveniste et al. (2020) subtract the altimetry time series from the tide gauge time series, and then compute the trend of the residual time series. Here, the authors define
"tau" as "the linear trend difference between altimetry and each tide gauge", which seems to imply that "tau" is the difference of the two trends from the tide gauge and
altimetry. If this is how you do it, what value do you use for the standard error (SE) in the FD equation? The whole point of defining "tau" as the trend of the residual time
series as in Benveniste et al. (2020) is that the value of SE is straightforward to calculate. When you define "tau" as the difference of two trends, as opposed to the trend of
sea-level differences, then you need to make assumptions about the dependence between SE_TGs and SE_altimetry. Please clarify.

We thank the reviewer for noting it. "Tau" is the linear trend of the difference between the SLA estimated from satellite altimetry and tide gauges. In other words, at each tide gauge location, we first calculate the difference between the SLA from satellite altimetry and from the tide gauge. The, we estimate the linear trend of the residual time series.

In the revised version of the manuscript, we have written:

(Lines 427-429) *Fractional differences are defined as* $FD = |\tau|/(t_{0.05/2} \cdot SE \cdot \frac{N}{N^*})$*, where* $|\tau|$ *is the absolute value of the linear trend of the SLA difference between altimetry and each tide gauge.*

17.
Lines 399-400. From Fig. 8, the confidence intervals overlap significantly, even at Tregde, Måløy, and Bergen, so I am surprised that FDs are > 1 at those stations.

We thank the reviewer for the comment. Tregde has the largest linear trend differences among the tide gauges stations, whereas Måløy has one of the largest. At the same time, the standard errors associated with the linear trend differences at these two stations is lower than the average. This results in a fractal difference (FD) of 1.91 for Tregde and of 1.59 for Måløy. Other stations, such as Oscarborg and Oslo, are also characteriezed by large linear trend differences. However, their large temporal variability leads to large standard errors which, in turn, reduce the fractional difference to less than one.
Bergen resembles the case of Tregde and Måløy. However, its FD exceeds unity by only 0.05 digits.

18.
Section 5.2. This section is called "Steric contribution to the sea-level trend". However, the authors do not really place the steric contribution in the context of the sea-level
trends. They simply say that the steric trends lie within a particular range and then refer to Fig. 11. I would expect the authors to say what fraction of the sea-level trends
is explained by the steric contribution, distinguishing between stations. This Section could benefit from a more focused discussion of the results.

We would like to thank the reviewer for the comment. To address it we have modified the second paragraph of Section 5.2, which now reads:

(Lines 496-506) *Over the period 2003-2018, we find that the linear trends of the thermosteric, halosteric and steric components of the sea level approximately range between -1.0 and 2.5 mm year*$^{-1}$*. The steric contributions to coastal sea-level trends experience a large spatial variability, with it being even negative at Sognesjøen and reaching a peak of approximately 55% of the sea-level trend estimated from satellite altimetry at Lista and Ingøy. Moreover, when we compare the thermosteric and the halosteric signals at these locations, we note that the latter contributes more than the former to the coastal sea-level trends (up to 55% of the sea-level trend from altimetry). The width of the confidence intervals of the thermosteric, halosteric and steric contributions ranges between 4.0 and 12.0 mm year*$^{-1}$ *circa, with northern Norway exhibiting larger* *uncertainties (Fig. 12). This is a result of the high inter-annual variability of the thermosteric and the halosteric components in the region (Figs. B1 and B4), which leads to a fewer effective number of degrees of freedom and, therefore, to less accurate estimates of the linear trend.*

19.
Line 637 (Appendix A). Strictly speaking, this is the semi-variogram.

We would like to thank the reviewer for pointing this out. We have now modified the manuscript.

20.
Lines 649-651. How do estimates of the degrees of freedom (dof) computed with this approach compare to estimates computed using the equation: dof * (1 – r)/(1 + r),
where 'r' is the autocorrelation? Providing such a comparison would give the reader additional confidence in your uncertainty estimates.

