The manuscript is focused on the amplitude of decadal and multidecadal variability of sea-level in the North Sea and its connections to the atmospheric forcing. The background motivation is to set a backdrop to frame the observed long-term trends of sea-level rise. The main conclusion is that wind forcing and sea-level-pressure variability are important contributors to the decadal sea-level variability, and that this variability is larger in the winter season. Methodologically, the study tries to separate the local atmospheric forcing from the impact of large-scale atmospheric patterns, and concludes that the relative importance of these two factors varies regionally. Also, the relative impact of the large-scale atmospheric patterns can be different depending on the location of the tide-gauge.

My overall impression of the manuscript is positive. It is in general clearly written and well structured. The motivation and conclusions should be interesting also for a wider readership. I have only some minor comments that the authors may want to consider in a slightly revised version.

1. My most important and more general comments pertains the separation of the atmospheric drivers between local and large-scale atmospheric patterns. I guess that both are not totally independent, at least the statistical models used do not explicitly attempt to separate the influence of both. Clearly, the large-scale patterns, such as the NAO, are also related to local changes in sea-level-pressure and local wind-stress, so that the amount of variances explained by local and large-scale patterns partially overlap. Could this overlap be quantified, or could these factors be statistically separated in a statistical model that uses all predictors simultaneously?
   The local and large-scale models are indeed not independent: the large-scale atmospheric patterns and local wind and pressure changes are linked. Therefore, the vast part of the local wind and pressure changes can be explained by the large-scale patterns. The variability that is explained by the local wind and pressure changes after removing the variability resulting from the large-scale atmospheric variability is negligibly small. We have added an extra remark to make explicitly clear that large-scale atmospheric patterns and local wind and pressure are related quantities:
   P2L22: These indices characterize the prevailing large-scale patterns in the atmosphere, but on a regional level, they translate into regional wind- and sea-level pressure changes that induce local sea-level variations.

2. Page 2, line 10 The sea-level response to atmospheric pressure often deviates from the inverse barometer effect (Woodworth, 2017b). I guess the authors mean a purely equilibrium inverse barometer effect, and that this deviation is caused by the time scales considered here (seasonal means) that indicate that the response of sea level to changes in atmospheric pressure do not reach equilibrium. Is this the cause or are there other causes? The authors may want to help the reader here.
   Yes, we indeed meant that around the North Sea, the local relation between sea level and local atmospheric pressure is often different from what would be expected from the static equilibrium expected from the inverted barometer effect. The most likely cause for this departure is that pressure changes co-vary with local and remote winds, precipitation etc., which also affect sea level via surges, for example (Woodworth 2009). Since the adjustment time scale for the IB effect is generally much shorter than a season (Wunsch and Stammer, 1997), it is unlikely that this departure is linked to the time scales we are interested in. We have added an explanation on the possible reasons for this departure:
   P2L13: It is known that the sea-level response to local atmospheric pressure variability along the British coast often deviates from the equilibrium inverse barometer effect, which has its likely cause in factors that co-vary with atmospheric pressure, such as wind-induced surges (Woodworth et al., 2009; Woodworth, 2017b).

3. Page 10 line 5 There is a duplication in this paragraph 'generally only explain a small part of the variability explain'
Fixed

4. Page 8, line 1 It must be noted that the principal components computed here, are not fully interchangeable ... Delete comma after 'here'
Fixed

5. Figure3. The plots show the gliding linear trends. Actually, these plots do not only show the variability of the trends but also the long-term acceleration (or lack thereof). According to these plots, only the winter records of Oslo and Cuxhaven show an acceleration of the sea-level rate, whereas all for other records the rate is statistically flat. Is this interpretation correct? If yes, it may be worth a short comment. Also related to this, it may be worth to include in the plot error bars in the estimation of the trends. These error bars would be dependent on the period and record, but to include a shading around the main lines could clutter the plot. My suggestion would be to include a typical errorbar as side vertical segment just as a guideline for the reader.
Thank you very much for bringing up this comment and number 6. The plot indeed suggests accelerations in seasonal sea level for some stations, which to a large extent disappear when removing the atmospheric forcing. Note that we do not try to explain sea-level accelerations in this paper, but we fully agree that this point must be discussed. We have chosen not to quantify the effect on sea-level accelerations, but to note that the low-frequency variability could be interpreted as an acceleration, and removing the wind and pressure effects reduces this low-frequency variability, and thus the possible accelerations induced by this variability. We have added remarks about this acceleration in seasonal sea level, and the effects of removing the atmospheric forcing.
P7L3: This variability in seasonal deviations could also be interpreted as an acceleration: for example, the trend in winter sea-level deviations in Oslo and Cuxhaven is generally higher in the last few decades than in the first few decades, which could translate into a long-term positive acceleration in the seasonal sea-level deviation time series.
P12L17: For all seasons, the local and large-scale regression model explain a large fraction the running-mean trends. For winter and autumn, after applying the regression models, the residual seasonal trends have the same order of magnitude as typical secular mean sea level trends, and the trend differences between the beginning and end of the considered periods have been substantially reduced. However, not all low-frequency has been explained by the models, and the residual time series still contain trends and accelerations.
Furthermore, to avoid that the trends in Figure 3 are mis-interpreted as long-term trends and accelerations in annual sea level, we added an extra remark:
P7L6: Again, note that these trends are trends in seasonal deviations from annual-mean sea level, and they don't represent secular trends in sea level.
About the uncertainty estimates: we have added a shading to Figures 3 and 9, which now shows confidence intervals from the estimated trends under the assumption that the residual can be described by an AR1 process. For Figure 9, to prevent clutter, we have split up the Figure into an upper and lower row. We have added a short remark on how these trends are computed to the Data and Methods section:
P6L3: To estimate trends in seasonal sea-level deviations, we use the Hector software (Bos et al., 2013), which computes the trend and the 1-sigma confidence intervals under the assumption that the residuals can be described by a first-order autoregressive (AR1) process.
We added a short sentence to discuss the reduction of the confidence intervals that results from applying the regression model:
P12L17: For winter and autumn, after applying the regression models, the residual seasonal trends have the same order of magnitude as typical secular mean sea level trends, and the

trend differences between the beginning and end of the considered periods have been substantially reduced.

6. Figure 10 Related to my previous comment, it seems that after removing the impact of atmospheric forcing, only the Oslo winter record show a long-term acceleration. Would this be statistically significant ? would it be worth mentioning ?
   Please see our remarks at point #5, which also handles this point.