General

The authors propose a new proxy index for large-scale pressure variability over Western Europe that explains a large fraction of sea level variability. They argue that this index, which is named the 'Baltic Sea and North Sea Oscillation' and corresponds to the air pressure difference between mid-Scandinavia and the Bay of Biscay, explains a larger fraction of sea level variability in the Baltic Sea and the German Bight, compared to the North Atlantic Oscillation index. Using a regression analysis, they argue that the inverted barometer effect explains the majority of the observed correlation pattern, and that the net energy and freshwater flux though the sea surface explains an other part. Finally they argue that Ekman transport generated from the atmospheric pressure pattern and the induced winds do not result in water exchange between the North Sea and the Baltic Sea in wintertime.

This subject is interesting, as this variability affects long-term trends and have an impact of sea level extremes. However, in my feeling, there are still substantial shortcomings, which makes the conclusions not yet convincing. I doubt whether the proposed index is novel, as the pattern has been found before, and I think the analysis of the results should go beyond showing correlation patterns, which do not prove the direction of causation. Hence, in my opinion, the paper is not publishable in its current form.

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.
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?
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.
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?
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.

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.
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).
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.

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

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?

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

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

P5L13: Altimetry data does not have a ¼ by ¼ 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 it very tricky to separate small-scale 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.

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.

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/)

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

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.

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?

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.

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?

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

P8L28: How do you define 'significant'?

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

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

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

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.

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?

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.

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

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

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.

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

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

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

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?

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.

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 ~ Q_net instead of SL ~ Q_net. What if you integrate Q_net before computing any correlation?

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

P15L25 and below: How did you compute this? If I'd compute the thermal heating that result of the aforementioned number, the whole Baltic sea would evaporate rather quickly. Are there in-situ 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?

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 ~ 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?

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.

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

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.

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 regionalocean reanalysis could bring more trustworthy results regarding changes in wind-induced transport and sea level variability.

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.

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.
- 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!
- Conclusion #3: There are essentially two indices: summer-BANOS and winter-BANOS.
- Conclusion #4: The BANOS index only correlates with sea level variability in the German Bight, and not in the whole North Sea in summer.
- 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.

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

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

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.

Figure 1: Maybe add the locations of the tide gauges

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

References

Calafat, F. M., Chambers, D. P., & Tsimplis, M. N. (2012). Mechanisms of decadal sea level variability in the eastern North Atlantic and the Mediterranean Sea. Journal of Geophysical Research: Oceans, 117(C9).

Calafat, F. M., Chambers, D. P., & Tsimplis, M. N. (2012). Mechanisms of decadal sea level variability in the eastern North Atlantic and the Mediterranean Sea. Journal of Geophysical Research: Oceans, 117(C9).
Chen, X., Dangendorf, S., Narayan, N., O'Driscoll, K., Tsimplis, M. N., Su, J., ... & Pohlmann, T. (2014). On sea level change in the North Sea influenced by the North Atlantic Oscillation: local and remote steric effects. Estuarine, Coastal and Shelf Science, 151, 186-195.

Dangendorf, Sönke, et al. "Characteristics of intra-, inter-annual and decadal sea-level variability and the role of meteorological forcing: the long record of Cuxhaven." Ocean Dynamics 63.2-3 (2013): 209-224.

Dangendorf, S., Wahl, T., Nilson, E., Klein, B., & Jensen, J. (2014a). A new atmospheric proxy for sea level variability in the southeastern North Sea: observations and future ensemble projections. Climate dynamics, 43(1-2), 447-467.

Dangendorf, S., Calafat, F. M., Arns, A., Wahl, T., Haigh, I. D., & Jensen, J. (2014b). Mean sea level variability in the North Sea: Processes and implications. Journal of Geophysical Research: Oceans, 119(10).

Frederikse, T., Riva, R., Kleinherenbrink, M., Wada, Y., Broeke, M., & Marzeion, B. (2016). Closing the sea level budget on a regional scale: Trends and variability on the Northwestern European continental shelf. Geophysical Research Letters, 43(20).

Le Traon, P. Y., Dibarboure, G., & Ducet, N. (2001). Use of a high-resolution model to analyze the mapping capabilities of multiple-altimeter missions. Journal of Atmospheric and Oceanic Technology, 18(7), 1277-1288.

Pujol, M. I., Faugère, Y., Taburet, G., Dupuy, S., Pelloquin, C., Ablain, M., & Picot, N. (2016). DUACS DT2014: the new multi-mission altimeter data set reprocessed over 20 years. Ocean Science, 12(5), 1067-1090.

Sterlini, P., de Vries, H., & Katsman, C. (2016). Sea surface height variability in the North East Atlantic from satellite altimetry. Climate Dynamics, 47(3-4), 1285-1302.

Xu, Q., Cheng, Y., Plag, H. P., & Zhang, B. (2015). Investigation of sea level variability in the Baltic Sea from tide gauge, satellite altimeter data, and model reanalysis. International Journal of Remote Sensing, 36(10), 2548-2568.

Yan, Z., Tsimplis, M. N., & Woolf, D. (2004). Analysis of the relationship between the North Atlantic oscillation and sea-level changes in northwest Europe. International Journal of Climatology, 24(6), 743-758.