Comments on “Western boundary circulation and coastal sea-level variability in northern hemisphere oceans” by Diabate et al, submitted to Ocean Science Discussions (https://doi.org/10.5194/os-2021-24)

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General comments:

The paper compares coastal sea level variability near two WBCs, the Kuroshio and the Gulf Stream (GS), and finds interesting links between sea level and shifts in the position of the currents after they separated from the coast. EOF analysis highlights some similarities and some differences between the two regions and emphasizes the different sea level response north and south of the separation point. The topic of remote influences on coastal sea level and links to open ocean currents is an important new area of research in recent years, and this study is certainly a nice additional contribution, even though some of the results may not be completely new. The paper is well written and the results worth publication, though it could be improved by having more clear indication what is new or novel here relative to other recent studies of WBCs (some not cited, see below).

Major comments:

1. One notable result, the dramatic shift in the North Atlantic region after the 1990s (Figs. 4 and 7) is not completely explained, in my opinion. Several recent papers on the topic that were not cited can shed more light on the topic and help the authors in their explanation of the dynamics involved. For example, Chen et al. (2019) and Ezer and Dangendorf (2021) already compared WBCs dynamics including the Kuroshio and the Gulf Stream, both discuss the link between uneven warming, spatial sea level rise differences and the intensity of WBCs (little is said in the current paper about the role of temperature change). While they did not focus on meridional shifts of WBCs, the position and intensity of WBCs are closely related, and this point is somewhat lost. On decadal time scales recent studies show that the Kuroshio is correlated with the wind much more significantly than the GS does (see Fig. 11 in Ezer & Dangendorf, 2021), which may partly explain why abrupt changes in the GS due to internal dynamics may be more likely. On decadal time scales there is also a shift in the GS transport from strengthening during 1970-1990 to weakening in 1990-2015 (see Fig. 9 in Ezer and Dangendorf, 2020) - was this shift in transport related to the change you found around 1990?

Another topic that was not fully explored is the disconnect north and south of the separation point (Fig. 3), which was investigated by others - a recent paper (Ezer, 2019, not cited) fucus on this very point, suggesting an explanation related to the proximity of the GS to the coast, showing how propagation of positive temperature anomalies can cause coastal sea level rise/fall for locations south/north of the separation point, as the GS intensified and shifts after separation, but stays near the coast with warmer waters in the south. I hope all these new studies will help the authors putting their results in context with other recent findings.

Chen C, Wang G, Xie S, Liu W (2019) Why does global warming weaken the Gulf Stream but intensify the Kuroshio? J. Clim. 32:7437–7451. https://doi.org/10.1175/JCLI-D-18-0895.1

Ezer, T., (2019), Regional differences in sea level rise between the Mid-Atlantic Bight and the South Atlantic Bight: Is the Gulf Stream to blame?, Earth's Future, 7(7), 771-783, doi:10.1029/2019EF001174.

Ezer, T. and S. Dangendorf (2020), Global sea level reconstruction for 1900-2015 reveals regional variability in ocean dynamics and an unprecedented long weakening in the Gulf Stream flow since the 1990s, Ocean Science, 16(4), 997-1016, doi:10.5194/os-16-997-2020.

Ezer, T. and S. Dangendorf (2021), Variability and upward trend in the kinetic energy of Western Boundary Currents over the last century: impacts from barystatic and dynamic sea level change, Climate Dynamics, doi:10.1007/s00382-021-05808-7.

Specific comments and suggestions:

2. Abstract, line 6: "… comparison between the two basins is missing", the statement is not completely true given the recent studies mentioned above that compared the two WBCs (e.g., Chen et al., 2019; Ezer and Dangendorf, 2021; others). This fact could be added to the introduction.

3. Lines 59-64: in addition to the cited Andres et al. (2013), please take a look at a more recent paper from the same group (Andres et al., 2020), which studied the path of the GS at two sections. They showed large differences between the western GS affected by local recirculation and the eastern GS where variations in the meridional path are much larger. The large spatial differences over short distances along the GS may explain some of the discrepancies you cited.

Andres, M., Donohue, K. A., and Toole, J. M. (2020), The Gulf Stream's path and time-averaged velocity structure and transport at 68.5W and 70.3W, Deep-Sea Res. Pt. I, 156, 103179, https://doi.org/10.1016/j.dsr.2019.103179, 2020.

4. Lines 85-89: The increase in kinetic energy of WBCs (Ezer and Dangendorf, 2021) and differences between the Kuroshio and GS with respect to AMOC (Chen et al., 2019) may be relevant to add here.

5. Lines 276-277: in addition to McCarthy et al. (2015) and Woodworth et al. (2014), the more recent study of Ezer (2019) focus specifically on the drop in correlation north and south of Cape Hatteras (in fact, Fig. 2 in Ezer's paper using altimeter data is the equivalent to and confirmation of Fig. 3 here using tide gauge data).

6. Lines 292-295: is it possible that the drop in correlation in the Atlantic may relate to weakening GS at that period (Ezer and Dangendorf, 2020) or/and to the shift in the hotspot of sea level rise (Valle-Levinson et al., 2017; Ezer, 2019)? Dynamically, it makes sense that a stronger current in early years has more coherence along its path than a weaker one that can be affected more easily by local factors. Have you considered this option?

7. Line 355: there is no "Figure 9", should be 6?

8. Line 469: "… re-qualified the Gulf Stream presence as a plausible sea-level driver…", it may be clarified that this result is not new and confirms many previous studies (e.g., Ezer et al., 2013, Ezer, 2013, 2015, 2019).

9. Lines 532-541: the "puzzling" result of the shift around 1990 is still not clearly explained, with several hypotheses offered. It may also be useful to see if this shift links to recent weakening of the GS and the southward shift in the hotspot of SLR from north of Cape Hatteras to south of Cape Hatteras; see comments above and Valle-Levinson et al. (2017) and Ezer (2019).

10. Line 641: "… further understanding of the forcing on sea level prior to ~1990 is needed…", it may be useful to add here that studying the link between open ocean dynamics and variations in coastal sea level in early years is possible and has been recently done using reconstructed sea level

approaches going back to 1900; see for example studies such as Ezer and Dangendorf (2020) and Dangendorf et al. (2021).

Dangendorf, S., T. Frederikse, L. Chafik, J. Klinck, T. Ezer, and B. Hamlington, (2021), Data-driven reconstruction reveals large-scale ocean circulation control on coastal sea level, Nature Climate Change, doi:10.1038/s41558-021-01046-1