

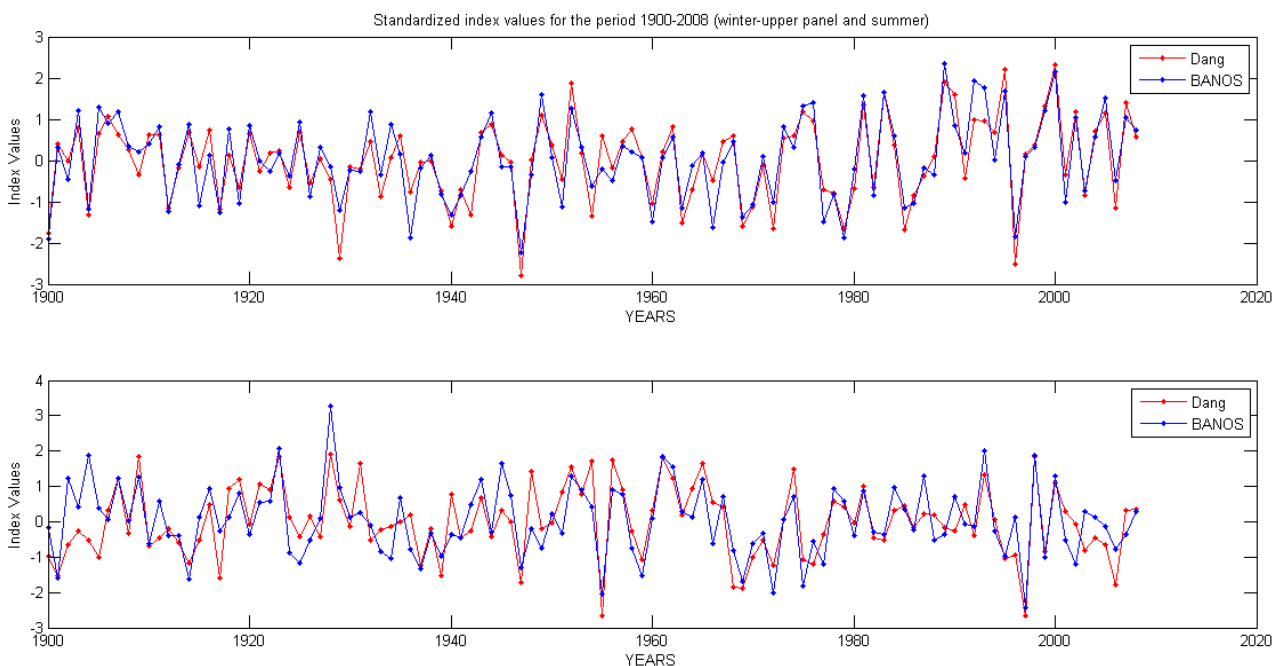
We thank the reviewer very much for reviewing our manuscript, for providing constructive criticism and useful suggestions. We respond to all comments below.

General comments

1. Dangendorf et al. 2013 found a very similar correlation pattern between sea level at the Cuxhaven tide gauge and atmospheric pressure. Furthermore, Dangendorf et al. 2014a derived an atmospheric proxy for sea level variability in the North Sea, based on the pressure difference from virtually the same areas to what has been presented here. Does the BANOS proxy represent a different pattern and does it perform better than this proxy? Otherwise, this study seems of limited use.

The BANOS mode of atmospheric circulation indicates different atmospheric pattern than SLP pattern that Dangendorf et al. 2014a suggest, especially in summertime. This difference also affects the role of the physical factors that explain the linkage between BANOS-index and sea-level variability. For example, in our interpretation, the BANOS mode does not indicate a wind-driven surface water transport from the North Sea to the Baltic Sea over the transition zone in wintertime. Furthermore, the correlation pattern (Figure 13-top) between the BANOS-index and SLA grids indicates a large scale seesaw effect of BANOS mode on sea-level variability between the North Atlantic and the Baltic Sea region. Our study explains that the Inverse Barometer Effect (IBE) plays a key role explaining the linkage between SLP BANOS pattern and sea-level variability. This effect was not discussed in the previous studies (i.e. Andersson 2002 and Dangendorf et al. 2014a).

To illustrate the covariability between those two indices (BANOS and Dangendorf et al. 2014a proxy), we computed the correlation coefficients of de-trended time series between those two indices. For wintertime (summertime) the correlation is 0.89 (0.74) for the period 1900-2008. The standardized index time series (not de-trended) in the winter (upper panel) and summer seasons for the period 1900-2008 are shown below.



2. The results almost only show correlation patterns. These patterns can give insight, but it does not show the amplitude of the signals involved. Which fraction of the observed sea level variability can be explained by the atmospheric proxy? What about the fraction of explained variance (R-squared) as a measure of the BANOS model skill? Xu et al. 2015 show that the typical amplitude of variability differs widely within the Baltic (See their figures 3 and 4). Does the coherent NAO/BANOS-induced variability share a coherent basin-mean signal, only with regionally-varying amplitude? What is the standard deviation/RMS of the residual sea level after removing the NAO/BANOS signal?

In Figure 13, we show the correlation pattern between BANOS-index and SLAs for wintertime and summertime. Actually, those figures provide information about the fraction of sea-level variability that can be explained by the BANOS-index. For this computation, correlation coefficients should be squared. Additionally, we mention about the BANOS explained variance of sea-level in the different parts of the manuscript (i.e. “Abstract”, Page 1 Line 18-20).

To show the amplitude of the sea-level variability involved in the BANOS mode of atmospheric circulation, we computed the sea-level standard deviations from observations, from the sea-level explained by BANOS-index predictions and from the residuals. For the BANOS prediction, we applied a linear regression between BANOS (predictor) and satellite SLAs (predictand) for each SLA grid over the period 1993-2013. The residuals are deduced from that linear regression. The results are provided in the following figure (units: mm).

