os-2015-13   Submitted 24 Mar 2015
Accelerated sea level rise and Florida Current transport
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Responses to Referee #1

Thank you for your review and criticisms of the manuscript, we feel that they significantly improve the content and presentation of the material. Please find below the original comments followed individually by responses, and please note that all responses are indented.

Prior to addressing the comments we wish to thank you for suggesting Higginson et al. (GRL 42 (5), 2015). Although it is dealing with surface gradients generally orthogonal to our analysis, it is within the same arena, and it is interesting that they seem to accept the analysis of Rossby (et al. GRL, 2014) who finds no significant weakening of the Gulf Stream. In the original manuscript we noted the apparent disagreement between Ezer et al. (JGR Oceans, 118, 2013) and Rossby (et al. GRL, 2014) regarding a recent weakening. It be may of interest to note that Ezer (Global and Planetary Change 129, 2015) has recently suggested that the differences can be attributed to linear (Rossby et al.) *versus* nonlinear trend (Ezer et al 2013) analysis, which we also pointed out in our manuscript. This is relevant to our work since it seems to resolve the discrepancy and supports our assertion that linear regressions (as historically used to ascertain Florida Current variability) may not be as suitable as a data-adaptive time-varying approach to estimate trends of highly variable geophysical signals.

Another interesting feature of Higginson et al. (2015) is their EOF #1 of mean dynamic topography (MDT) shown in their figure 2b (also fig. 1B & c) which suggests that the MDT variance between our areas of interest (Vaca Key to 27N) is quite small. Could this be interpreted as support for one of the major, unverified assumptions in our analysis? "... where we have assumed that the transport–SSH relationship deduced at 27N applies at the tide gauges". Since the MDT variance is small here, it seems plausible as a supportive observation.

Errata: Please note an error in the statistics of linear regression of transport onto time in the third paragraph of section 2. The standard errors were mis-reported. The changes are: 0.77 ±0.22 Sv to 0.77 ±0.55 Sv; 1.08 ±0.46 Sv to 1.08 ±0.11 Sv; and 0.57 ±0.38 Sv to 0.57 ±0.19 Sv. This does not change any of the arguments or conclusions of the analysis.

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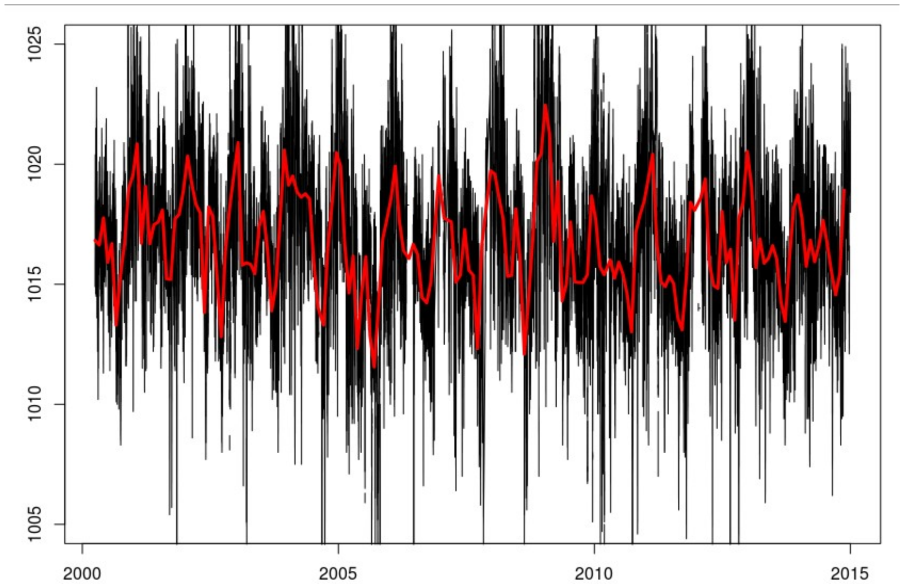
page 552, lines 1-5 and equation (1) on page 559:
A first comment is that the relationship will not just be geostrophic. There is a discussion of the possible dynamical terms in an area of strong current, bottom friction, and wind stress on a narrow shelf in Higginson et al. (2015, GRL) and also in text books. All terms may not be important but they could at least be mentioned.

> Agreed. It is certainly the case that SSH/transport relationship has other contributors. We have modified the text in the second paragraph of the Introduction to address this omission.

