Response to Referee Comment #2 (Stefan Talke)

First of all, the authors would like to warmly thank Dr Stefan Talke for his careful reading of the paper and his many constructive comments. We tried to do our best to implement them, and they allowed to greatly improve the manuscript.

The reviewer comments are in bold, our replies are in normal font.

Summary: The manuscript "Climate-scale changes of the semidiurnal tide over the North Atlantic coasts from 1846 to 2018", by Pineau-Guillou et al., evaluates 9 long tide data sets on both sides of the Atlantic to investigate whether there is evidence for basin-scale perturbations to tides caused by climate variations or climate change. It is found that M2 for 3 gauges in the northeastern Atlantic follow a similar pattern as the decadally filtered NAO index. No clear correlation is found in the western Atlantic. It is noted, however, that rates of M2 change go negative at many of the 9 stations after around 1990.

Evaluation: It is an interesting idea to try to discern whether there are coherent, basin-scale variations in M2 in the northern Atlantic, and one that has proved challenging to find in the past (to my knowledge). It would be quite an interesting result if M2 patterns caused by climate variability only show up in long data sets.

We thank the reviewer for this comment. We agree that the idea of discerning some coherent basin-scale variations in M2 is interesting, and challenging. We changed the title of the paper (as suggested by the Editor), and replaced "Climate-scale changes" by "Large-scale changes".

Therefore, if properly done, it would be interesting to convincingly prove (or disprove) the hypothesis that (for example) long-term NAO patterns affect tides coherently. However, as presently conceived and presented, am not convinced that the authors have really shown that this is occurring (or disproven it).

We share the reviewer comment, and we analyzed more deeply the data to go further in the hypothesis. We developed a new part related to the statistical analysis, which led to new results. We also added new stations. More details are in the following.

Issues include:

1. There is no attempt at a statistical correlation between the tide records and climate records such as the NAO index. While the tidal records in Europe superficially follow a similar trend as the filtered NAO, it is quite possible for data sets with few degrees of freedom (here, a decadal median) to resemble each other by random chance. For this reason, it is important to do some sort of significance testing and report statistics such as R2 and the p-value. Similarly, would be good to verify that the 6 US East coast records show a statistically insignificant correlation.

We agree with the reviewer, and we made a statistical analysis of the data.

1) We computed the correlation between normalized M2 variations and climate indices (NAO and AO). To be consistent, we filtered out M2 variations on the same time window as NAO and AO (9 years). We computed the correlation since 1910, to have similar periods for all the stations. We considered that the correlation was significant only if the p-value was lower than 0.05 (95% statistical significance). The results are the following: for NAO only, 10 stations out of 12 show significant correlation. We found the strongest positive r-value in the North East Atlantic (Cuxhaven, Brest, Newlyn), with a maximum of 0.58 at Cuxhaven. This confirms the possible causal relationship between M2 variations and NAO, as suggested in the paper. We also found a strong anti-correlation with Halifax (-0.55). For AO, we found similar, but overall larger, r-values. This is not surprising as these two indices are highly correlated. We propose to add a figure showing the r-value at all the stations with (a) NAO and (b) AO.

Note that at Brest the data record starts in 1846. The correlation with NAO is significant from 1910, but not from 1864 (NAO index used in this study starts only in 1864). This can be explained by the M2 larger amplitude over all the XIXth century, which decreases between 1890 and 1910 (Figure 3a). This inconsistency was already noticed by the anonymous reviewer #1. However, the construction of dykes that partially closed the harbor of Brest since the end of the XIXth century may have altered the tide. To go further, the potential role of these successive constructions needs to be investigated (https://en.wikipedia.org/wiki/Brest_Arsenal). Cartwight (1972) made a first attempt to evaluate the influence of reducing the width of access to the harbour but did not take into account a potential role of dredging for which we have no information. This example underlines the complexity of interpretation of the variations when local and large-scale changes occur at the same time.

In the following, M2 variations, MSL and NAO are filtered over 9-year time windows and normalized. MSL are now corrected for land movement (estimations from SONEL website), which led to more consistent MSL trends at the basin scale.

2) We computed the r-value between M2 variations and two linear regression models. First, we fitted M2 variations with MSL variations only (model 1). We then computed the residual (M2 variations – model 1), and fitted this residual with a NAO linear model. The objective is to estimate the relative contribution of MSL and NAO in M2 variability. Models 1 and 2 may be expressed as:

$$\text{Model 1} = \alpha \text{MSL}$$
$$\text{Model 2} = \alpha \text{MSL}+\beta \text{NAO}$$

We checked that there was no significant correlation between NAO and MSL at the stations (there is no correlation at 7 stations, and the r-value is between 0.2 and 0.4 at 5 stations; note that there is no NAO-MSL corrrelation at the stations in the north (Halifax, Brest, Cuxhaven, Newlyn), where we found a significant NAO contribution compared to MSL - see below).

