The manuscript is a very good contribution towards the use of coastal altimetry from ALES for local sea level trend and sea level budget assessments. For this reason, the scope of the article goes beyond the regional character of the study. While I strongly encourage the publication of this study, I must assign a major revision due to two main points that the authors will find specified in the attached document alongside other minor comments:

1) The insufficient description of the method to assess trends and in particular about the computation of the uncertainty, which raises the suspect that a fundamental issue such as serial correlation in geophysical time series is not being taken into account. The absence of such a description negatively affects the possible value of the results and the discussion about statistical significance.

We would like to thank the reviewer for the comment which led to a modification of the manuscript.

To determine the effective number of degrees of freedom, we performed and analysed the variograms:

- of the detrended and deseasoned sea level anomaly (SLA) from the ALES-retracked satellite altimetry dataset.
- of the detrended and deseasoned SLA from tide gauges.
- of the detrended and deseasoned SLA difference between the ALES-retracked satellite altimetry dataset and the tide gauges.
- of the detrended and deseasoned SLA and of the thermosteric, halosteric and steric sea-level at each hydrographic station.

We preferred the variogram to the classical autocorrelation function as the latter tends to mask the longer time scales of variability. Variograms make a more honest and more stringent test of autocorrelation.

The reviewer will find the variograms and the corresponding analysis in Appendices A and B of the paper. The reviewer will also find a new version of Figs. 8, 9 and 11, where the confidence intervals have been recalculated to account for the effective number of degrees of freedom (number of data points divided by the time scale of autocorrelation). We used the effective number of degrees of freedom to also recompute the fractal differences (FD) in Section 4.3.

This modification only little altered our conclusions on the validation the first part of the paper. Indeed, we have found that the SLA difference between the ALES-retracked satellite altimetry dataset and the tide gauges is statistically different from zero at only 3 out 22 tide gauge locations: Tregde, Måløy and Bergen (versus 6 stations in the previous version).

On the contrary, it partly modified our conclusions on the second part of the paper. Because of the large uncertainties, we have found that we need longer time series to assess the thermosteric, halosteric and steric contributions to the sea-level trend at the hydrographic stations' locations (in the text, we only give a qualitative assessments of these contributions to the sea-level trends).

2) The several missing points and abundance of speculations in the discussion, in particular concerning the "sea level budget", which actually cannot be defined as such, since it does not provide any assessment of the components other than the steric ones. To solve this issue, the authors should either expand the discussion with the necessary analysis or reduce the aims and ambitions of the paper for what concerns Section 5

We would like to thank the reviewer for the comment, which led to a substantial modification of the second part of the manuscript.

We now state that we only determine the thermosteric, halosteric and steric contributions to the sea-level variability while assessing the synergy between satellite altimetry and tide gauges along the Norwegian coast.

We have modified the second part of the paper as follows:

- (Lines 474 - 503) We present the linear trend of thermosteric, halosteric, and steric components of the sea-level at each hydrographic station. We show the results with the corresponding confidence intervals and, given the large uncertainties, we only qualitatively assess the contribution of both temperature and salinity to the sea-level trend along the Norwegian coat between 2003 and 2018.
- (Lines 586 - 626) We present the empirical seasonal cycle of the thermosteric, halosteric and steric components of the sea level at each hydrographic station in contrast to the more large-scale approaches in the previous literature (e.g., Richter et al., 2012). We also assess their contribution to the seasonal cycle of the sea-level at each hydrographic station.

In both cases, we find that the relative thermosteric, halosteric and steric contributions to the sea-level trend and to the seasonal cycle of sea-level at each hydrographic station depends very little on whether we use the SLA from satellite altimetry or from tide gauges.

Richter, K., Nilsen, J. E. Ø., and Drange, H.: Contributions to sea level variability along the Norwegian coast for 1960-2010, 117, https://doi.org/10.1029/2011JC007826, 2012.

I think both the major and the minor issues highlighted in my review can be solved with a bit more work and possibly some additional analysis. Therefore I am willing to consider a revised version.

Minor comments highlighted by the first reviewer on the text.

Is the density of the altimetry tracks along the norwegian coast much better? particularly considering the shortness of their time series compared to a tide gauge network? I would rather highlight other advantages, for example:

1. Altimetry provides an assessment of absolute sea level, without the need to correct TGs for VLM

2. Tide Gauges in Norway are often located in very sheltered bays. It has been shown that sea level trends can differ significantly within very few kms from the coast (10.1038/s41597-020-00694-w). Altimetry can provide a spatial description of the variability of sea level trends and therefore a useful comparisong against the TG network. Is this of interest?