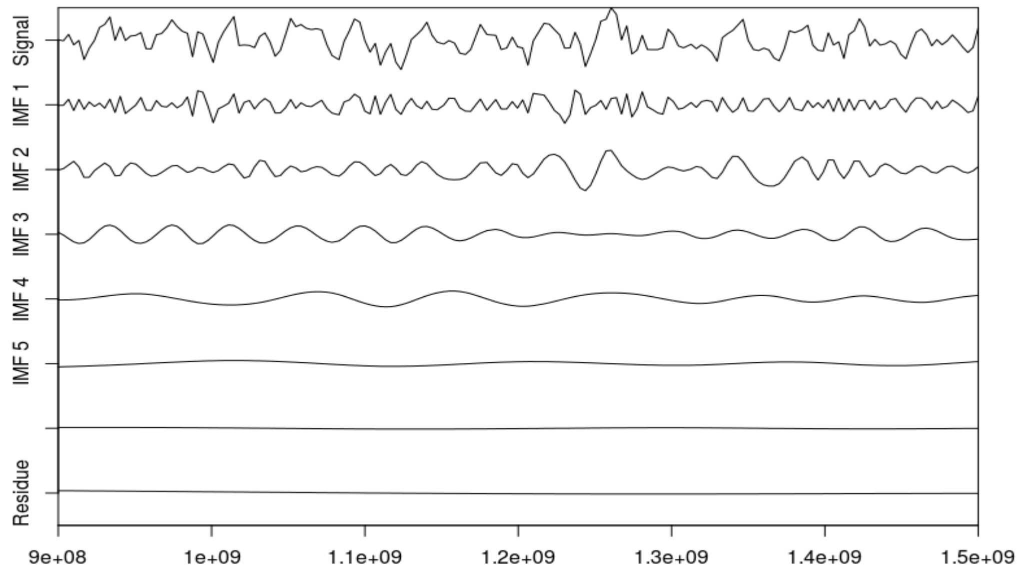
Figures 4 and 5:
A second is that the MSL data is not IB-corrected, as far as I understand the paper, which it should be for comparison to transport variations.

Thank you for pointing out this important physical forcing. We expected that the effect would be rather insignificant on the timescales we examine since the power spectrum of barometric pressure is dominated by atmospheric tides and annual cycles. It would be surprising if a signal in the MSL trend over a decade, (at least an essentially monotonic rise as in our case) would be driven by barometric pressure. While the NAO is linked to North Atlantic SSH, the primary signal which we link to transport is not a decadal or shorter-scale oscillation such as the NAO. Shorter-scale oscillations of MSL, including those expressing atmospheric pressure effects, are captured by the EMD IMFs and are not in the EMD residuals or decadal IMFs. Nonetheless, as the reviewer points out, the physical forcing and response of barometric pressure are of primary importance to water level, thereby the foregoing expectations should be examined.

At the Vaca Key tide gauge pressure data is available from March 2000. Shown here are the hourly pressures (black) and monthly mean pressure (red).



In the analysis we examine the EMD residual of monthly mean water levels, and relate those to EMD residual of the transport data. An EMD of the monthly mean pressure is shown here (time units are seconds from Jan 1 1970):



This visually suggests that there is no trend in pressure since 2000. Summary statistics on the pressure, EMD residual, and IMFs 5 and 6 are tabulated here:

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Monthly Mean Pressure (Signal):
   Min. 1st Qu.  Median    Mean 3rd Qu.    Max.
   1012    1015    1017    1017    1018    1022

EMD Residual:
   Min. 1st Qu.  Median    Mean 3rd Qu.    Max.
   1017    1017    1017    1017    1017    1017

IMF 6:
     Min.   1st Qu.    Median      Mean   3rd Qu.      Max.
-0.133500 -0.081280 -0.006017  0.004583  0.092020  0.153400

IMF 5:
      Min.    1st Qu.     Median       Mean    3rd Qu.       Max.
-0.5716000 -0.2653000 -0.0052080 -0.0001251  0.2607000  0.5564000
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As pointed out by the reviewer, the monthly mean pressure has potential to vary water levels by approximately 10 cm, contributing to the annual variance of monthly mean water levels shown in figure 4 of the manuscript. However, the EMD residual is static, with the two lowest frequency IMF's able to contribute about 1.3 cm to SSH variance. We therefore expect that the monotonic increase in sea level we find at Vaca Key of 7.4 cm over the last decade is not coupled to barometric pressure.

We have added mention of this in Section 4 describing the mean sea level data.

A third relates to the trends shown for 3 stations in Figure 4 which are compared, via the residual EMD, to trends in transport in Figure 5. What is the role of vertical land movements in these tide gauge trends? They should at least be mentioned even if they are thought to be small.

> Thank you for this addition. As the reviewer suggests vertical land motion (VLM) on the Florida Plateau is quite small with most areas estimated to have a subsidence rate of less than 1 mm/yr. This has been added to the text in Section 4 describing the mean sea level data.

Page 553, lines 12-25: Some more relevant papers that could be mentioned here for the MAB are Woodworth et al. (2014, JGR), Thompson and Mitchum (2014, JGR), Goddard et al. (2015, Nature Comm).

> Thank you for bringing these our attention. We agree that they are relevant and informative to the topic and have added them into the discussion.

Page 555 and caption for Figure 2 - what does the 'data reconstruction' for the missing data in effect mean? You take the climatological averages?