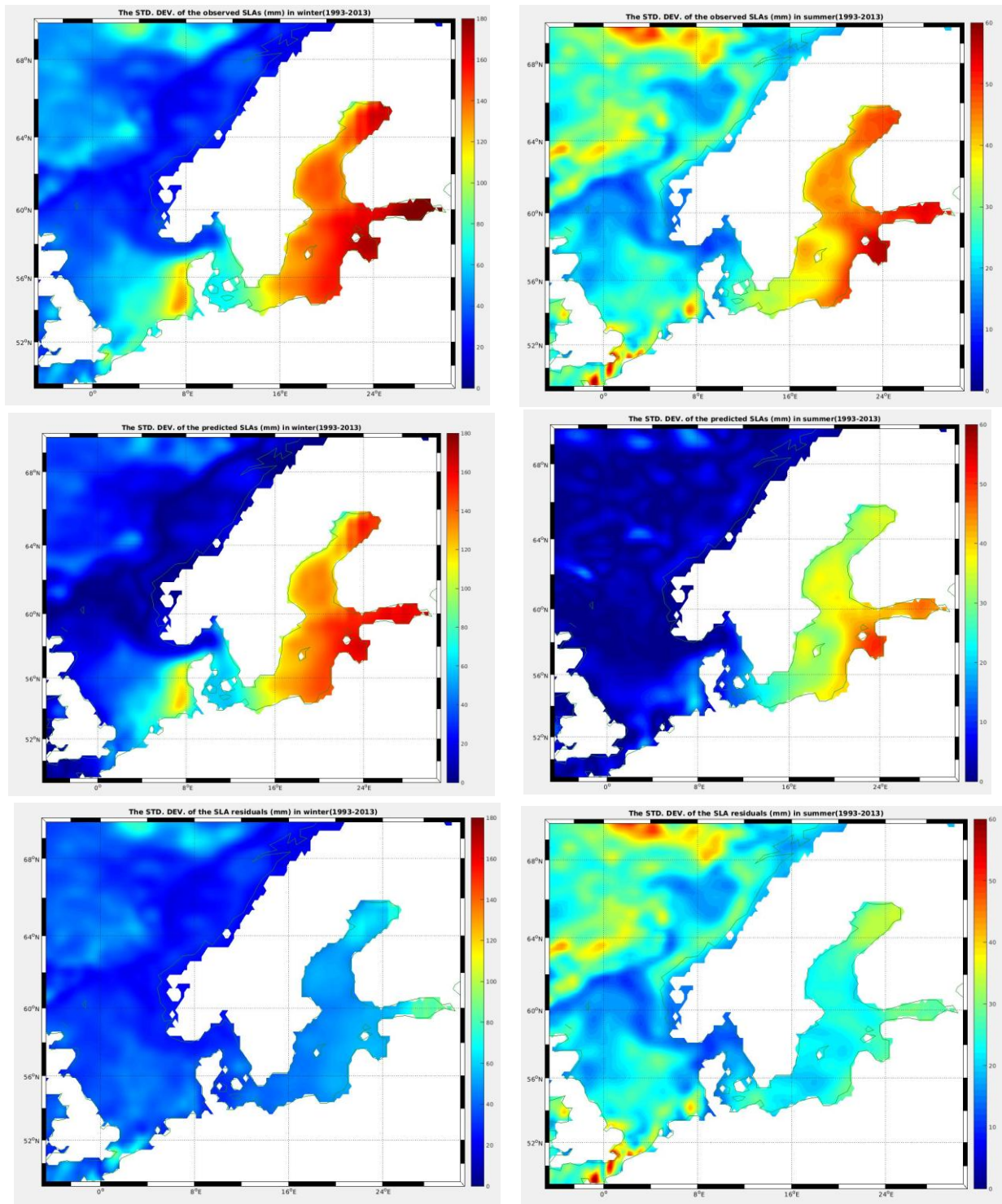


Figure shows the amplitude of sea-level variability from observations (top), BANOS predictions (middle) and the associated residuals (bottom) (Measured SLA_i – BANOS predicted SLA_i) for the winter (left panels) and summer (right panels) seasons for the period 1993-2013. In the figure, the scale is up to 180 (60) mm for the winter (summer) season.

We note that observations and BANOS predictions depict consistent spatial distribution of the standard deviations, especially in wintertime. For wintertime, high sea-level standard deviations occur in Bothnian Bay, in Baltic proper, in the Gulf of Finland and in the Gulf of Riga. The

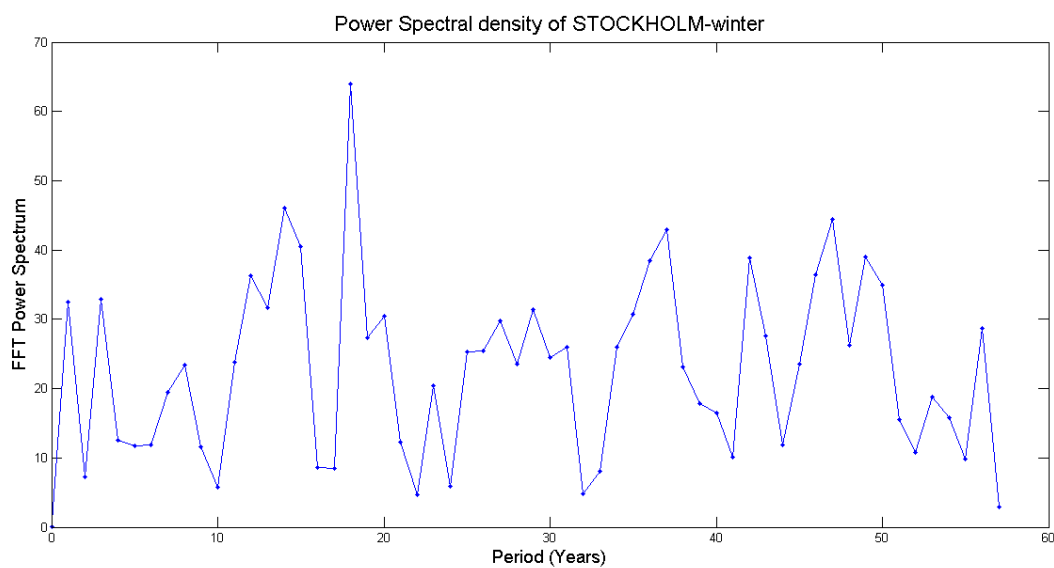
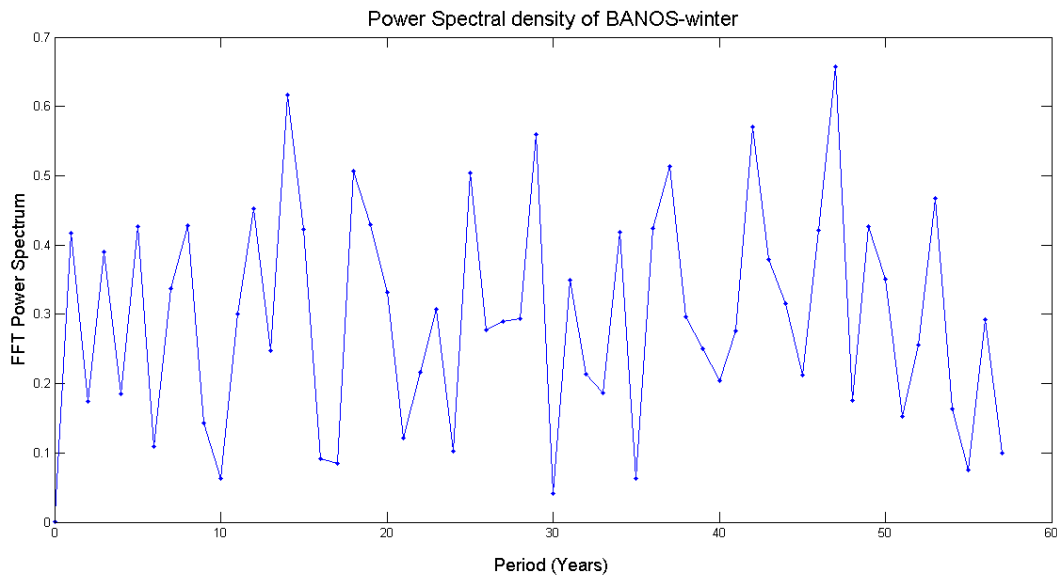
residuals show relatively small and spatially homogeneous standard deviation (~50 mm) distribution in the Baltic Sea and the North Sea in wintertime. In summertime, the spatial distribution characteristics are also consistent. For summertime, the Gulf of Riga shows the highest standard deviation, which is also showed by the standard deviations explained by the BANOS index. Again in summertime, it could be said that residuals show small standard deviation values (>25 mm) and a homogeneous pattern in the Baltic Sea basin except for the Bothnian Bay and the southern part of the North Sea where the standard deviations differ.