The three main results of this analysis are the following: the first result is that at first order, M2 varies with mean sea level (strong r-value for model 1). The second result is that the introduction of NAO in the model (model 2) allows to systematically increase the correlation (stronger r-value for model 2 than for model 1). This confirms that NAO-related mechanisms may explain part of the variability of M2. At some stations, this increase is quite large. For example, at Cuxhaven, the r-value is 0.65 for model 1, but reaches 0.85 for model 2. The third result is that we can estimate at each station the relative NAO contribution (compared to MSL) in M2 variability. Indeed, as in model 2 MSL and NAO are normalized, the ratio β/α represents roughly this relative NAO contribution. We found a significant NAO contribution at some stations (e.g. more than 30% at Halifax), whereas negligible at others (e.g.

5% at Eastport). Values suggest that the northern part of North Atlantic is more sensitive to NAO, with quite similar values. This suggests a possible basin scale coherence, with correlation on the northeast side, and anti-correlation on the northwest side. We propose to add in the paper a table including the r-value for models 1 and 2 and the ratio β/α. We also propose to add a figure with M2 variations, model 1, and model 2 at the stations where the correlation with model 2 is significant (r-value>0.3) and the NAO contribution significant (β/α>0.25). This new figure shows that the model 2 better captures the M2 variations than model 1.

2. Overall, it would seem to me that the data selection is incomplete. On the European side, there are some Dutch coastal records that predate 1920, such as Hoek van Holland and Delfzijl (maybe also others; perhaps check if the GESLA data set has them). Similarly, there are additional records on the US East Coast that could be used. These include Fernandina (1897-present; available from NOAA) and Sandy Hook (Available since ∼1910 from NOAA) (Note that Baltimore (1902-present) and Philadelphia (1901-present) also exist, but are not coastal stations). Moreover, there are a number of M2 estimates extending into the 1800s in New York Harbor (Talke et al. 2014 supplement and Chant et al., 2018), Boston (Talke et al., 2018), Eastport (Ray and Talke, 2019), and Long Island Sound (Kemp et al., 2017). Some additional estimates of tidal range at coastal stations deep into the 1800s are found in Talke & Jay (2020). These can be used as a proxy for M2 if divided by ∼2.

We agree with the reviewer that it would be interesting to analyze more stations, and consequently we have changed our criteria of selection to extend the number of tide gauges. The stations suggested by the reviewer are missing because they did not match the criteria for tide gauge selection (see paragraph 2.2.1): we selected tides gauges from UHSLC, with time series starting before 1920, with at least 80 years with data, and a significant tide (M2>10 cm). For these reasons, the following stations were not selected:
- Hoek van Holland and Delfzijl are not in the UHSLC sea level database; note that they are in the GESLA dataset only for surges (not for sea levels). However, as we could easily download French stations online (data.shom.fr from the French Hydrographic Office), we analyzed 2 other stations from the North Sea (Calais, Dunkerque) – the results are provided later in this document.
- Fernandina is in the UHSLC database, but with only 62 years of data (<80 years), and over two distinct periods (1898-1923 and 1985-2018). For this reason, this station was not selected.
- Sandy Hook and Long Island are not in the UHSLC database. However, New York has been selected (see below), and as these 3 stations are very close together (<80 km). New York can supplement the study by covering this area.

New-York, Eastport and Boston are in the UHSLC, but starting respectively in 1920, 1929 and 1921. Following the reviewer’s suggestion, we changed our criteria (times series starting before 1930 instead of 1920), in order to include these three stations. Note that Pensacola (in the Gulf of Mexico) and Tregde (in the North Sea) also start before 1930 (respectively 1923 and 1927), but were discarded due to the small tidal amplitude (M2<10 cm).

Finally, three stations were added – New-York, Eastport and Boston – leading to a total of 12 stations instead of 9. We also added values of M2 in 1862 at Eastport (from Talke and Ray, 2019), and New York (from Talke and Ray 2014). Note that this 1862 value was estimated from the supplementary Fig. S20. For Boston, there was no M2 value in Talke et al. (2018), however we mentioned the observed decrease of M2 between 1870s and 1920s (Talke et al., 2018).

We have added in the limitations and perspectives, that other possible relevant stations could be analyzed, provided different selection criteria are adopted, among them on the US coast Sandy Hook and Long Island Sound (Kemp et al., 2017), and in the North Sea Hoek van Holland and Delfzijl.

3. Further, it's not clear how significant the data cut-off of 1920 is. If 1930 or 1940 were used, for example, how might results or conclusions change? A later cutoff would enable inclusion of a number of additional US East Coast stations (e.g., Mayport, Fort Pulaski, Sewells Point, Willets Point, Providence, Eastport, etc). Perhaps the same is true for Europe. How much would conclusions or patterns change if a slightly less restrictive date-cutoff were used? It would be good to use a statistics-driven reason for the date cutoff (e.g., degrees of freedom in a correlation analysis, or something like that), and to check the effect of relaxing the cutoff. Regardless, my guess, based off of Figure 2 of Talke & Jay 2020, is that patterns of M2 change might be even less coherent if more data are used. This is in part because there are so many local processes that can affect tide gauges, even those at or near the outlet of an estuary (for example, Mayport, Lewes, Charleston, Fernandina, Sewells Point, Boston, and Sandy Hook in the US; Cuxhaven in Germany; and the Saint John gauge in Canada are all "coastal" gauges that are actually at the mouth of or within an estuary).