> In a sense we have a climatological average, but we allow the sample for a reconstructed data point to be drawn from any percentile of the distribution, not just the median. The reconstructed data are uniformly sampled from distributions constructed from all available data for a missing yearday. For example, if January 1st 2000 is missing, then a Gaussian kernel is fit to all available data for January 1st. A random sample is then drawn from this distribution and used as the reconstructed value. This preserves the overall distribution of the data for a yearday, while realistically allowing for variance away from the mean. Clearly, other techniques could be applied, but for the purposes of this analysis which focuses on decadal and longer scale change, this surrogate was deemed appropriate as it does capture the annual cycles and daily variability.

Page 555 line 14 - .. of the ship data with time ...
line 19 - .. 0.002) is obtained.
line 20 - +/- 1 Sv (reference?)

> Corrected.

Page 555 line 21 - .. in mean transport over the last decade.
My point here is that you have said in the Abstract that the trend in the last decade was not significantly different, but it is not flagged very strongly in the text here.

> Please consider the sentence and paragraph following this line (p 555, line 21), which is one of the main points of the paper, that linear regression over the period of record seems ill-suited to the detection of nonlinear, time-varying trends. We have expanded the discussion of this topic in the Introduction. More on this topic immediately follows in the next comment.

Doesn't it worry you a little that there is no significant difference in trend using regression but there is with the EMD method? It makes you wonder how real the effect is.

> Thank you very much for this comment. As above, we find that the data supports the notion that linear regression over the period of record is not the best tool to detect and quantify a time-varying trend embedded in a multidimensional signal where nonlinear, process-driven cycles, not just random noise are present. This is mentioned in the text (p 555 line 22 – p 556 line 8), and this very issue: when is a linear regression appropriate to represent a time-varying trend?, is the reason we suggest Wu et al. (2007) to the reader as a primer on the relevance and extraction of time-adaptive nonlinear trends. Discussion of this has been expanded in the Introduction.
>
> In response to a comment from Reviewer #2, we have added a comparison of linear trends with the EMD residuals in Figure 2. From a linear perspective there is not much difference in the cable data versus the EMD residual (if one were to engage in a linear model of the EMD residual). However, we suggest that a single coefficient over 33 years is not appropriate to capture time-varying trends such as the recent decadal decline in AMOC and rise in South Florida sea levels. So, our perspective is that a linear regression is perhaps the wrong tool for the job when it comes to trend analysis of geophysical data, which in this case is known to contain significant variance that is not random-process. Indeed, the emergence of time-dependent decompositions such as EOFs (applied to timeseries) and wavelets are recognitions that geophysical processes are often not best represented with a single time-invariant coefficient.
>
> Also, as mentioned in the introductory note to this review, the recent paper of Ezer (Global and Planetary Change 129, 2015) examines this issue regarding a debate over Gulfstream variability in the Mid Atlantic Bight, and reach a similar conclusion.
>
> As to the question of 'how real the effect is', we are always guided by the observational data itself in deference to models. This is the reason that we make the raw transport data, EMD residual and low-frequency IMFs a prominent graphic (figure 3), and that we took care to minimize the vertical scale so as not to exaggerate a perception of transport change over time. If there was a noticeable discrepancy here, an immediate concern would be raised, but both the mean behavior and low-frequency variability are well-represented by the EMD. The supporting physical argument is the MSL behavior coincident with the detected transport decline.

There is a problem in this section as the reader doesn't easily know what timescales the IMFs for each analysis relate to. Could a table be provided which shows the period bands in each case? I take it (line 9) that IMF 17 is the lowest frequency (apart from the residual). And then the analyses for NAO etc. uses a different number of IMFs. Needs a decent table.

> Yes, the first IMF contains the smallest temporal-scale variations, with successive modes capturing longer-scale (lower instantaneous frequency) changes. A table has been added listing the range of instantaneous frequencies for each of the modes shown in the figures.

p. 556, line 9 - longest? You mean lowest frequency?

> Yes. Longest temporal scales which correspond to the lowest frequencies. Clarified in the text.

p. 556, line 19 - 'with a reduction in AMOC since 2004'. Does this refer to Ezer et al. (2013) or something else? If so, give reference. It is unclear at the moment.

> We have clarified this with explicit citation of Robson et al. (2014) and Smeed et al. (2014).

page 558, line 21 - '4 through 7' means what? See above.

> This refers to the individual MEI IMFs numbered 4 through 7 (perhaps 4 - 7 is better) as shown in figure 3f. We are attempting to convey the observation that the strong El-Nino events in '82 and '97 (as expressed in the green curve of figure 3e) can be attributed to a (random?) synchronization of IMF's 4, 5, 6 and 7. We have clarified this in the text.

p560, line 2 - define SD

> Corrected.

p560, line 4 - subscript T

> Corrected.

p560, line 5 – straits

> Corrected.

p560, lines 14 on - see above regarding land movements and IB correction

> Please see above regarding the IB correction and subsidence.