Manuscript “**Remote forcing of the Florida Current on seasonal time scales**”

Journal**: Journal of Geophysical Research – Oceans**

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**Acknowledgment**

Dear Editor/Reviewers.

We have carefully addressed all comments and suggestions provided by the reviewers, and we believe that our manuscript is now in a better format that may be suitable for publication.

Thank you very much for your time.

**Response to reviewer’s comments**

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| Text Code | |
| Reviewer’s comments | Blue |
| Reply to Reviewer’s comments | Black |
| Changes in the manuscript | *Italic* |

**Note:** the initial summary provided by each reviewer is omitted here.

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Reviewer #1 Evaluations:   
  
1) Line 149-150: Relating to "the westward amplification of the spectral power is consistent with classic wind-driven theory", I cannot understand what process the authors mean by classical wind-driven theory. Explain the process in detail. I wonder why baroclinic signals within 73-525 day period do not reduce during their propagations across the North Atlantic, because such signals may suffer influences of Ekman pumping with the time scale of 73-525 day period, which changes the direction of its velocity several times during 4 to 7 years.

We thank the reviewer for raising these comments. Regarding the first statement specified by the reviewer (originally on lines 149-150), the language was misleading, and the text was adapted to the following:

(lines XXX) “*The westward amplification of the spectral power is consistent with westward intensification predicted from classic wind-driven theory, and is observed at all latitudes.*”

We have also included the following statement to clarify the meaning of “classical wind-driven theory”:

(lines XXX) “*In other words, the increase in the spectral energy of westward propagating signals towards the western boundary is consistent with the basin-wide integrated input of wind-driven energy into the ocean expected from classical Sverdrup Theory.*”

Regarding the conservation of the properties of westward propagating signals while they travel across the North Atlantic, this is in part because dissipation in the ocean interior is usually very small – vorticity production generally dominates in the ocean interior, rather than vorticity destruction. We have included the following statement for clarifying purposes:

(lines XXX) “*These results suggest that these signals conserve their spectral (wavelength and period) characteristics while crossing the North Atlantic Ocean, which may partly because vorticity dissipation rates are likely small in the ocean interior.*”

Regarding the comment about Ekman pumping forcing in the interior, the reviewer is referred to the answer to the following question number 2, which addresses this effect in more detail.   
2) Line 155-157: Relating to "the striking persistence of significant seasonal periodicity implies the presence of transient seasonal variability". I would like to know the generation mechanism of westward propagating baroclinic signals within 73-525 day period in the eastern North Atlantic, and the reason why such signals propagate during 4 to 7 years without their reductions.

We appreciate the questions raised by the reviewer. Specifically about the generation mechanisms for westward propagating signals, we have included the following language in the text for clarification purposes. Regarding the westward propagation of these signals without reduction of intensity, the reviewer is referred to the answer to question 1, which has specifically addressed this topic.

(lines XXX) “*Westward propagating signals discussed in this study are largely linked with wind-driven mechanisms. Previous studies have shown that these signals may be generated by local wind forcing in the ocean interior due to Ekman Pumping [Krauss and Wuebber, 1982], and also by wind stress variations on the eastern boundary that can lead into changes in the depth of the thermocline [Anderson and Gill, 1975; Krauss and Wuebber, 1982]. The detailed analysis reported by Watanabe et al., (2016) based on the comparison of a minimalistic wind forced Rossby wave model with satellite winds and altimetry observations showed that the main forcing mechanism is due to wind stress variations on the eastern boundary of the basin. This is likely the main reason why the variability of signals observed here can be traced all the way to the eastern boundary.*”

3) Line 224-226: Relating to "seasonal SHA signals formed in the eastern North Atlantic may conserve their spectral characteristics while traveling across the basin", I repeat the same questions.

The reviewer is referred to the answer of questions 1 and 2.

Minor comments:   
Line 32: Replace "Rousset and Beal, 2011" after "DiNezio et al., 2009". Done. Line XXX  
  
Line 51: I misunderstood FCt as FC transport in my first reading. It may be better to clearly express the whole word as follows: "the transient seasonal variability of the FC transport" (FCt) is defined ...