We would like to thank the reviewer for the suggestion. We have also computed the autocorrelation of the:

1. detrended and deseasoned SLA from ALES
2. detrended and deseasoned SLA from tide gauges
3. detrended and deseasoned SLA difference between ALES and the tide gauges
4. detrended and deseasoned thermosteric component of the sea level
5. detrended and deseasoned halosteric component of the sea level
6. detrended and deseasoned steric component of the sea level

The formula $N^* = \frac{1-r_1}{1+r_1}$, where r_1 is the lag-one autocorrelation (Bartlett, 1935) returns, depending on the tide gauge or on the hydrographic station under consideration, an equal or a higher number of degrees of freedom when compared to the variogram approach. The case for the detrended and deseasoned SLA difference between ALES and the tide gauges is an exception: the autocorrelation approach returns a fewer number of degrees of freedom compared to the variogram approach. However, this does not alter our conclusions on the fractional differences. Moreover, we prefer using the variogram approach to determine the effective number of degrees of freedom of the detrended and deseasoned SLA difference between ALES and the tide gauges because it is more robust since it fits all values of autocorrelation instead of the lag-1 only.

In the tables below, we show the ratio between the effective number of degrees of freedom provided by the formula $N^* = \frac{1-r_1}{1+r_1}$ and by the variogram approach for each tide gauge (Table R1) and for each hydrographic station (Table R2).

	SLA from ALES	SLA from tide gauges	SLA difference between ALES and the tide gauges
Viker	1.5	1.7	1.8
Oscarborg	1.4	1.7	1.7
Oslo	1.6	1.2	1.7
Helgeroa	1.5	1.6	0.9
Tregde	1.6	1.3	0.7

Stavanger	1.5	1.3	0.7
Bergen	1.4	1.2	0.9
Måløy	1.4	1.4	0.8
Ålesund	1.4	1.4	0.9
Kristiansund	1.3	1.4	1.0
Heimsjø	1.2	1.3	0.9
Trondheim	1.2	1.0	4.8
Rørvik	1.2	1.1	0.9
Bodø	1.4	1.0	0.9
Narvik	1.3	1.0	1.8
Kabelvåg	1.5	1.0	0.8
Andenes	1.2	1.2	0.7
Harstad	1.3	1.1	0.7
Tromsø	1.2	1.2	1.0
Hammerfest	1.2	1.0	0.6
Honningsvåg	1.2	1.1	1.0
Vardø	1.2	1.2	0.9

Table R1: for each tide gauge location, ratio between the effective number of degrees of freedom estimated using the formula $N^* = \frac{1-r_1}{1+r_1}$ and the variogram approach. The column to the left refers to the detrended and deseasoned SLA from the ALES, the central column to the detrended and deseasoned SLA from the tide gauges, and the column to the right to detrended and deseasoned SLA difference between ALES and the tide gauges.

	Thermosteric sea level	Halosteric sea level	Steric sea level
Lista	1.7	2.1	3.4
Indre Utsira	1.5	2.2	2.4
Ytre Utsira	1.4	1.5	2.1
Sognesjøen	2.7	2.2	5.3
Bud	1.6	1.7	3.1
Skrova	2.6	1.6	4.4
Eggum	2.2	2.0	4.2
Ingøy	1.2	2.3	1.7

Table R2: for each hydrographic station, ratio between the effective number of degrees of freedom estimated using the formula $N^* = \frac{1-r_1}{1+r_1}$ and the variogram approach. The column to the left refers to the detrended and deseasoned thermosteric component of the sea level, the central column to the detrended and deseasoned halosteric component of the sea level, and the column to the right to detrended and deseasoned steric component of the sea level.

To address this comment, we have modified the manuscript as follows:

(Lines 244-252) *To compute the 95% confidence interval of the linear trends, we then use formula (7) in appendix A. Together with the semi-variogram, we also estimate the effective number of degrees of freedom using the formula $N^* = \frac{1-r_1}{1+r_1}$, where N is the length of the time series and N^* is its lag-1 autocorrelation (Bartlett, 1935). However, in this paper, we opt for the more stringent approach and only present the confidence interval derived using the semi-variograms. Indeed, we find that the semi-variogram approach returns either the same or fewer number of degrees of freedom (not shown) when compared to the other method. This is not the case for the effective number of the degrees of freedom of the detrended and deseasoned SLA difference between ALES and the tide gauges. However, we find that the choice of the approach does not alter our conclusions.*

21.

Abstract. The authors write "we find that the sea-level annual cycle is more affected by variations in temperature than in salinity, and that both temperature and salinity give a comparable contribution to the sea-level change along the entire Norwegian coast". I find this sentence a bit confusing. Seasonal variations are "sea-level changes", so what do you mean by "contribution to sea-level change"? Could you please rewrite this important sentence to improve clarity?

We would like to thank the reviewer for the comment that has made the manuscript clearer. Instead of "contribution to sea-level change", we should have written "contribution to the detrended and deseasoned sea-level variability". We have modified this sentence accordingly in the revised version of the manuscript.