Overall, the results indicate that BANOS-induced atmospheric signal can explain a considerable amount of sea-level variability in the Baltic Sea and North Sea region. Especially in wintertime, the BANOS-index explains almost of all sea-level variations linked to the atmospheric circulation.

3. What are the typical time scales of the variability explained by the BANOS index? Are we explaining monthly variability, seasonal, annual or even longer variability? The abstract suggests 'interannual', which is sometimes repeated, but is not worked out. Since many processes act on different time scales, this classification is very necessary. For example, North Sea variability on decadal time scales is generally assumed to be driven by integrated longshore winds that cause coastally-trapped waves (See Calafat et al. 2012/2013, Dangendorf et al. 2014b and Frederikse et al. 2016), which has not much to do with NAO/BANOS-related effects. Does this signal affect the Baltic Sea? A tool that can be suitable to find the relevant timescales at which the correlations are largest is the wavelet toolbox from Aslak Grinsted (<http://www.glaciology.net/wavelet-coherence>). Furthermore, a plot that shows observed sea level and the fraction explained by the BANOS index could give more insight.

In this study, we analyse sea-level variability on interannual time scale. All time series involved in the analysis are winter means (December-January-February) and summer means (June-July-August), which are computed from monthly means.

Considering the analysis technique that Grinsted used, we applied a frequency domain analysis (Fourier Analysis) on the Stockholm sea-level and BANOS-index time series for winter seasonal means. The power spectrum of BANOS-index (upper panel) and of the Stockholm record in the following figures. Time series are detrended prior to the analysis and the analysis period was 1900-2013.



The time series show a white noise character, with no clear peaks in the spectrum.

In addition, several researchers using different techniques have examined the power spectra of the NAO indices. A spectral analysis on the NAO-index (Hurrell et al. 2003) indicates that the spectrum of winter mean NAO index is red, but there is no significant peak.

4. It's not clear to me how the time series are formed: do the authors use a mean value for each summer/winter (thus one value per year), or do they use the monthly data from the winter/summer months (thus multiple values per year)? How is the seasonal cycle treated?

For the whole analysis, we used seasonal mean value for each winter and summer. Those winter and summer mean values are computed from monthly means prior to the analysis. We analysed winter and summer separately. Thus, there is no need to remove the seasonal cycle. We will make some clarification in the manuscript about it.

5.The region is unique due to the presence of many long tide gauge records. Why not use all of these records to show the capability of the BANOS index? Figure 6 and 7 suggest a non-uniform NAO response at different tide gauge locations. This analysis may also provide the much-needed insight into my points 2 and 3 above. Furthermore, the analysis of long-term records in the North Sea only seems to cover the German Bight, while many more tide gauges are available for most of its coastlines.

Keeping in mind that interannual sea-level variability in the Baltic Sea and the North Sea is spatially quite coherent (i.e. Stramska 2013), we used nine representative tide gauges assuming that they would be representative for sea-level variability on interannual time scale in this region.

The correlation pattern between BANOS-index and satellite SLAs indicates that only the eastern part of the North Sea(only German Bight) is connected to the BANOS mode of the atmospheric variability in wintertime(summertime). Since Dangendorf et al. (2013) considered the Cuxhaven record to analyse sea-level variability in the German Bight, we also carried out a statistical analysis considering the connection between the BANOS-index and the Cuxhaven station. That statistical analysis indicates that 64% of sea-level variance can be explained by the BANOS-index in wintertime for the period 1900-2008. The following table show the correlation coefficients among the BANOS-index, the Cuxhaven and Stockholm stations for the period 1900-2008.

Corr. Coeff. Winter (Summer)	Cuxhaven	Stockholm
BANOS	0.80(0.50)	0.84(0.72)
Cuxhaven	-	0.88 (0.67)

6.The inverted barometer effect (IBE) is and only is the static sea level response to air pressure anomalies, and therefore dictates a fixed regression coefficient of -1 cm/hPa. Therefore, the observed pressure correlations, for which a different regression coefficient is found are not only resulting from IBE.