The effect of relaxing the cut-off has been tested (1930 instead of 1920) and even adopted (see above point 2.). This conducted to add three stations in the North West Atlantic coast (New-York, Eastport and Boston). The M2 variations at these stations are very similar to the ones of North West Atlantic with positive trends, i.e. Portland, Charleston, Key West (Figure 3 (b)). Similarly, for these 3 new stations, M2 decreases since 1980, and then increases since 1990, particularly for New York. We propose to update Figure 3 (b) with these 3 new stations. We also propose to update the figures with the trends (Figures 4 and 5).

On the other hand, there is an interesting and not completely explained decrease in tidal range and M2 at Sandy Hook, The Battery, and Boston from the mid and early 1800s until the 1920s, and then an increase. In other words, it's possible there is a larger Northwest Atlantic Signal, in addition to local processes. However, while models such as Schindelegger et al. 2018 are able to see coherent trends, it has been challenging to see it in the data (as mentioned in this manuscript as well, also in Ray 2006).

Yes, we added on Figure 3 (b) values of M2 in 1862 at Eastport (from Talke and Ray, 2019), New York (from Talke and Ray 2014), and mentioned at Boston the observed decrease of M2 between 1870s and 1920s (Talke et al., 2018). The normalized values in the XIXth at Portland, Eastport and New York are close together.

4. A similar data comment holds for the trend-switch which is observed around 1990. It would be quite interesting if this is a coherent signal throughout the Atlantic. To prove, would suggest that there are many more gauges that are available that could be used to test this hypothesis. Again, a trend switch in 6 out of 9 gauges need not be statistically significant or could be argued to be local in nature.

We agree with the reviewer that it would be interesting to find a coherent signal throughout the Atlantic. With the new data (see previously point 2.) and new statistical analysis (see previously point

1.), we are now able to partly interpret this trend-switch since 1990. The switch occurs in stations that are significantly influenced by NAO (high ratio β/α). In contrast, the stations that are not influenced by NAO - but mainly by MSL – show no switch, and even an increase of their trend (acceleration).

However, would be more significant if you had 30 or 40 gauges and found a similar percentage shift between the 1950 to 1990 trend, vs. 1990 to the present, That would be quite interesting.

We agree with the reviewer that it would be interesting to have more tide gauges. However, this would lead to add short records, whereas the present paper focuses on long-term records starting no later than 1930 and with at least 80 years with data. Moreover, the study would then be closer to Müller (2011), who selected tide gauges with at least 35 years of data prior to the year 1980, leading to 17 stations. Finally, shorter series are affected by stronger correlations between the NAO and the MSL (as they increase since 1960, see below the case studies of Calais and Dunkerque), which is problematic for the statistical analysis, when fitting model 2 to distinguish the influence of the MSL from that of the NAO. From 1910, only 5 stations among 12 show a significant correlation between MSL and NAO (on average, r=0.28), whereas from 1960, this figure jumps to 9 stations, with higher r-value (on average, r=0.40).

Also, the effect of moving the date (1985 or 1995 instead of 1990) might be worth investigating. If results depend on start date, the interpretation becomes less clear (and vice versa).

As suggested by the reviewer, we investigated the effect of moving the date (1985 or 1995 instead of 1990). This does not change significantly the results. When we computed recent trends (from 1990) instead of long-term trends (since 1910), 5 stations showed a trend-switch (Newlyn, Brest, Cuxhaven, Halifax, Charleston). Moving the date from 1990 to 1985 leads to similar result: 3 stations show the same trend-switch (Newlyn, Cuxhaven, Halifax), and 2 stations show a significant decrease in the trend – but not enough to be a switch (from 0.13 to 0.01 mm/yr at Brest, from 0.32 to 0.06 mm/yr at Charleston, between 1910-trend and 1985-trend). Moving the date from 1990 to 1995 leads also to similar results: 4 stations show the same trend-switch (Brest, Cuxhaven, Halifax, Charleston), and 1 station shows a significant decrease in the trend – but not enough to be a switch (from 0.14 to 0.04 mm/yr at Newlyn, between 1910-trend and 1995-trend). As a consequence, the interpretation of the results is not highly sensitive to the start date. We propose to add a short comment on this robust aspect in the the manuscript.

5. The paper would also be improved by digging more deeply into mechanisms. Some discussion of how the NAO affects sea-level (and therefore, perhaps M2) is made, but it is quite qualitative. There are, however, a number of process-based studies that look into tidal changes at local, regional, and oceanic scale (see the Haigh et al. 2020 and Talke &Jay, 2020 reviews for references). The Atlantic is known to be near resonance, and there could be coupling between the shelf and the deep ocean (Arbic and Garret papers). Tide changes in the Gulf of Maine (and for that matter, Long Island Sound) can in theory radiate out to the larger ocean (e.g., Godin 1993). Also, it is known that the M2 amphidrome in confined, shallow seas moves with sea-level changes (see the references in the Haigh et al. review, or the Lee et al. 2017 and Ross et al. 2017 papers on the Chesapeake). Based on this, also perhaps on basin scale modeling (e.g., Schindelegger et al. 2018), what sorts of coherent patterns might be expected in the Atlantic based on historical sea-level rise? How much might this be spatially variable based on sea-level change caused by long-term NAO patterns? What might be the magnitude of the signal? The Schindelegger et al. 2018

paper shows that coherent changes across the basin are possible with sea-level rise, but are relatively small, for a given increment of sea-level change. Also, they show that some locations in the Atlantic are anti-correlated. How much of a sea-level change is needed, roughly, before a coherent longterm signal is findable in tide gauge data (given noise in data, etc)? One needs to know whether it's even possible (by the mechanisms listed) to obtain a secular coherence in basin-scale variability.