We would like to thank the reviewer for the very relevant suggestions. We have now highlighted these two points in the Introduction.

(Lines 69 - 73) "This is an advantage over Richter et al. (2012) since some of the Norwegian tide gauges are located in sheltered areas and might not be representative of the variability captured by the nearest hydrographic station (which can be as far as 100 km apart). Moreover, sea-level measurements from satellite altimetry, unlike those from tide gauges, do not need to be corrected for the vertical land motion."

Probably worth mentioning that the other source of validated coastal altimetry data, which is also based on ALES, i.e. the Sea Level Climate Change Iniative from ESA, cannot be used in your case because the coast of Norway is not fully included in the available regions

We would like to thank the reviewer for the suggestion. To address this comment, we have added this sentence to the manuscript:

(Lines 89 – 91) The European Space Agency (ESA) also provides, through The Sea Level Climate Change Initiative Programme, a coastal satellite altimetry dataset reprocessed with the ALES-retracker. However, it only covers the northern latitudes up to 60°N and, therefore, only part of the region of interest in this study (Benveniste et al., 2020).

I am puzzled by this procedure. To my knowledge, the EOT11 tidal correction is provided as a field in OpenADB, but not as separate tidal contribution. So how did you run the model to separate the contributions? In any case, the simple fact that you produce monthly values of your tide gauges is enough to filter out the tidal contribution, so I don't understand why this step is necessary.

We use the EOT11 tidal model to remove the low-frequency tides (such as the annual and the nodal tides) because they would otherwise be present in the (monthly averaged) tide-gauge records. Indeed, due to their low-frequency, their contribution is not averaged out when we average the signals at each tide gauge location.

I don't understand, are we talking about Tide Gauges or Hydrographic Stations?

In the text, we referred to the hydrographic stations. To avoid confusion, we deleted the phrase “As for the tide gauges” at the beginning of Line 178.

I am not an expert of the variability of surface atmospheric temperature, but I would appreciate a comment in the manuscript about the possible impact of comparing a dataset with such a coarse spatial resolution against pointwise coastal measurements at the hydrographic stations

We removed the part where we compare the trend of the thermosteric sea-level with the atmospheric temperature at 2 m. Indeed, the comparison between the two is made difficult by the large-uncertainty associated with the thermosteric sea-level trend.

This section is insufficient. The following points need to be discussed:

1. First of all I would not call it "sea level decomposition". This is easily misunderstood with a decomposition of the effects affecting sea level. You are simply fitting a trend and an annual sinusoid to the time series. This is therefore a harmonic analysis

   We would like to thank the reviewer for the suggestion. We have renamed the sub-section “Harmonic analysis of sea-level” (Line 191).

2. How do you solve the search of the coefficients? For example: weighted least squares? This shall be mentioned

   We use the Levenberg-Marquardt algorithm to perform the non-linear fit. We have added this information to the section (Line 193).

3. Most importantly: how do you treat uncertainties? Anything not accounting for the serial correlation in the estimation of the trends cannot be accepted nowadays. The kind of analysis you are performing will be strongly affected by the uncertainties, so a clear description of the methodology is fundamental. It is impossible to judge the results without being sure that serial correlation have been taken into account. If not, the uncertainties reported are simply not realistic.

We would like to thank the reviewer for the suggestion. We have extended sub-section 3.1 to explain how we deal with the uncertainties and the serial correlation in the sea-level anomaly time series. Moreover, we have dedicated Appendices A and B to explain how we compute the confidence intervals of the linear trends of the time series analysed in the manuscript.

The correlation analysis and the different treatment of along-track vs across-track distances is an interesting addition made possible by the coastal shape in Norway. But the coupling of altimetry VS TG based on correlation is also nothing new, so the authors should mention at least one example of the previous literature.

We have modified the manuscript and added Cipollini et al. (2017) as a reference (Line 227).

Cipollini, P., Benveniste, J., Birol, F., Joana Fernandes, M., Obligis, E., Passaro, M., Ted Strub, P., Valladeau, G., Vignudelli, S., and Wilkin, J.: Satellite altimetry in coastal regions, in: Satellite Altimetry Over Oceans and Land Surfaces, https://doi.org/10.1201/9781315151779, 2017.

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?

We would like to thank the reviewer for the comment. To clarify this point, we have provided additional information in the manuscript:

(Lines 227 – 234) "To select the minimum and the maximum distances from the coast, we have proceeded as follows. We have set the minimum distance from the coast following the recommendations on how to use the ALES dataset: these suggest to discard data within 3 km from the coast. We have then performed a sensitivity analysis and found only small differences between the results obtained applying a maximum distance of either 40 km or 20 km. To only focus on the observations over the continental shelf, we have selected the range of distances from the coast between 5 and 20 km. Similarly, 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."