The comment by the reviewer assumes that air pressure changes only at the location of the tide gauge. In the manuscript, we will clarify that the inverse barometer effect (IBE) should include the pressure gradients, since water should be transported from one point where pressure increases to another where pressure decreases. We assume the presence of an infinite ocean without topographic limitation and complete equilibrium in the Baltic Sea and North Sea region. For example, concerning the winter season, the increase in high SLP system over the North Atlantic (especially around Gulf of Biscay- Figure 9) pushes the water into region where the low-pressure decreases in the Baltic Sea and North Sea region.

We will rewrite that part of the text in order to make the explanation more clear than in the current version.

7. The conditions for Ekman transport to develop are to my knowledge not satisfied, I'd guess, since the Baltic Sea is small and very shallow. For Ekman transport to develop, the dominating balance in the equations of motion is between wind stress and the Coriolis force. Since the Baltic Sea is both shallow and small, bottom friction will probably play a large role, and the basin size is probably smaller than the Rossby radius of deformation. Hence, I'm not convinced by the conclusions that follow from this analysis. Many other studies point at the large influence of local winds on sea level variability here. It may be worthwhile to investigate the wind effects using a barotropic ocean model. These models can explain a large fraction of the observed sea level variability of monthly-mean data, as shown by Xu et al. (2015).

In our study, Figure 18 shows the expected transport based on the assumption that wind-driven sea current is due to only a geostrophic wind approximation and a complete Ekman layer, which assumes that bathymetry does not interrupt the Ekman Layer.

However, knowing that transition area between the North Sea and the Baltic Sea is shallow, it is likely that bathymetry will interrupt the Ekman Layer, and that the water transport is more parallel to the geostrophic wind flow implied by the BANOS pattern.

We will update the text in order to clarify our assumptions about the Ekman Layer.

8. In a paper by Chen et al [2014], the role of barotropic and baroclinic responses to the NAO in the North Sea are extensively discussed. One of their main arguments is that local density effects on a shallow shelf are small, but a horizontal pressure gradient that develops when a deep ocean column expands results in mass transport towards the shelf. How could we combine these results with your attribution process, which relies quite heavily on density effects (freshwater flux/heating)? Are in-situ temperature and salinity profiles available in the region to verify whether local density effects play a substantial role? Otherwise, regional ocean reanalysis products (some are available at <http://marine.copernicus.eu>) may provide estimates. From the observation of the anti-correlation between BANOS/NAO and open-ocean sea level, couldn't it be a wind-driven mass redistribution process? Over the last 15 years, you may have a look at what GRACE observations say about mass changes.

This is an extended comment that addresses several points. The reviewer is right that the expansion of the open ocean water column may affect coastal sea-level. This effect has been estimated for the North Sea in the context of future sea-level rise due to climate change by Grinsted et al., who estimate its possible contribution with about 10% of the total sea-level rise. It may be therefore not negligible but not totally significant. We will discuss this possible contribution in the revised version by looking at the heat flux associated with the BANOS patterns.

Regarding the link between the NAO and sea-level variations in the North Atlantic (open-ocean), we feel that this is actually beyond the scope of our study that is restricted to the shelf seas.

In-line comments

Note that the page numbering re-starts every page. I use the PxLy notation, referring to page x line y.

Title and P1L6: This study mainly deals with the Baltic Sea, and only partially with the North Sea. I suggest: 'German Bight' instead of North Sea.

Here, we analysed the relation between atmospheric circulation and satellite SLAs including the whole North Sea. However, the results of our analysis show that German Bight is the most sensitive area in the North Sea to the BANOS mode of the atmospheric circulation. Therefore, the atmospheric mode that we identified mainly explains sea-level variability in the whole Baltic Sea and a part of the North Sea. At this point, we should mention that we considered the off-shore sea-level variability in the whole North Sea, but, only a part of the North Sea sea-level variability can be explained by BANOS mode of atmospheric circulation.

In addition, to quantify the contributing factors to the linkage between BANOS-index and sea-level variability, we made basin wide analysis in this region including the whole North Sea basin.

For those reasons, we prefer to keep it as 'North Sea'.

P4L15: References to Dangendorf et al. 2013/2014 should be discussed here, and further on, what do we learn from this paper that we do not know yet after reading these papers?

We will discuss the Dangendorf et al. studies. As explained in the previous comments, there are indeed some differences of interpretation of the physical mechanism and also in the geographical areas considered.

P5L3: For completeness, it's a good idea to add links to the web sites from which you've obtained the data.

We will add those links to web sites.

P5L11: Do you derive season-means from monthly data? Or monthly data only over this period? What about spring and autumn?

We calculated seasonal means from the monthly data sets. The focus was on winter and summer seasons when the atmospheric anomalies are expected to be in the largest and the smallest phases.

P5L13: Altimetry data does not have a 1/4 by 1/4 degree resolution: along-track observations are interpolated onto a grid which can have a higher resolution than the data from which it is composed. Note that observations are integrated over distances of about 100 km (See Le Traon et al 2001 or Pujol et al. 2014). Furthermore, observations deteriorate quickly close to land, and shallow-water tides may alias into lower frequencies. Hence, it is very tricky to separate smallscale features in shallow shelf seas. Tide gauges are generally more reliable in such areas. An alternative may be to use along-track altimetry observations, which do not suffer from problems related to interpolation. These are widely available from AVISO.

The reviewer is right that caution is needed when using satellite altimetry near the coast, but this is the reason why we also included a comparison between the tide-gauge records and the co-located altimetry pixels. We will also better explain the spatial resolution of the satellite altimetry data sets.

P6L2: The word 'slope' here seems a bit misleading: you compute the regression coefficient between the atmospheric pattern and sea level. 'Slope' suggests a linear trend to me.

We do not use word "slope" in the mentioned line.

However, in P7L2 we had written "The slope of the regression line is denoted as the sensitivity". We changed it as "The linear regression parameter of the regression analysis is denoted as the sensitivity".

As well as P13L9 was "is estimated from the slope of the regression line resulting from the regression analysis where the BANOS index".

We changed it as "is estimated from the linear regression parameter of the linear regression where the BANOS-index..."

P6L11: The NCEP/NCAR reanalysis 1 is not really state-of-the-art anymore. Furthermore, since you use this data set to derive heat fluxes and precipitation over sea, how good does this model perform for these quantities? I guess that this model does not directly assimilate heat flux and precipitation data, and that they are derived from wind and pressure data. It may be worthwhile to use something like MERRA or ERA-interim, in which flux observations derived from satellites are assimilated. An other alternative may be OAflux (<http://oaflux.whoi.edu/>)

We thank reviewer for this suggestion.