We thank the reviewer for this helpful suggestion on the text. The definition of the acronym has been changed accordingly. Please refer to line XXX  
  
Line 67: The word "is" may be changed to the word "are".

We appreciate the suggestion by the reviewer. It is our understanding, however, that, for this specific case, the verb “is” is in agreement with the subject of the sentence “term”.   
  
Line 96, Figure 1b and its caption: In the caption, write that the 95% confidence level is indicated by the white line. Because the line thickness is too thick, the white line does not look to be a line in the high-frequency band with periods less than 73 days. The meaning of bold black lines in Figure 1b should be also explained in the caption.

We would to thank reviewer for noticing the missing information in our figure caption, and for the suggestion on the figure. We have adapted the line thickness in the figure included, and included the following language in the figure caption.

(line XXX) *“The thick white contours represent the peak-based significance levels, computed at 95%. The thick black curve indicates the cone of influence.”*

Line 206-209 and Figure 5: I cannot clearly trace SHAt signals to the interior region in time sequences (e.g., Figures 5a, b and c). It is helpful to mark the westward propagating signal in the figures.

We appreciate the suggestion by the reviewer here. We have adapted Figure 5 accordingly.

(lines XXX) “*Specific positive (negative) westward propagating signals are emphasized by the green (yellow) shaded contours.*”

Line 220: Change "Anderson and Corry [1985a]" to "Anderson and Corry [1985]". Done, line XXXx  
  
Line 256: "Where" should be "where". Changed accordingly. Line XXX.  
  
Line 285: The word "complimentary" is typo, and may be "complementary". Text changed accordingly. Line XXX.   
  
Line 293: It may be helpful to change "Figure 6c" to "a red bar shown in Figure 6c".

We thank the reviewer for the suggestion. We included the following statement:

(line XXX) “*(red bar shown in Figure 6c)*”  
  
Figure 6b: I cannot understand why the number of significant PCs (gray bar) is zero in the region 25-27N, because the explained FC variance (thick black line) is not zero.

We thank the reviewer for noticing this issue with our plot of the number significant Principal Components (PC) used by our analysis. In fact, the values presented on Figure 6c between 25ºN - 27ºN were not exactly zero, they were missing from the plot. This issue was caused because an earlier version of this figure was truncated at 27ºN, and when the southern limit was extended to 25ºN, the missing values between 25ºN - 27ºN were not included.

This issue has been corrected in the new Figure 6c. The updated Figure 6b shows that the number of significant PCs used by our approach between 25ºN-27ºN is essentially equal to 1, given that coastal sea height anomalies time-series in the Florida Straits are highly correlated.

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Reviewer #2 Evaluations:   
  
Reviewer #2 (Formal Review for Authors (shown to authors)):   
  
Major comments:   
1. The manuscript gives the impression that the WPS is a "forcing of" or a "link to" the seasonal cycle. However, the seasonal cycle and the WPS may be two separate entities, so more accurate description would be that WPS modulate the seasonal signal (as it does to other variations, from weekly to decadal time scales). If the main driver of the seasonal cycle is seasonal wind pattern, than the two are independent (but they may be coupled on interannual, decadal and multi-decadal scales through variations in the NAO and AMO; NAO is surprisingly missing from the discussion despite many papers linked it to FC).

We understand agree with the reviewer that a more accurate description of the observed mechanism is that westward propagating signals can modulate the seasonal FC variability. We have replaced all occurrences of “forces” or “drives” for “modulates” in the text accordingly.

With respect to the NAO, we appreciate the comments and suggestions provided by the reviewer. The reviewer is referred here to the answers of comments number 8 and 14.

The focus on "seasonal time scales" in the title and elsewhere is a little misleading, since the described remote signals propagate within 4-7 years, so it is in fact part of interannual to decadal signals in the N. Atlantic.

We understand the concern of the reviewer here. We have adopted the following term in the title and throughout the manuscript “year-to-year changes in the seasonality of the Florida Current”. We believe that this provides a more accurate description for the timescales that is the focus of this study. We have changed the title to:

*Title: Remote sources for year-to-year changes in the seasonality of the Florida Current transport*

There is also no explanation for the chosen band of 73-525 day periods, which seems somewhat arbitrary (why not 2-mon to 1-year band?). Please clarify the discussion with respect to the above suggestions.