Please add latitude and longitude in this figure.

We have added the coordinates to all the maps in the manuscript.

There are plenty of "might" in this paragraph. Please highlight with a more appropriate scientific language the statements on which you have confidence, and connect them with the proper argumentation provided in the next paragraph (correlation analysis at variable distances)

We have reformulated the paragraph to cite supporting literature and give more details of the local geography of anomalous stations.

This conclusion is assuming that, in stations where the standard deviation is higher, the best correlated points are the closest ones. But is this the case? Before you said that the correlation was getting better when considering larger distances, so this is now counter-intuitive. Isn't there any case in which the higher standard deviation is due to:

1. Remaining "outliers" in the altimetry points close to the coast
2. Location of the tide gauges in sheltered bays?

You briefly mention cause number 2) in the text. I think this should be very important in the discussion, as the issue of the correspondence between altimetry and tide gauge is a hot debated topic.

We would like to thank the reviewer for the comment on the correspondence between the altimetry observations and the tide gauges. To address it, we have added this paragraph to the “Discussion and conclusions” section of the manuscript:

(Lines 600 - 607) “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 thousands 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 reduce the noise in the SLA from altimetry which might result, for example, from the rough topography of Norway.”

Such a consistent result is worth a couple of lines of discussion. Is one of the dataset over/underestimating the annual cycle, or is this a reasonable result given that coastal seasonality is known to be higher than open ocean one? And is there a correspondence between tide gauges located in enclosed bays and points in which the difference reverses? And is this statistical significant at all, given the uncertainties (this comes back to my major comment on the absence of description about uncertainties in the methodology)

We would like to thank the reviewer for the comment. To tackle it, we have added a paragraph to the Discussion and Conclusions section of the manuscript:

(Lines 571 - 577) The ALES-retracked satellite altimetry dataset is found to underestimate the amplitude of the annual cycle along large portions of the Norwegian coast (Fig. 6). Even though the difference between the two sets of estimates is not significant at a 95% significance level (the 95% confidence interval is approximately twice the standard error), we find this result interesting because of its consistency. We do not expect such a consistency to depend on the ALES retracker since we find a comparable result when we use the along-track (L3) conventional altimetry product (Fig. C3). We rather suspect a dependence of the amplitude of the annual cycle on the bathymetry and, therefore, on the distance from the coast, as shown by Passaro et al. (2015) along the Norwegian sector of the Skagerrak.

Passaro, M., Cipollini, P., and Benveniste, J.: Annual sea level variability of the coastal ocean: The Baltic Sea-North Sea transition zone, 120, https://doi.org/10.1002/2014JC010510, 2015.

How do you explain instead the points in which the altimetry trend estimates within 5 km are much less in agreement with TGs than the black dots? For example Viker to Helgeroa to Oslo in the south? Is this a problem of residual outliers in your time series? I would have said so, but this looks more systematic. Moreover all trends up to Bodo are lower at 5 km than at the black dots, why?

During the revision process, we noticed that we had at first corrected the tide gauge data for the geoid changes from GIA, even though the altimetry data were not corrected for these two contributions. We reprocessed the tide gauge data not to account for this contribution to the sea level and found a better agreement with the altimetry data in Figs. 8 (where we compare the sea-level trends).
Moreover, we found that, with the newly reprocessed tide gauge data, restricting the altimetry observations to 5 km from the coast does not improve the agreement between the sea-level trend from altimetry and tide gauges. As such, we modified the manuscript and not considered the 5 km case anymore.

This also changes the discussion section: we remove the paragraph where we discuss the possible dependence of the sea-level trend on the distance from the coast.

Modification of Figures 4 and 5

The RMSD between the detrended and deseasoned SLA from satellite altimetry and each tide gauge is:

$$RMSD = \sqrt{\frac{\sum_{i=0}^{N}\left(SLA_{altimetry,i} - SLA_{tide\ gauge,i}\right)^2}{N}}$$

where N is the length of the time series derived by computing the difference between the detrended and deseasoned SLA from satellite altimetry and the tide gauge. When we computed the RMSD in the original manuscript, we did not account for the presence of missing values in the detrended and deseasoned SLA from satellite altimetry and from the tide gauges. Therefore, we set N equal to 192 (the number of months between January 2003 and December 2018). In the revised manuscript, we recomputed the RMSDs, this time accounting for the number of missing values in the time series. While this change only slightly affects Figs. 4B and 5B (but not the text describing them). Therefore, in the revised manuscript, we present the new version of Figs. 4 and 5.