As a simple assessment test, we investigated the NCEP/NCAR net heat flux performance with respect to OAflux net heat flux. For this test, we considered field mean of a geographical area over the North Atlantic (a box covering 35° W - 15° W and 50° N-60° N) in the winter season(DJF) for the period 1984-2009. The correlation analysis on those time series indicates that NCEP/NCAR data set performs well. The correlation coefficient is 0.98.

Considering the MERRA and ERA-interim, the data sets are available from 1979 on, which is quite short in comparison to the NCEP/NCAR reanalysis data set. In this study, we analysed the contribution of atmospheric factors to the connection between BANOS mode of atmospheric circulation and sea-level. Therefore, once we established a statistical linkage between BANOS-index and sea-level variability, we could compute the strength of relation between atmospheric factors and BANOS-index by using the climatic variable as long as possible over the last century. Therefore, analysis period was not limited to satellite era. However, we can use products of MERRA and ERA-interim for the direct comparison between satellite SLAs and climatic variables in a future work.

P6L34: This statement seems easy to verify: what is the correlation over the common altimetry/TG period?

Probably there is a typo here - page 6 has only 28 lines. We could also not guess what could be the statement from the comment.

P7L10: Why only check for these three? If you use all available tide gauges in the region with a substantial amount of observations, you can generate a map with the correlations at each TG location. This will make much clearer whether altimetry observations do a good job, especially at the narrow straits (Kattegat/Skagerrak etc) and around islands.

Indeed, Figure 2 and Figure 3 provide information about the coherency of the satellite SLAs over this region as well. Since nine tide gauges have strong correlations to satellite SLAs on and around the closest point of their positions.

However, we will compute additional correlation values between tide gauges and satellite altimetry over those areas.

P7L11: Could you make the followed procedure more clear? I don't fully understand how the data has been treated. I also wonder how you treated the seasonal cycle.

We will clarify the text based on the suggestion. Also please see our response to point 4.

P7L16: A correlation only does not show that signals are coherent: what about the fraction of explained variance, or a simple plot, in which both time series are compared?

The reviewer probably means the variances themselves, since the fraction of explained variances are just the correlation squared.

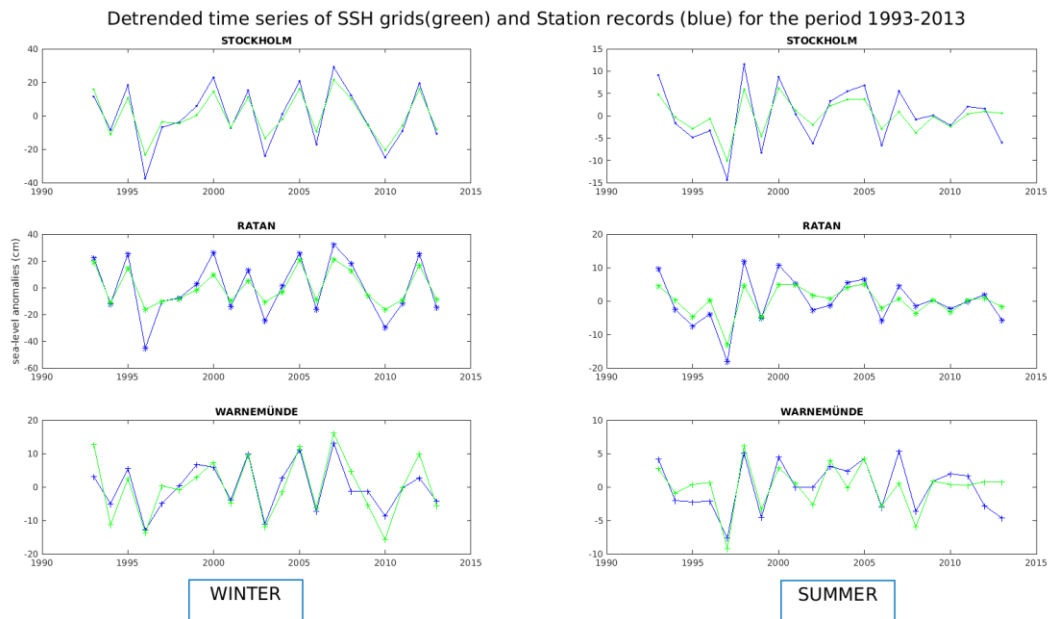
The explained variances are:

	Winter	Summer
Ratan-SLA	0.90	0.83
Stockholm-SLA	0.94	0.85
Wismar-SLA	0.78	0.61

In the following figure, we show the detrended time series of tide gauges and satellite altimetry observations, which are from the closest points to each tide gauge. In each panel, the top figure is for Stockholm, middle is for Ratan and bottom is for Warnemünde, also left(right) column represents winter(summer) values.

It can be seen from the figures that, in general, the stations agree well with the satellite SLAs in wintertime, only the Warnemünde station has some discrepancies over the beginning and end of the considered period. In comparison to the wintertime, a relative discrepancy is found between all stations and satellite SLAs in summertime, especially in the relation between Warnemünde and satellite SLAs that discrepancy becomes clearer with respect to the wintertime. This discrepancy

between satellite SLA and tide gauge in Warnemünde may occur due to the complex structure of the coast.