The reviewer is referred to the answer of comment number 5.

2. The manuscript could be greatly enhanced by putting the results in the right perspective with respect to several other important studies that were not cited. In particular, the link between the FC/Gulf Stream and coastal sea level has been suggested in several old papers (Montgomery, 1938; Blaha, 1984) and more recent papers (Ezer, 2016; Ezer et al., 2013- only the latter paper is cited, but in a different context).

We thank the suggestion provided by the reviewer. With the exception of Montgomery (1938), all papers suggested have been cited accordingly, and the reviewer is referred to the answer of the comments below for more details. Unfortunately, we were unable to obtain a copy of the publication by Montgomery (1938), which is the reason why this study is not cited here.

The remote forcing mechanism of the Gulf Stream and sea level by WPS is not new and has been shown before by the simple model of Sturges and Hong (2001) and by the 3D Atlantic model of Ezer (1999); the latter paper discussed WPS that can be detected ~8 years prior to their arrival at the US coast when they affect the Gulf Stream transport and sea level, in a similar manner as described here (the current study with altimeter data thus seems to confirm the earlier model results, which is important to point). Also, the mechanism of linking remote forcing to coherent coastal sea level signals through the generation of coastal trapped waves has been discussed in details by Huthnance (2004) and Ezer (2016). I suggest that the author take a look at the papers referenced below and cite them appropriately.

We once again thank the reviewer for the valuable suggestion provided. These references have been cited accordingly. The reviewer is referred to the answers of comments 6, 9 and 12 for more details.

References

Blaha J. P., (1984), Fluctuations of monthly sea level as related to the intensity of the Gulf Stream from Key West to Norfolk. J. Geophys. Res.- Oceans, 89(C5), 8033-8042.

Ezer, T., (2016), Can the Gulf Stream induce coherent short-term fluctuations in sea level along the U.S. East Coast?: A modeling study, Ocean Dynamics, 66(2), 207-220, doi:10.1007/s10236-016-0928-0.

Ezer, T., (1999), Decadal variabilities of the upper layers of the subtropical North Atlantic: an ocean model study. J. Phys. Oceanogr., 29(12), 3111-3124. doi:10.1175/1520-0485(1999)029

Huthnance, J. M., (2004), Ocean-to-shelf signal transmission: a parameter study. J. Geophys. Res., 109(C12029). doi:10.1029/2004JC002358.

Montgomery, R., (1938), Fluctuations in monthly sea level on eastern U.S. Coast as related to dynamics of western North Atlantic Ocean. J. Mar. Res., 1, 165-185.

Sturges, W. B., Hong, G., (2001), Gulf Stream transport variability at periods of decades. J. Phys. Oceanogr. 31, 1304-1312.

Minor suggestions and comments:

3. Title- please consider whether or not "seasonal" should be used (see comments above), as the study is actually related also to interannual and longer variations.

The title has been changed to:

*Title: Remote sources for year-to-year changes in the seasonality of the Florida Current transport*

4. Lines 34-35: "... baroclinic signals coming from the ocean interior...", may be add citation to Sturges and Hong (2001) and Ezer (1999) here?

We thank the reviewer for the suggestion. The references were cited accordingly. Please refer to line XXX in the new draft.

5. Line 53: please explain the reasoning behind 73-525 day period band (also it is "period", not "frequency").

We thank the reviewer for the comment. The following explanatory statements have been included in the manuscript.

(line XXX) “*In this study, focus is given to the band at periods of 73-525, which is defined following the inspection of the wavelet transform diagram of the FC transport (Figure 1b): the lower and upper limits of 0.2 year (73 days) and 1.44 year (~525 days), respectively, are defined to ensure that the dominant annual and semi-annual signals are included in the transient FC component, which is used here to assess year-to-year changes seasonality of the transport linked with westward propagating signals.*”

6. Line 72-76: in addition to the importance of the FC to MOC and MHT, one should also mention here its importance in relation to coastal sea level variability (Blaha, 1984; Ezer, 2016) and coastal sea level rise (Ezer, 2013; Ezer et al., 2013).

We agree with the reviewer, and appreciate the suggestion provided. Additional language has been included in the manuscript to address his recommendation.

