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Interactive comment on "Climate-scale changes of the semidiurnal tide over the North Atlantic coasts from 1846 to 2018" *by* Lucia Pineau-Guillou et al.

Anonymous Referee #1

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In this paper, nine very long (>80-year) tide gauge records along North Atlantic coasts are analyzed for secular changes in the M2 amplitude. The series are compared both among each other and with climate mode indices in an attempt to relate the observed amplitude changes to large-scale forcing mechanisms. Unfortunately, the paper is of limited scope and the methods are not innovative. The arguments put forth to link the observed M2 changes to the North Atlantic Oscillations (NAO) are fallacious (see below) and invalidate exactly that part of the paper that is thought to break fresh ground compared to similar analyses in the past (e.g., Mueller 2011, GRL). These shortcomings are not easily redressed in a revision, and I therefore recommend the manuscript to be rejected. Overall, the study offers too little new insight. It merely highlights similarities without investigating the underlying processes.

Major issues:

[-] The authors' explanation of the pronounced M2 increase over 1960–1990 at the three European stations ($\sim$3 cm at Brest and Newlyn, $\sim$10 cm at Cuxhaven) in terms of typical NAO sea-level pressure patterns is flawed. The response of sea level (or water column thickness) to changing pressure loading on time scales longer than a few weeks is static isostatic (IB), creating a sea-level difference of about 6–10 cm at the location of the tide gauges, according to the authors' plots (Figures 8 and 9). These changes in water depth are simply too small to cause the observed M2 amplitude trends. Typically, sea-level changes alter the propagation characteristics of tides in shallow water such that one can expect perturbations in the M2 amplitude of 1–5% relative to the imposed water depth change, see the modeling results by Schindelegger et al. (2018). Cuxhaven may be an exception of that rule, although a 6-cm increase in sea level from atmospheric pressure will engender less than 2 cm changes in the M2 amplitude (Figure 8 of Schindelegger et al.).

The authors circumvent the problem by assuming higher sea-level changes in areas distant to the gauges (-20 cm near Island) and picking a 10% sensitivity of M2 amplitudes to water depths from literature – an inordinate value that only holds in very shallow settings (e.g., estuaries) and not across entire shelf regions.

Moreover, Figure 8 displays higher atmospheric pressures around the European mainland in 1989, implying that sea level actually dropped relative to 1969, opposite to what is shown in Figure 9. So that's an inconsistency on its own, but more alarmingly for the authors' theory, numerical modeling (see references) strongly suggests that an increase of M2 in the German Bight, as observed at Cuxhaven, actually requires local sea level to rise, not to fall.

[-] A key argument is that the low-frequency winter NAO index (Figure 7) is similar to the evolution of the M2 amplitude at the three European stations. Such an important point in the paper should be substantiated by an appropriate plot (in which annual M2

changes would be filtered using the same 9-year running median as the NAO index). More importantly, the mentioned similarity is never established in a quantitative sense, e.g., by tabulating correlation measures and their statistical significance considering effective degrees of freedom. In fact, the Brest time series in Figure 3a seems to have rather little in common with the NAO time series as it has a dip in the 1980s (when NAO steadily increases) and features an all-time high in the late 19th century (when NAO just erratically switches sign). For Newlyn and Cuxhaven, I expect the correlations to be higher, although the timing of individual peaks might be different. Such phase lags and leads are not easily explained in terms of physics; certainly not within the framework proposed here, because both sea level and subsequently tides would adjust instantaneously to NAO-related atmospheric pressure loading.

[-] The data basis on the European Shelf (three stations) is very shaky. It would be desirable to make the analysis more robust by adding results from tide gauges that are somewhat shorter but still provide good coverage of the period with distinct variability in NAO (1960 onwards).

[-] The Introduction leaves a lot to be desired. It is incoherent, lacks any quantification as to the size of observed tidal changes and does not tell the reader why he/she should bother. A very good example of clarifying the relevance of this subject matter up front is Mawdsley et al. (2015, https://doi.org/10.1002/2014EF000282).

Minor comments (most of these issues are indications of the authors' unsteadiness regarding the physics of tides):

[-] The Introduction's first sentence is wrong. Tides have been changing also prior to 19th century, e.g., due to Earth's continental and glaciation cycle.

[-] The main tidal constituents in the North Sea are presently not in a state of resonance (as argued on lines 172 and 219). They are rather described as Kelvin waves, dampened as they propagate from the Northwest through the basin in cyclonic fashion.

[-] Lines 250–252: First, stratification will not only change in response to heat fluxes, but also due to the advection of water masses, evaporation, salt dilution, etc. Second, in a discussion of stratification effects on "tides", one must use very precise language, in particular distinguish between barotropic, baroclinic, and surface (barotropic + baroclinic) tides. Third, the process identified by Kang et al. (2002) as cause for tidal seasonality in the Yellow/East China Sea is mixing strength (changes of vertical eddy viscosity) and not barotropic-to-baroclinic energy conversion.

[-] Lines 231–234: The simulations of Pickering et al. (2017) show exactly the opposite of what is described here (that is, their Figure 1a highlights an M2 increase in the German Bight, not a decrease).

[-] I understand the pragmatic approach of normalizing M2 changes to show results from different stations in one plot, but it would still make sense to include some absolute numbers (e.g., by using text or secondary Y axes) to facilitate quantitative comparisons among stations and allow for a meaningful interpretation of results derived later on.

[-] Annual tidal harmonics are computed from data spanning full years, but the discussion of atmospheric pressure changes only focuses on snapshots from winter months – another inconsistency in the analysis. Surely, if annual averages of atmospheric pressure fields are considered, the magnitude of the static sea-level response would decrease even further.

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