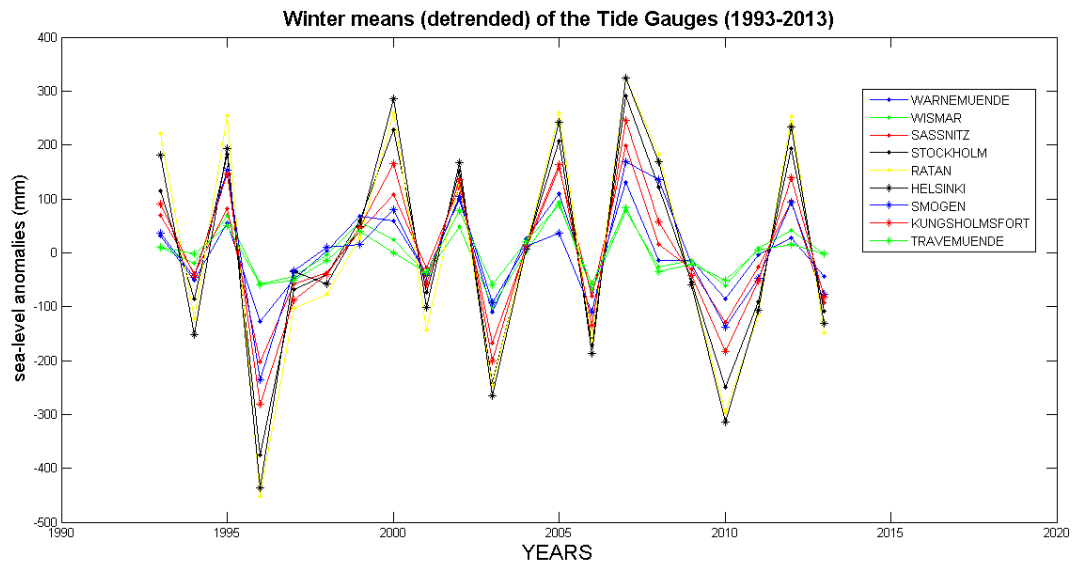
P7L17: Are you sure that you compare the same signals with Yan et al [2004] ? Same treatment of seasonal cycle/lowpass filters/detrending etc? Maybe this statement falls outside the scope of this manuscript.

In our study, we computed the winter means from monthly data sets, then detrended the data over the considered period prior to the correlation analysis. Since we analyse data sets in winter and summer separately on interannual time scale, we did not need to remove the seasonal cycle.

As far as we understand, Yan et al. 2004 also made correlation analysis with detrended time series based on interannual time scale (in Table II). Therefore, we think that a comparison between our results (our Table I) and Yan et al. 2004 (their Table II) is possible concerning those correlation coefficients.

P7L24: From this correlation pattern I'd assume that the tide gauge records have a high mutual correlation. Could you show the time series of all tide gauges in one plot to verify this?

The following figure shows the time series of associated tide gauges over the period 1993-2013. (time series represent detrended winter means (DJF) of tide gauges)



P7L28: This is an interesting finding and may give some hints about the underlying processes!

The seesaw pattern that suggests negative and positive dipole relation between North Atlantic and Baltic Sea basin wide sea-level variability also brought us to test the contribution of inverse barometer effect (IBE) to the linkage between sea-level and BANOS-mode of the atmospheric circulation. As a result, we found that major driver of the connection between sea-level and the BANOS-index was IBE.

P8L28: How do you define 'significant'?

We will add the significance threshold of the correlation coefficient for that record length.

P9L3: Add here very explicitly about which time scales you are discussing.

We will specify the time scale here, and change the sentence: *“As pointed out, the patterns of correlation between the tide gauges and the satellite altimetry fields indicate that the variations of sea-level in these regions are spatially quite uniform in both seasons (Figure 2 and Figure 3) on the interannual time scale”*

P9L7: ‘tend to be spatially coherent’: again, the large amplitude variations over the region do not support this statement.

We will remove this statement, but we would like to point out that the term *“coherent”* is sometimes used instead of the term *“strong correlation”* and that it does not necessarily provide information about the amount of the relative variation of two variables.

P9L8: ‘the influence of NAO is spatially quite heterogeneous’: where do you show this?

It has been documented in the literature (i.e. Yan et al. 2004, Hünicke and Zorita 2006, BACC Book

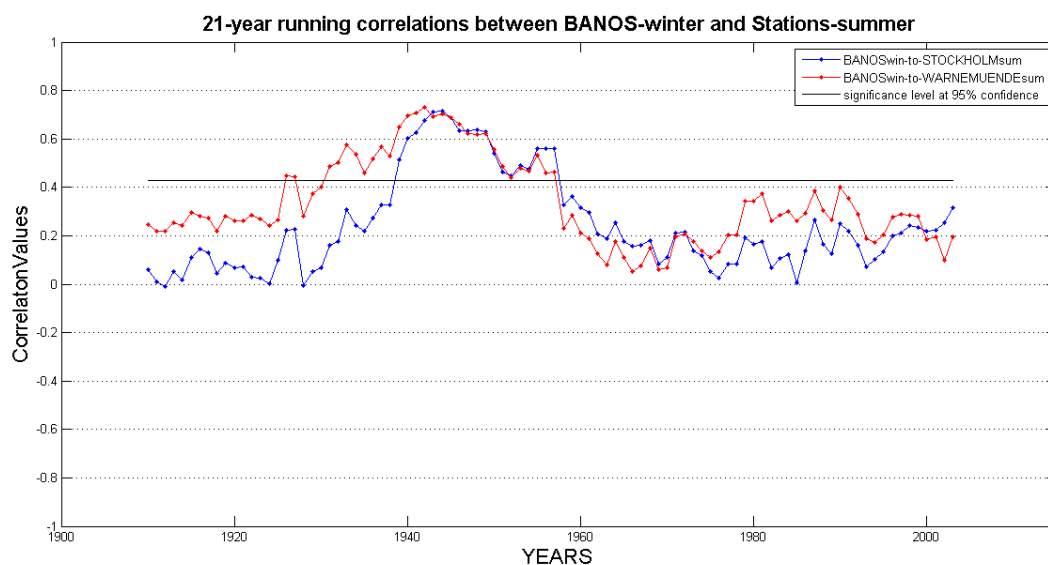
II 2015) and we also show in Figure 5 and Figure 6. We make clarification in the manuscript about this statement.

P9L26: These findings contradict with the conclusion that Baltic sea level is coherent, as the difference in the found correlations is rather large, which would not be the case if the signals at both tide gauges were coherent.

Here, we wanted to exactly highlight this point. Whereas the sea-level variability seems spatially coherent, in the sense that the tide-gauges are relatively highly correlated, the correlations with the NAO are not. This implies that there may be another large-scale effect rather than the NAO which is driving the sea-level variability over the whole Baltic Sea basin. That rationale was also the background to look for another atmospheric pattern that is strongly connected to the Baltic Sea level variability and that differs from the NAO pattern.