We have added the shifting locations of amphidromic points under SLR scenarios (Pickering et al. 2017, Idier et al. 2018, Haigh et al. 2020) – see reviewer comments below, lines 182 and 218-219 from submitted paper. We also have substantially rewritten the paragraph on the impact of MSL rise on tide – see reviewer comment below, line 226 from submitted paper.

We share the questions of the reviewer, but it is challenging to determine what sorts of coherent patterns might be expected in the Atlantic caused by long-term NAO changes, only from papers on MSL rise effect on tide (e.g. Schindelegger et al. 2018). Note that Schindelegger et al. (2018) show that the sign of the M2 change is correctly reproduced at 80% of the tide gauges, but trend values tend to differ by a factor 3 to 5. Moreover, the response to a 0.5 m MSL rise is opposite between Cuxhaven and Brest (see Figure 6 from Schindelegger et al. (2018)), whereas the observed changes are of the same sign (positive) at these two stations (Figure 6, red dots, or Figure 3 (a) from our paper).

We conducted further investigations to test if the magnitude of sea-level pressure changes induced by large-scale atmospheric circulation (Figure 9, few tens of hPa) can generate the observed decadal-scale M2 changes (few cm). Note that we now express the Figure 9 in terms of hPa (and not cm). It is directly the difference of winter sea-level pressure between a NAO+ year (1989) and NAO- year (1969). Note also that M2 changes due to large-scale atmospheric circulation are only a small part of the total observed changes (few cm), the changes being also due to MSL rise (see the statistical analysis above).

The underlying mechanism invoked in the present paper (i.e. the influence of the atmospheric circulation on the tide) is very close to the one in Huess and Andersen (2001), except that we are at a larger time scale (decadal instead of seasonal). Huess and Andersen (2001) explains partly M2 seasonal variations through the effect of atmospheric circulation. They run a barotropic model in the North Sea, forced (1) with tides only and (2) with both tides and meteorological fields. Results show that the seasonal modulation is better captured when the model is forced with both tides and meteorological fields (Plate 2, top right, amplitude higher that 10 cm in the German Bight) rather than with tides only (Plate 2, top left, amplitude lower that 5 cm in the German Bight). It is important to underline that the model is barotropic, and that there is no effect of stratification, which may also play a role in M2 changes (see 3.3.6 in the review of Haigh et al, 2019).

At seasonal scale (instead of decadal scale, in the present paper), we computed monthly (instead of yearly) M2 variations at Cuxhaven over 5 years (2010-2015). Results show a seasonal cycle with a range of around 15 cm, maximum in summer, and minimum in January (which is consistent with Huess and Andersen (2001)). Similarly to Figure 9 in our paper (now in hPa), we computed the difference of monthly sea-level pressure between January and July (sea-level pressure data come from NOAA 20th-Century Reanalysis, Compo et al. 2011). We obtain values very close to the ones in Figure 9 (few tens of hPa). This shows that the order of magnitude of sea-level pressure changes (few tens of haPa) is consistent with M2 observed changes at Cuxhaven (few cm). The assumption is not strictly proven, but provide reasonable new insights worth to be brought to the attention of the community for further investigations. As mentioned in the paper, dedicated simulations should be conducted to go further, and confirm or discard this hypothesis.

In other words, a more clear hypothesis of what a basin scale shift in M2 tides might look like and whether it is detectable (given current understanding of processes) might help with the interpretation. What sort of excursion in M2 would you expect the NAO to cause, based on how it affects sea-level? Setting up a hypothesis with specific criteria that can be proven or disproven might help. It's ok, in my opinion, to have a paper with a non-detect result (a possible outcome here). However, since the set of non-detect papers is infinitely large, the added value could come from adding scientific or statistical insight into the problem. A great example of this is the Haigh et al. 2014 paper which showed that one would need to wait a couple decades before being able to analyze recent sea-level acceleration at an acceptable level of confidence. Is something similar true here? My qualitative guess is that it might be hard to see a basin scale coherence in data, but that regional scale effects that are driven by a similar process like sea-level fluctuations can perhaps be detected (see for example the Devlin et al. Papers). Basically, the paper would be improved by more specifically investigating what sort of change is needed (and what sort of data quality/signal to noise is needed) before it might become possible to discern coherent climate effects on tides across the entire basin.

We have added in the paper some new insight from the statistical analysis. M2 changes are correlated at first order with mean sea level rise, and at second order with NAO, but only at some stations. These are mainly located in the northern part of North Atlantic. There are correlated on the northeast side, but anti-correlated on the northwest side. This suggests a basin scale coherence in the data. Dedicated simulations should be conducted to go further on this hypothesis.

