Reviewer 1

The authors use available data and model products to quantify correlation between different components of the Gulf Stream system during the past couple decades. The main take-home message is that, during recent interannual and decadal periods, the system shows gyre-scale structure: subtropical circulation features—Florida Current, western boundary current, Oleander Gulf Stream—are correlated with one another to varying degrees, but uncorrelated with elements in the Nordic Seas—Barents Sea Opening, Svinøy, Greenland-Scotland Ridge inflow. The authors also identify potential forcing mechanisms related to large-scale modes of surface climate variation as well as whether these circulation features exhibit significant trends.

This is an excellent paper. The authors study an important question relevant to large-scale observing systems of the North Atlantic. By focusing on particular flow features within the Gulf Stream system, rather than overturning streamfunction, the authors provide a valuable new perspective on the meridional coherence of North Atlantic Ocean circulation—a topic of longstanding interest. The paper is well-written and clear, the reasoning is logical, and the conclusions follow naturally from the results. From what I can tell, the methodology and analysis are scientifically sound.

I found very little (if anything) to criticize here. My minor comments are given below. The paper should be suitable for publication after minor revisions.

Congratulations to the authors on a very nice study

Best,
Chris Piecuch, Woods Hole

We thank the reviewer, Chris Piecuch, for constructive comments and encouraging feedback. Please find our response below to each of the points raised.

* References to "the Bahamas" (lower-case "t") should be changed to "The Bahamas" (upper-case "T")

We have changed to the correct capital 'T' accordingly.

* I found this sentence near lines ~90-95 confusing: "along a transect from New Jersey to Bermuda (Rossby et al., 2005). The ADCP measurements reach 250-400m depth for the 1992-2004 period, and 500-600m from 2005 and onwards (Sanchez-Franks et al., 2014)." To me, it reads like the ADCP measurements are made *over the range* 250-400 m for 1992-2004 and *over the range* 500-600 m from 2005 onwards. Rather, I think what the authors mean is that the ADCP measures velocities *down to* 250-400 m and *down to* 500-600 m from 2005 onwards. I suggest to clarify.

As suggested by the reviewer, we have altered the sentence so that it is clear the ADCP measures velocities down to the depth ranges stated (l.92-93).

* ~Line 134 "... ocean bottom pressure FROM GRACE AND GRACE-FO"

We have added this information to l.136.

* I think the figure reference on line 142 should be to Figure 4?

Thanks for pointing out this imprecision – it is more accurate to refer to Figure S4c here (l.151).

* It'd be informative to explain how the authors compute "Florida Current Transport" in ECCOv4-r4 and how thy distinguish it from the total western boundary current transport. The resolution of ECCOv4-r4 is very coarse, and the model bathymetry in that region is heavily modified, such that the

"Florida Straits" in the model are much broader than in reality, and the depiction of The Bahamas very unrealistic.

As the reviewer states, the coarse ECCO-grid does not fully resolve the complex topography in e.g. the Straits of Florida. We have added a paragraph in the methods section explaining the process in defining currents in ECCOv4-r4 that are comparable to the observed currents (l.139-145). We explicitly state how the Florida Current and the Western Boundary Current at 26.5N were defined on the ECCO-grid. We have also added a section to the Supplementary detailing all six transports section definitions in ECCOv4-r4.

* Can the authors explain why they normalize the ECCOv4-r4 and observational time series for comparison? I think the result would be more powerful if the authors didn't divide by the standard deviation.

Based on this comment and similar comments from Reviewer 2, we have chosen to alter Figure 4 so that panel a) shows the volume transport anomalies from the time mean instead of the normalized volume transport. We also find that this makes for a more intuitive comparison with ECCOv4-4.

* Minor point on ~Line 205 and Table S1. I suggest to place the correlation values in the lower-left part of the matrix rather than the upper-right so that it's easier for the reader to go back and forth comparing to the corresponding values in Table 2.

Following the reviewer's suggestion, the correlations in supplementary table Table S1 have been moved to the lower-left corner accordingly.

* ~Line 235 the authors should also cite work by Lobelle et al. on this point (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL089974)

A reference to Lobelle et al. 2020 has been included in l.262.

* Line 241 table -> tables

This has been corrected.

* Line 259ff and Figure 5. The authors' analysis here, being based on correlation coefficients, only considers in-phase or anti-phase relationships between transport and sea-level pressure. But earlier, the authors talked about the various mechanisms that mediate the ocean response, which include processes that impart lags and phase differences between forcing and response. Are the authors confident that figures like Figure 5 entirely capture relationships between transport and sea-level pressure? It may be informative also to consider the relationship between transport and the Hilbert transform of sea-level pressure.

In Figure 5, we shed light on one well-known driver of ocean variability (atmospheric circulation represented by sea level pressure), which at zero-lag serves as a partial explanation for what the current measurements show. The reviewer makes an important point in that the ocean response to atmospheric circulation is complex and occurs on a range of time scales. Additionally, the ocean influences the atmosphere through SST feedback. We have added the following paragraph to be clearer on the interpretation of, and limitations to, the regression analysis shown (l.325-329): "We do find relatively straightforward relationships between regional atmospheric circulation (represented by sea level pressure) and the section volume transports. These zero-lag regressions (Figure 5) are likely most representative of sea level pressure patterns related to ocean circulation's relative immediate barotropic response to anomalous atmospheric forcing (e.g., Eden & Willebrand 2001). It should be noted that the ocean responds to the atmosphere on a range of time scales and also influences the atmosphere through feedback mechanisms (Marshall et al. 2001)."

* Line 259 and elsewhere (e.g., Table 2). The authors refer to their analysis as measuring "coherence" between different quantities. This isn't strictly true. Coherence is a measure in the frequency domain. I suggest the authors adjust the language to more clearly say theirs is a correlation analysis not a coherence analysis.

We acknowledge the reviewer's note on the appropriate use of terminology. However, in common language and in scientific literature (e.g., Bingham et al. 2007, Gu et al. 2020, Frajka-Williams et al. 2023), a similar use to ours is typical. As a middle ground, we have partly remained by our use but also elaborated on the usage, hopefully in line with the reviewer's suggestion. Specifically, we have added a sentence explaining our non-statistical use of the expression 'meridional coherence' in the methods section (l.179-180) and altered the sentences mentioning 'coherence' prior to the explanation (e.g., l.6, l.39, l.63). We have also changed the word 'coherence' to 'correlation' in the table captions for Table 2 and Table S1.

* Line 279. indecies -> indices

This has been corrected.

* Discussion section. The paper focuses on volume transports. But, with the exception of coastal sea level, what we really care about from a climate perspective is the transport of heat and other tracers like carbon. Based on their results, can the authors briefly speculate on the potential meridional coherence of heat transport or transport of other tracers?

The reviewer raises a very valid and intriguing point. We have added a paragraph to the discussion section putting our results in the context of poleward propagation of heat anomalies (l.364-367): "While observations and models show that ocean heat anomalies and other tracers can propagate persistently poleward through the North Atlantic Ocean, leading to potential for skillful climate prediction (e.g, Keenlyside et al. 2008, Årthun et al. 2017), our results herein show that volume transport anomalies do not. Therefore, the mechanism by which the gyres exchange, for instance, heat anomalies, remains unclear and is thus a challenge to address following up on the present study."