(lines XXX) “*In addition, changes in the strength of the FC are largely associated with coastal sea-level variability [Blaha, 1984; Ezer, 2016] and sea-level rise along the east coast of U.S. [Ezer, 2013; Ezer et al., 2013], which is of ultimate importance for the resilience of coastal communities and ecosystems. Therefore, because changes in the FC transport can be linked to physical processes that may lead to societal impacts, efforts aiming to improve the understanding of mechanisms driving such changes in the FC are important.*”

7. Line 92 (and elsewhere): no need to repeat the definition of SV more than once.

We thank the reviewer for the suggestion on the text. The additional definitions were removed accordingly, and only one was maintained in the introduction. Please refer to line XXX in the new draft.

8. Line 94 and Fig. 1b: figure has no units or colorbar scale. Can the low frequency signal be explained by the NAO? (low NAO in the 1970s and 2010 and high in the 1990s). The figure show quite chaotic pattern, except may be the coherent hi-frequency signal (which was addressed by Ezer, 2016).

The color scale has been included in Figure 1b accordingly. The reviewer raises an interesting point that the low frequency signal may be possibly linked with the NAO. However, investigating such link is beyond the scope of this study, once this study focuses on changes in the FC transport modulated by westward propagating signals that occur at periods of 73-525 days.

We have included the following statement in the manuscript for further clarification.

(lines XXX) “*Changes in the low-frequency spectral characteristic of the FC transport may be linked with the reported interannual adjustments in the wind stress curl related with the North Atlantic Oscillation (NAO) [Baringer 2001; Atkinson et al., 2010].*”

9. Lines 162-163: "Observations indicate that coherent SHA along the coast... (not shown)..."- Fig. 1 in Ezer (2016) shows an example of this coherent sea level. It may be useful here to also add the references to the coastal trapped waves mechanism (Huthance, 2004) that explain the coherent features.

We thank the suggestion provided by the reviewer. We added the following language in the manuscript in accordance with the reviewers recommendation.

(lines XXX) “*Observations indicate that coherent SHAt are observed along the east coast of the U.S. between 26.5°N—42°N (not shown here); for example SHAt along the coast are correlated with the sea level at 27°N with lags of less than 1 week. These observations are generally consistent with previous studies [Mooers et al, 2005; Ezer, 2013; Ezer et al., 2013; Ezer, 2016], and imply that changes in the coastal sea-level at 27°N are associated with the variability associated with westward propagating signals that is rapidly transmitted along the coast. In a study using idealized model simulations, Huthance [2004] showed that large-scale ocean motions could be indeed transmitted to the shelf through the generation of coastally trapped waves, which in the North Atlantic Ocean, imply in southward phase propagation along the east coast of U.S.*”

10. Lines 217-218: "... seasonal variability of the FC transport may be partly forced...", may be use "modulated" instead of "forced"?

We agree with the reviewer that the term “modulated” is better suited to our results than the term “forced”. All instances of “forced” and of “driven” have been replaced by “modulated” in the manuscript, when they refer to results from this study.

11. Lines 227-230: One of the most important results shown in Fig. 4a is the coherent correlation along the Gulf Stream path downstream of the FC- it is not completely explained here given that Rossby waves at different latitudes propagate at different speeds and arrive at the GS edge at different times. On the other hand, Ezer (2016) shows that imposed variations in the FC transport itself can produce coherent transport variations along the entire GS path even without any Rossby waves. Please clarify.

The reviewer raises a valuable point here. Results of Ezer (2016) indicate that changes in the Florida Current transport can lead to coherent transport variations along the entire Gulf Stream path, which may feedback to the Florida Current into the Florida Straits through the generation of coastally trapped waves.

Westward propagating signals may have an effect similar to the oscillations in the FC transport imposed as boundary conditions in the study by Ezer (2016). Our analyses indicate that, as these signals reach the western boundary, they can interact with the background GS circulation, and modulate the generation of coastally trapped waves, which will in turn affect the FC transport.