Detailed comments:
Line 14: Would also cite the review of Talke & Jay, 2020, since the historical changes in tidal range shown therein are relevant to this paper.
Yes, we added the reference Talke & Jay, 2020.

Line 19: "Long-term changes in tidal constituents are rather small" Would modify this to specify "at coastal stations". As shown in Talke & Jay 2020 (and refs therein), the secular change at many estuary and tidal river gauge stations is huge.
Yes, we added "at coastal stations". We also mentioned that changes are larger in many estuaries and rivers, referencing to Talke & Jay 2020.

Line 20 "still poorly understood"– Not sure I would say this. Some of the mysteries are being solved (see the review papers), while some issues remain. Maybe rephrase?
Yes, we rephrased. The physical causes of these changes are generally difficult to understand. The complexity comes first from the combination of local and regional changes. Moreover, regional changes may be a combination of different processes, largely dependent from each other, and which interact together – it is then challenging to identify separately which are the processes at the origin of the changes, and in which proportion are they contributing.

Line 46-48: Check grammar; grammar of list Is not quite right.
Yes, the list has been correctly rewritten.

Line 60-63: There are some M2 results for 19th century US stations that you could/should use. See for example the supplement of Talke et al., 2014 or Chant et al., 2018 for New York and Sandy Hook. See Ray & Talke (2019) for Eastport and Portland. See also Talke et al., 2018 for Boston. Finally, there are multiple tidal ranges shown in the Talke & Jay 2020 review paper. These can be divided by two to get an estimate of M2 over time.

The value of M2 at Portland from Ray & Talke (2019) was already added in the paper (blue star on Figure 3 (b)). Following the reviewer suggestion, we also added New York, Eastport and Boston, by relaxing the criteria of selection of stations (we now select stations with time series starting before 1930, instead of 1920). As mentioned previously, we also added values of M2 in 1862 at Eastport (from Talke and Ray, 2019), New York (from Talke and Ray 2014), and we mentioned the observed decrease of M2 at Boston between 1870s and 1920s (Talke et al., 2018).

Also, Sandy Hook data from around 1910 is available at the NOAA site. The datum is wrong, but that shouldn't matter for tidal analysis.

Sandy Hook is very close to New York which has been included in the study (see above a previous response). We have added in the limitations and perspectives, that other relevant stations could be analyzed, among them Sandy Hook.

Table 1: Since you are using Cuxhaven, why not also use some of the Dutch stations? In the records I have, Hoek van Holland starts in 1900, and Delfzijl starts in 1876. Maybe there are earlier ones as well, see for example the Hollebrandse 2005 thesis. You could check if they are in the Gesla dataset and/or contact the Dutch. Data used to be available at waterbase.nl, but not sure that works anymore.

As mentioned previously, Hoek van Holland and Delfzijl are not in UHSLC database, and not in the GESLA database. Figure 1 shows that there is no Dutch data in UHSLC dataset. However, we agree that having more points in the North Sea would be interesting. As we can easily download French stations online (data.shom.fr), we led a similar analysis on Calais and Dunkerque tide gauges, located in the North of France (North Sea). Calais starts in 1941, with only two years 1941-1942, and then a gap until 1965, and data from 1965 up to now. Dunkerque starts in 1956. The results confirm that the variations at these two stations are similar to the variations at the 3 other stations in the North East Atlantic (Newlyn, Brest, Cuxhaven, Figure 3(a)). M2 increases from 1960 to 1990, and then becomes more steady since 1990. Similarly to the three other stations in North East Atlantic, the trends are decreasing when they are computed only since 1990. However, the main difficulty with these short time series (since 1960) is that the correlation between NAO and MSL is significant, as over this period (1960-2018) NAO and MSL are increasing together. For example, at Dunkerque, the correlation coefficient is 0.53. It is then not possible to fit model 1 and model 2, in the statistical analysis assuming them independent. For this reason, we choose not to include in the paper these short records. However, we propose to mention the consistency in the variations of M2 between Calais Dunkerque and the other stations over North East Atlantic.

Table 1: Why is Fernandina (1897-present) not used?

As mentioned previously, Fernandina does not match with our selection criteria: this station has only 62 years with data, our criteria being at least 80 years with data. Moreover, data cover two distinct periods (1898-1923 and 1985-2018).

Table 1: Unclear what the meaning of mean sea-level is. What is the datum? Why not include the trend, rather than an absolute measurement (which is not necessarily meaningful).
The MSL is referenced to an arbitrary reference. As mentioned by the reviewer (and also by the Editor), this is not necessarily meaningful. This column has been deleted.

Line 69: "This constraint resulted in excluding between 1 and 9 years". Unclear what you mean. You mean for each station?
Yes, we added "for each station".

Line 70 seasonal variation: where? Again, non-coastal stations will see more variability. Also, in the North Sea the change is higher (e.g., Graewe et al. 2014). So, maybe be specific and mention the Atlantic.
The seasonal variation is significant in coastal areas and polar regions (Müller et al. 2014). In the North Atlantic, the largest values are over the North East Atlantic (English Channel and the North Sea, see Figure 3 in Müller et al., 2014). As suggested by the reviewer, we have added "in the North Atlantic", and also referred to Gräwe et al. (2014).