P10L19: The correlation patterns seem to be almost the same as already found by Dangendorf et al, 2014a. Following the argumentation, this makes sense, as sea level variability in the Southeastern North Sea, used by Dangendorf et al, and the Baltic sea is coherent and thus has a common driver. Can't we just suffice by saying: "The atmospheric proxy, developed by Dangendorf et al. [2014] does not only work as a proxy for the S.E. North Sea, but also for the Baltic Sea."? Or is the new BANOS index doing significantly better? The only difference between Dangendorf et al and BANOS seems to me the Eastward shift in summer for the BANOS index. However the summer correlations are substantially less convincing than in winter, as I observe in figure 10. Especially the right panel in fig. 10. How does the model perform when you stick to the winter definition, even for summer?

To statistically test the performance of the winter BANOS-index for the summer sea-level variability, we computed the 21-year running correlation values between winter BANOS and summer stations(Stockholm and Warnemünde).



Based on the result of this analysis, we can say that winter BANOS does not perform well to explain the summer sea-level variability. For the rest of this comment, please see our response to

point 1.

P11L11: It seems that there are some decadal features that are shared between NAO and BANOS. Here, a wavelet analysis, as described above, may be more insightful.

Please see our response to point 3.

P12L8: Isn't this negative trend just a symptom of the non-stationary correlation? Something similar happened between 1905-1935.

The reviewer is right. The correlation between the NAO and sea-level is not stationary, and this is one of the motivations of the study - namely to find an atmospheric pattern that yields more stationary correlations. The reason why the strength of the relation between sea-level and the NAO-index has decreased from 1970 onwards is not known yet. One possible reason is that the variability of the BANOS pattern becomes stronger so that the sea-level records deviate more strongly from the NAO. But this explanation would then prompt the question of as to why does the BANOS pattern become more energetic. We feel that an explanation for this behaviour lies rather in a study of the atmospheric dynamics and lies beyond the scope of this study

P12L9: I'd say: "No significant link between NAO and Baltic sea level in summertime"

We will add this statement into the text.

P12L11 and the following section: Like the NAO, you find a strong anti-correlation between North Atlantic sea level and the BANOS index. That's an interesting finding in my opinion.

Please see our response to P7L28. We will add some comments to the manuscript.

P13L5 and the following sections: Here you show the spatially heterogeneous sensitivity, again pointing at a spatially varying sea level signal. Which fraction of the variability is explained?

Please see our response to point 2.

*P13L9: Avoid the word 'slope' here. Maybe insert a short equation:
 $dSLA = a * BANOS$ with a in [mm/BANOS]*

Please see our response to P6L2.

P13L12: It may well be the case that next to BANOS, more effects are at play here, that are not necessarily linear.

It may be the case. Indeed, a quantitative separation of the different contributions to sea-level variations and the estimation of their possible non-linear interaction can only be done by numerical experiments with a realistic Baltic Sea&North Sea ocean model, which should be analyzed in future studies. In this study, we established a statistical linear connection between natural variations and

sea-level variability by assuming that established connection will stay same when we go backward or forward in time.

P14L4: Why is that suggested? Horizontal pressure gradients will result in a sea level gradient due to the IB effect, and generate geostrophic winds. Do you mean that wind effects play a role?

Please see our response to point 6.

P14L18: As stated in the introduction: The IBE effect is the static response to pressure effects with a sensitivity of 10 mm/hPa. Since, static equilibrium is generally reached on timescales in the order of days, deviations from this static effect imply that some other effects are at play here. That's not so strange, as close to coastal areas, winds play a large role. To separate these effects, a barotropic ocean model can bring more clarity.

We tried to explain our approach in our response to point 6. However, it is obvious that we have to clarify how we handle with the inverse barometer effect (IBE) in the manuscript.

P15 equation 1: I'd suppose that the rate of change in steric sea level correlates with the heat flux and not the sea level itself. i.e. $dSL/dt \sim Q_{net}$ instead of $SL \sim Q_{net}$. What if you integrate Q_{net} before computing any correlation?

The reviewer is right, but, we actually did integrate the heat flux over the whole season, so that the variable that we use to correlate with the seasonal sea-level is the total amount of heat that goes into the ocean (or leaves the ocean) over one season

P15L24: This number is rather large, I suspect it's incorrect.

This number is computed from the sensitivity of net energy flux to one unit change in the BANOS-index. Sensitivity value is 3.28 J/m².s. This means one unit increase in the BANOS-index causes 3.28 J/m² per second over the winter. To compute the sensitivity of net energy flux to the BANOS-index over one winter, we multiplied the 3.28 with 60*60*24*90, which is equal to that number. That amount of heat distributed over the upper 50 meters in the water column would rise the water temperature by about 0.1 K, so it is not clear to us what the reviewer means. We will double check this number.

P15L25 and below: How did you compute this? If I'd compute the thermal heating that result of the afore mentioned number, the whole Baltic sea would evaporate rather quickly. Are there insitu T/S observations, or SST observations that can confirm the large impact of density changes induced by local heat fluxes? I'd guess that on a shallow shelf, the effect of density changes is rather limited. Furthermore, if so much water evaporates or rains into the basin, doesn't the resulting sea level change lead to transport with the open ocean?

Please see our response to P15 equation.

P16L11 and beyond: Like with energy fluxes, isn't it expected that sea level varies according to the integral of the total freshwater flux?:

$dSL/dt \sim E$

Furthermore, do you suppose that the total mass in the Baltic Sea is affected, or that the effects are caused by changes in the salinity?

We actually use the water flux integrated over the whole season.

P16L25: The correlation pattern does not tell much about what causes what. I'd say that the precipitation/evaporation pattern changes and sea level changes are both caused by the BANOS-related pressure changes. Therefore, they show mutual correlation. But that does not show that the P-E flux causes sea level changes! Hence, the conclusions reached from P16L32 onward are not really justified in my opinion without further proofs.

The reviewer is right that the statistical analysis is not proof of causal relationships. The statistical analysis is rather an estimation of the possible contribution of the fresh water flux and is to be understood as an analysis of plausibility.

P17L7: Do you mean 'geostrophic wind flow' here?

Yes, we mean geostrophic wind flow. We changed the text accordingly.

P17L8: I don't understand what you mean here: why can't the BANOS-induced wind forcing transport surface water between both basins? Although for different regions, many studies point at the large impact of local wind variations on monthly and interannual sea level, including Sterlini et al. 2016, Dangendorf et al. 2013, 2014a, 2014b, and many more. Does the same happen in the Baltic Sea? Figure 3a in Dangendorf et al, 2014a clearly suggest a wind set-up effect.