We appreciate the comment provided by the reviewer, and have included the following language in the manuscript:

(lines XXX)“ *Along the U.S. coast, coherent changes in SHA were negatively correlated with the FC transport. These results suggest that coastally trapped waves may provide the main link between the open ocean variability associated with westward propagating signals and the modulation of the FC transport. For example, results from Ezer (2016) based on idealized numerical simulations showed that imposed changes in the FC flow could lead to coherent variations in the Gulf Stream transport along its the entire path, which often caused the generation of coastally trapped signals that could feedback into the FC variability. The interaction between westward propagating signals and the Gulf Stream may drive a similar effect, leading to the generation coastally trapped waves that can modulate the FC seasonal variability.*”

12. Line 273-274: the influence of Cape Hatteras topography on the pattern of acceleration in sea level rise has been indicated in Ezer (2013) and the blocking effect of CH on the southward propagation of baroclinic coastal waves has been indicated in Ezer (2016).

We have included additional language in the manuscript according to the suggestion provided by the reviewer.

(lines XXX) “*For example, the bottom topography c-an play an important role on accelerating sea-level rise around Cape Hatteras [Ezer, 2013], and also on blocking the southward propagation of baroclinic coastal waves [Ezer, 2016].*”

13. Lines 292-297: are these estimates consistent with simple geostrophic calculations?

Yes, observed coastal sea-level changes are consistent with an geostrophic intensification of FC the transport above the thermocline, indicating baroclinic adjustment of the circulation in the Florida Straits. We have included the following language and analysis in the manuscript to support this conclusion.

(lines XXX) “*Coastal sea-level changes and rise along the U.S. coast have been previously associated with adjustments in the geostrophic dynamics of the FC and Gulf Stream [Ezer, 2013; Ezer et. al., 2013]. To further verify that the observed sea-level changes reported here were also linked with adjustments in the geostrophic dynamics of the FC, values reported here are compared with values expected from simple geostrophic calculations. For example, taking into account that the Coriolis parameter has a value of 6.6 X 10 5 s-1 at 27ºN, that the area of the Florida Straits is ~4.5 X 107 m2, and assuming a barotropic adjustment in the water column, a 5 cm change in coastal sea-level across the Florida Straits would result in a ~3 Sv change in the FC transport, which is three times larger than values observed here (~5cm / 1Sv, Figure 6c). Assuming a baroclinic adjustment instead, where the Florida Straits area is replaced by the area of the water column above the thermocline (~1.8 X 107 m2), the calculation results in a transport of 1.2 Sv, which is very close from values observed here. Therefore, these results indicate that coastal sea-level changes reported in this study for the U.S. coast may be largely linked with fluctuations in the FC transport above the thermocline, suggesting baroclinic adjustments in the geostrophic circulation.*”

14. Lines 308-321 and Fig. 7: can the disagreement between the observed and derived FC transport in recent years (since 2010) be related to the extremely low NAO and the unusual (~30%) weakening in AMOC during 2009-2010? (see Srokosz and Bryden, 2015; Ezer, 2015)- if this is the case, remote WPS may have less impact in those years than AMOC does.

The reviewer raises an excellent point that changes in the wind forcing linked with NAO may explain some of the discrepancies between the observed and the synthetic FC time-series associated with westward propagating signals. We believe that year-to-year changes in the wind forcing linked with the NAO may indeed account for most of the these discrepancies. We appreciate the question. Additional language has been included in the manuscript accordingly.

(lines XXX) “*For example, extreme values of the NAO observed during 2009-2010 were linked with changes in the wind field and attributed as the one of the main drivers for the extreme low value of the North Atlantic MOC observed during this period at 26.5ºN [McCarthy et al., 2012; Ezer, 2015; Srokosz and Bryden, 2015]. Therefore, it is likely that similar interannual NAO-related changes in the wind field may lead to adjustments in the transient seasonal component of FC transport that can overlap or even outweigh the effect of westward propagating signals at times.*”

Srokosz, M. A., and H. L. Bryden (2015), Observing the Atlantic Meridional Overturning Circulation yields a decade of inevitable surprises. Science, 348(6241). doi:10.1126/science.1255575.

Ezer, T. (2015), Detecting changes in the transport of the Gulf Stream and the Atlantic overturning circulation from coastal sea level data: The extreme decline in 2009-2010 and estimated variations for 1935-2012, Global and Planetary Change, 129, 23-36, doi:10.1016/j.gloplacha.2015.03.002.