Line 74 to 91: This would seem to be pretty standard nodal correction theory. Unless you can explain what is unique about your approach, would suggest greatly condensing this and simply citing an older study that discusses this in more detail
We have moved the technical details concerning nodal corrections to an Appendix.

Line 98-110: What is the rational for using the more complex method here? As you later state, it doesn't lead to significantly different results. I guess it's interesting that it can work for small time series. Do you see any evidence of changes to nodal cycle? There are a few papers on this recently. However, am not convinced that there is a physical reason for these observations, vs. just statistical noise. Could be something to look into, though if there is a coherence between nodal cycle variations in the western and eastern Atlantic, would be worth commenting on. Otherwise, not sure that you need the complex approach to nodal cycle characterization.
As underlined by the reviewer, the main interest of this approach is that it can work with short time series. Following the reviewer's suggestion, we simplified this part on nodal modulation, and have moved it to an Appendix.

Line 117, Equation 5: Can you explore/motivate the use of the standard deviation a bit more? A potential issue is that sigma may also reflect errors in the gauge data (e.g., timing errors, etc). Some exploration would be good as to whether this is a factor. For example, does sigma change as a function of time? If there is a decrease in sigma around 1990 or 1995 when new digital gauges started being used, at least in the US then it might indicate that instrumental issues are potentially affecting your results. See for example Zaron & Jay, 2014, who concluded that some constituent trends in the Pacific are spurious. Using some sort of method to validate the causes of sigma would therefore be good. How can we be sure that a few years of non-optimal data are not biasing sigma? The method of Zaron & Jay, 2014 could be used, or the method used by Talke et al. 2018 to assess timing errors could be used (see their supplement).

Here, we used the standard deviation computed over the period 1910-2010 only to normalize the data, in order to compare all the stations together (i.e. on the same figure). This allows to clearly see on

Figure 3 (b) the change that occurs around 1980 (M2 decreases over the period 1980-1990 at the 6 stations – note that now we have 3 more stations).

We did not investigate the changes of standard deviation with time, but we agree with the reviewer that this kind of exploration would be interesting. However, here, the sigma is used only in order to normalize M2, so any bias of the sigma due to few years of non-optimal data should not impact the results of the study.

Figure 2: This figure may have some educational/explanatory value, but it's not really a new result. One could consider removing.
We propose to move this figure to the Appendix, related to nodal corrections.

Line 128 maybe remove "are essential, as they"? Doesn't' really add much to sentence
Yes, this has been removed.

Line 156 "is no linear trends" should be "is no linear trend" or "are no linear trends"
Yes, this has been corrected.

Line 158 "curve is flattening" should be "curve flattens"
Yes, this has been corrected.

Line 159 Remove "yet" in "yet noticed"
Yes, this has been corrected.

Line 163-164 Not sure that the lack of an astronomical explanation automatically implies a solid earth-ocean-atmosphere coupling system cause. Am not even sure what is meant by that. A few more logical steps are needed before a reader can believe that
The sentence has been removed.

Figure 3: Charleston is a harbor city with a channel that has probably been subjected to dredging, though I haven't looked into it extensively. Can you discuss how/whether this impacts results?
Yes, we have added that Charleston has probably been subjected to dredging. Channel deepening increases the water depths, which reduces the effective drag, leading to tidal range amplification, that may be particularly large in estuaries (Ralston at al., 2019; Talke and Jay, 2020).

Line 165: The Delfzijl station starts in 1876, so would be worth comparing to Cuxhaven. It's probably somewhat impacted by long term changes to the Ems estuary tides.
As mentioned previously, Delfzijl was not analyzed (selection criteria). However, it is cited as a relevant station in the perspectives of the paper.

Then again, Cuxhaven is probably influenced by the large change to Elbe tides. See for example Winterwerp et al. 2013.
Following the reviewer suggestion, we have mentioned in the paper that Cuxhaven is located in the Elbe estuary, and that some river engineering works, as narrowing and deepening, may induced tidal amplification (Wintewerp & Wang, 2013; Wintewerp et al., 2013)

In general, there are quite a few papers out of Germany (e.g., Jensen et al. 2003, 2005 conference papers, and maybe Mudersbach et al. 2013 (?) that discuss a big increase in tidal range from about 1960 to the 1990s on the German coast. More recently, I've been told this has slowed or reversed (though I'm not sure there is a paper on that yet). Another good reference is the Hollebrandse 2005 Master's thesis on Dutch gauges.

In the present paper, variations at Cuxhaven (Germany) show an increase in M2 from 1960 to 1990s, followed by a decrease from 1990. As suggested by the reviewer, we have added that different authors noticed a similar increase of tidal range from 1960 to 1990 in the southern North Sea. Hollebrandse (2005) found a gradual increase of tidal range during the period 1955-1980 at all the stations of the Dutch coast (5 stations including Hoek van Holland) and the German coast (7 stations). Mudersbach et al. (2013) found a significant increase in M2 amplitude at Cuxhaven since around the mid-1950s.