In wintertime, the SLP BANOS pattern attributed geostrophic wind flow does not indicate westerly winds over the transition zone. It rather implies south-westerly wind, which can cause strong water accumulation towards west Norwegian coast and German Bight. Hence, we can speculate that water accumulation (coastal downwelling) can cause water transport from German Bight and west Norwegian coast towards the Baltic Sea.

P17L15: To my knowledge, as described above, both the local bathymetry and shallow water, as well as the presence of coasts render the Ekman transport assumption invalid. The width of the basin is about 100 – 200 km, which is probably smaller than the Rossby radius of deformation. Especially around Skagerrak and Kattagat, the basin dimensions become very small. Hence, I would not trust results based on the Ekman transport assumption. Again, a wind-forced barotropic ocean model or a regional ocean reanalysis could bring more trustworthy results regarding changes in wind-induced transport and sea level variability.

In our discussion we provide an estimation of the potential contribution of Ekman pumping under 'perfect' conditions. We are aware that these conditions are not totally met, even less so in the shallow straights connecting the North and the Baltic Sea. However, our estimation is not focused on these areas. We wanted to estimate the transport that can be attributed to the large-scale BANOS

pattern. In the end, the total water transport will be caused by a combination of the local wind forcing and the larger scale transport, which will also cause local pressure gradients along the narrow straights.

The reviewer is right that a quantitative estimation requires the use of a comprehensive ocean model, but our study is a statistical analysis of the available data.

P18L3: The impact of NAO/BANOS-related variability is the only atmospheric effect on sea level analysed in this study. There may be more atmospheric processes affecting sea level on interannual time scales.

We agree with the reviewer's comment. It is also reason that we write "partly quantified".

I'm afraid that most conclusions are not justified by the presented results:

- *Conclusion #1: According to other studies, the amplitude of the interannual variability differs widely around the region and therefore, the variability is not coherent. Furthermore, in figure 14 you show that the spatial signal is far from coherent, as the sensitivity values differ by a factor 10 over the basin.*

The comment by the reviewer is based on a different meaning of coherency. The reviewer uses this term when the records would show roughly the same amplitude of variability, whereas we use it to denote correlated behaviour in time. We will clarify this point in the revised version.

- *Conclusion #2: In figure 6, top I see a rather strong correlation between the NAO and the altimetry-derived sea level in wintertime, rather than weak!*

We derive this conclusion from the 21-year running correlation values between the NAO and two tide gauges over the last century. The temporal variability of the link between the NAO and sea-level variability can sometimes show some strong correlation for an individual short term period, as it is happened over the satellite era.

- *Conclusion #3: There are essentially two indices: summer-BANOS and winter-BANOS.*

We will change this accordingly.

- *Conclusion #4: The BANOS index only correlates with sea level variability in the German Bight, and not in the whole North Sea in summer.*

Our conclusion #4 does not say something inconsistent to this comment.

- *Conclusion #5: Since the regression coefficient deviates from the static IB response, it's probably not only the IB effect that causes the pressure-sea level link. Furthermore, if the IB effect explains 88 % of sea level variability, and surface fluxes 35%, we explain more than 100 %. Again, you've only showed a correlation pattern and not what causes what. They also may have a common cause. How are these percentages derived? The conclusions regarding wind-driven variability depend on the Ekman transport approximation, which is probably not valid in this region.*

One of the complexities in identifying the physical mechanism of the sea-level variation is that there can be interrelations among the considered physical mechanisms. Hence, a possible overestimation may occur if some interdependent forcings (predictors) are included in the associated statistical model. This possibility can be partially excluded with the use of atmosphere indices that contain several effects and represent them in a single index (e.g. Sterlini et al. 2016). For example, the NAO, as a mode of atmospheric variability, can carry information about the wind, sea-level-pressure and surface heat fluxes. In this part of the study, describing relations between the effects of the related driving factors and sea-level variation based on atmospheric indices enables us to use those relations without making any further analysis such as multicollinearity test between drivers. Considering the explained variances of sea-level by the inverse barometer effect (IBE) and net energy flux (NEF), it seems that sea-level variance is overestimated due to the amount of explained variance in total. The first reason is that we assumed a complete equilibrium over the Baltic Sea and North Sea region for the IBE, which is in real not the case. Broadly speaking, we estimate a possible maximum contribution of the IBE to the BANOS attributed sea-level variability over the study area. The second reason is that the impact of the NEF is computed by taking the spatial average of the Baltic Sea and North Sea basins. The amount of thermal expansion of the water per one unit change in the BANOS-index would differ depending on the assumed average value of temperature, salinity and pressure through the water column (for this, please see our response to P15 equation).

Here, it should also be noted that a high correlation does not necessarily mean a strong direct physical connection between the conducted factor and sea-level variability. Therefore, the statistical analysis that we applied in this study investigates the potential of contributions of the considered physical factors to the sea-level variability. The quantitative attribution of the driving factors to the sea-level can only be described by numerical experiments with a realistic Baltic Sea and/or North Sea ocean model (e.g. Kauker and Meier 2003).

Figures,

1. In general, it may be a good idea to avoid the ‘rainbow’ color scale for correlations. A good summary of which color maps are suitable can be found here:

<https://betterfigures.org/2015/06/23/picking-a-colour-scale-for-scientific-graphics/> I'd suggest to use a ‘diverging’ color scale. It looks like you use GMT for the plots, for which many good diverging color palettes can be found here:

<http://soliton.vm.bytemark.co.uk/pub/cpt-city/jjg/cbcont/div/index.html>

We used “MATLAB” software for all plots. We will try to enhance the colour scale.

2. It may also be a good idea to contour areas with significant correlations

A contour delineating the significant correlations will be added to the figures

3. Some figure captions can be expanded to describe the followed procedure. For example figure 4: “The correlation pattern between de-trended sea level during the winter months(DJFM) and the de-trended NAO index over the same months. The correlation has been computed between January 1993 and December 2014” or something similar. This will allow easier reproduction of your results.

We will add expanded captions to the figures.

Figure 1: Maybe add the locations of the tide gauges

We will add the locations of the tide gauges.

Figure 10: On the left, some data seems to be missing

To compute the correlation coefficients, we prescribed a threshold of 75% data availability for the considered period.

REFERENCES (in addition to reviewer’s references)

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