Line 166 The Talke & Jay (2013, 2017) paper and report are good references for sea-level/tide data archaeology, as are Peauvreau 2008 and some of the papers by Marta Marcos.
Yes, we have added references to Talke & Jay (2013, 2017), Pouvreau 2008 (already cited in the paper), and Marcos et al. (2011) for data archaeology. We also have added these references line 327 of the submitted paper, in the conclusions.

Line 172 Again, note the Winterwerp et al. 2013 paper that includes the Elbe. There are probably some German references too. The Talke & Jay 2020 and Haigh et al. 2020 reviews discuss tidal resonance (see also references therein).
Yes, we have precised that the environmental setting of Cuxhaven in the Elbe estuary could introduce some amplification (Winterwerp and Wang, 2013; Winterwerp et al., 2013).

Line 182 The Ray (2006) and Ray & Talke (2019) papers discuss change in M2 trend in the 1980s in the Gulf of Maine maybe reference.
Yes, we have added that this increase in Gulf of Maine was reported by Ray (2006) and Ray and Talke (2019).

Line 186 There are many other papers that have explored Gulf of Maine resonance besides Ray & Talke. That is not perhaps the best example. See e.g. the discussion and references in the Talke & Jay 2020 or Haigh et al. 2020 review, in addition to the Garret and Godin reference.
We have also referred to the reviews Talke & Jay (2020) and Haigh et al. (2020).

General comment: The Godin 1993 reference, and also for that matter the Arbic and Garret and Arbic et al.papers, are interesting because they discuss how resonance on a small scale (Gulf of Maine, Continental Shelf) can affect the larger Atlantic. See also the Platzman papers on resonance from the 1970s. All this could/should be discussed and investigated, since it gets at the idea that there might be a mechanism through which western and eastern Atlantic tides could be coupled. Is there reason to believe there might be? In a sense, this is an implicit hypothesis that is being investigated here, through correlation with climate indices. However, it would be helpful to motivate and explore physical mechanisms as well. Further, it might be helpful to explicitly pose a hypothesis in the introduction, such as "is there any evidence for correlated/coupled changes in tides that might provide evidence for cross-Atlantic connectivity"?
Yes, the higher correlations with NAO are located in the northern part of North Atlantic, suggesting possible coupled changes between the eastern and western coasts.

Line 190-194: Why not use the Eastport data points from Ray & Talke, or at least discuss? The composite Pulpit Harbor/Bar Harbor data set might also be worth discussion. Boston is a possibility, too, though it is influenced by local processes as well (see the Talke et al. 2018 paper. . .).
The values at Eastport and Pulpit/Bar Harbor from Ray&Talke (2019) were not used because these stations were not selected. Now, we added Eastport (from Talke and Ray, 2019), New York (from Talke and Ray 2014), and we have mentioned the observed decrease of M2 at Boston between 1870s and 1920s (Talke et al., 2018).

Line 198 : It would be good to compare Atlantic City to Sandy Hook and The Battery (see Talke et al, 2014 and Chant et al., 2018). In fact, the case of Sandy Hook and The Battery/Governors Island are interesting, since there is a marked decrease from the 1860s until the 1920s or 1930s, and then an increase. Chant et al. (2018) show an even bigger change in nearby Newark Bay, though the 19th century data there are based on very short time series. In any case, the results are sort of consistent with the results at Brest. Dredging may have at least somewhat caused the 20th century amplification (see Ralston et al., 2019), and work at the channel mouth may have cause the early 20th century changes (Marmer, 1935). Also, Boston showed a similar, large decrease in tidal range through the 1920s, then an increase. While this is likely in large part local, Talke et al. 2018 did note that it’s similar to the pattern observed at Sandy Hook.
Yes, we mentioned the decrease observed between 1860s until 1920s at 4 stations.

Line 218-219: Would also look into/discuss amphidrome changes. See the Haigh et al. review and references therein.
Yes, we have added that the shifting locations of amphidromic points could also play a role (Haigh et al., 2020). In the North Sea, different authors show a possible migration of the present day amphidromes, under a 2 m sea-level rise scenario (Pickering et al., 2012; Idier et al., 2017).

Line 218-223: These are very short paragraphs and not that well developed. Some more thought would be good. For example, “The trends have to be interpreted carefully” is perhaps an obvious statement (hopefully there is not a case when it is ok to interpret trends haphazardly. . .).
We have developed the paragraph on amphidromic points (see above, reviewer comment on lines 218-219 from submitted paper), and rephrased the one on trends.

Line 226 Somewhat misleading statement. Ray & Talke (2019) are referencing other results when they state that MSL rise only partly explains trends. Furthermore, they only focused on Gulf of Maine. Would instead look into some of the studies that have more carefully looked at SLR effects, such as Schindelegger et al. 2018 or Greenberg et al. 2012.
Yes, this paragraph on possible link with mean sea level rise has been substantially rewritten.

Line 228 “than mean sea” should be “as mean sea”
Yes, it has been corrected.

Line 230-235: The Pickering papers are for large sea-level rise scenarios, but don’t retrospectively look at 20th century rise (if memory serves). Hence, is it a fair comparison? There are probably some papers or reports that discuss reasons for North Sea changes more thoroughly please look into and review.

Yes, this paragraph has been substantially rewritten.

Figure 6: One could include the Portland sea-level data point from Talke & Ray 2019. If you include the Battery, then a longer data set is possible. Not sure however if this graph is needed or is critical for the story. It is not really a result of this study, just a replotting of other results. There is no clear analysis of how tides might be influenced by SLR it's basically just a literature review.

We now fit a model on MSL (see model 1 in our statistical analysis), so the figure 6 showing annual MSL becomes more important in the paper. For this reason, we kept it. However, we plotted MSL corrected from land movement (because the MSL model in "model 1 = α MSL" is corrected for land movement), instead of relative MSL.

Line 247-250. Wouldn't storminess also impact tidal constituents, at least on the shelf or in a harbor? I think there are some references on that. I came across a Pugh reference at some point for the Irish Sea, if memory serves. The Graewe et al. 2014 reference also discuss this for the North Sea, I think. In any case, wind stress and wave breaking and these sort of things represent an input of turbulent kinetic energy and could in theory affect tides at some stations, if there are climate-based shifts in storminess. In the context of this paper, Talke et al. 2014 showed that the probability of large storm tides in New York goes up when the NAO is negative. There are also known NAO effects in Europe (see the Woodworth et al. (2007?) paper). Does this matter for tides? Might be something to at least investigate.

In the North Sea, the wind stress and the wave breaking affect firstly the surges (Pineau-Guillou et al. 2020, Ocean Modelling). In coastal areas, the wind stress contribution is more effective due to shallow waters, water pileup along the coast, as well as resonant effects (Moon et al., 2009; Bertin et al., 2012). In addition, in nearshore areas, the radiation stress, i.e. the momentum flux carried by the waves, generate nearshore currents and wave setup (i.e. additional surge) when the waves dissipate (Brown et al., 2010; Bertin et al., 2015; Choi et al., 2018). Concerning a possible connection between storm surges and NAO, Menéndez & Woodworth (2010) found a significant correlation between NAO and extreme high waters in the North Sea. Marcos & Woodworth (2017) found a correlation between NAO and skew surges.

Here, we did not study the impact of storminess on tidal constituents. However, we agree with the reviewer that it would be interesting to investigate. We have added in the limitations and perspectives, that dedicated studies are necessary to estimate if changes in storminess could affect significantly tidal constituents.

By the way, it's not clear to me that a measured decrease in M2 during periods of stormy weather is a real change in M2. Another (perhaps not mutually exclusive) explanation is that depth changes during storms alter the phase speed of the tides, such that they arrive a bit earlier than usual (See for example Horburgh and Wilson 2007). A period with a lot of ups and downs in mean sea-level is going to cause lots of phase speed variations, more spectral spread (cusping), but decreased amplitude. Just as timing errors can cause a decrease in measured M2 (see Zaron & Jay 2014), so would changes in phase speed.

We agree with the reviewer that in period of stormy weather, the depth changes (because of the surge), and the tide will arrive a bit earlier than usual. As mentioned previously, we did not go further on the impact of storminess, but we have mentioned that dedicated studies would be valuable.

Line 254 “possible role of stratification” for what? Would clarify, e.g., something like “possible role of stratification on secular tidal trends”
Yes, this has been added.

Line 255 “between these processes” what processes? Maybe be specific.
By “processes”, we meant atmospheric circulation and ocean stratification. The sentence has been rephrased.

Line 266 How can the NAO decrease globally? It is specific to the North Atlantic.
By “globally”, we meant “overall” (and not worldwide). We have corrected the sentence.

Line 278 “Pushed southern” should be “pushed southerly”.
Yes, we have corrected the sentence.

Line 289 Might be good to discuss the role of wind earlier. See notes above.
Yes, we discussed earlier the role of wind, with reference to Devlin et al. (2018). Devlin et al. (2018) shows that the impact of atmospheric circulation (via the wind stress, through Ekman current) on M2 seasonal cycle could be significant and comparable to the effect of permanent (geostrophic) currents.

Line 291-292 Maybe, but there is quite a bit of variance in all the plots and it seems like a couple curves looking similar could easily happen from random chance. Unless you can figure out the statistical robustness of these results, would perhaps avoid ascribing M2 behavior at a few locations to NAO.
Yes, this sentence has been removed. The statistical analysis (see above) shows now anti-correlation between NAO and M2 variations in Halifax.

Line 332 The Devlin et al. papers discuss correlations between sea-level anomalies and tidal anomalies, and possible reasons for them. In a way, you are trying to do something similar, but over a larger time scale. However, there is little statistical correlation or significance testing done here. Would suggest this be done.
For the seasonal variation of M2, we could have added Devlin et al. 2018 as a reference (even if it focuses of Southeast Asian Waters). However, this sentence relative to the seasonal variation of M2 has been removed (lines 330-332), as we now consider years with at least 75% (following a suggestion from the Editor).

Note that the Devlin et al. 2018 paper is interesting in the context of our study. It shows the impact of the wind stress (via Ekman current) on M2, at a seasonal scale. As mentioned by the reviewer, we are investigating a similar mechanism (effect of the atmospheric circulation) but at a larger time scale (decadal instead of seasonal).

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2020-56/os-2020-56-RC2-supplement.pdf
Note that we did not notice any differences between the main file and the supplement.