Ref.: os-2021-10
A dynamically based method for estimating the Atlantic overturning circulation at 26N from satellite altimetry

Reviewer #2

Review of Ocean Sciences 2021-10

This article presents an estimate of the overturning transport at 26N based solely on satellite data. In doing so, it investigates the structure and correlation of each part of the calculation, with an emphasis on the mid-ocean transport.

This is a worthwhile publication that combines previous findings with new analyses and creates self-consistent analysis that will be of use to the field. It is well written and has graphics that are a pleasure to look at. My main concern is that there are a number of steps that lack a physical motivation and seem ad hoc, especially so because the purpose of the approach taken is not made explicit. More generally, the paper would be clearer if it were more explicit about its goals, what is novel, and how how it compares with earlier studies.

We thank the reviewer for their constructive comments.

We have now revised the manuscript to include further background information and discussion from previous studies to give better context to results from this work. We have also re-written some sections to add clarity on the novelty of this work and its advantages and disadvantages compared to previously published methods.

A supplementary materials section has now been added with new analysis showing the results of sensitivity testing depending on the choice of Gaussian filtering of the data on the results (Fig. S1, S6, S7, S8). An additional figure comparing SLA to dynamic heigh anomaly thickness has been added to provide a more complete discussion of the relationship between SLA and dynamic height (Fig. S2) in section 3. Further discussion of the scale factor a new figure showing east-west difference in dynamic height vs SLA (Fig. S3) has been added to section 4.3. A new figure showing transport anomalies from the reconstructed dynamic height anomaly for the first baroclinic mode, the second baroclinic mode, and the barotropic mode has been added and discussed in section 4.4 (Fig. S4). Finally, a new figure showing the correlation map between the SLA in the Florida Straits and the cable-based Gulf Stream transport (Fig. S5) is added and discussed in section 5.

Details for all these changes are described in the reply to specific comments below. All reviewer comments are itemized and addressed in blue. Line numbers given here are in reference to the tracked changes manuscript. New text added to the manuscript is in red.

Detailed comments:

1. Abstract: I did not get a clear sense of how this paper fits into the existing literature from reading this abstract, and what analyses were done and to what immediate purpose.

Details on relevant background literature and the associated knowledge gap have now been significantly expanded on in the entire introduction section.

2. Equation 4: Since surface geostrophic velocity is not treated further, suggest removing this equation.

Equation 4 is now removed.

3. section 3.2, figure 2b. This section and the data presented are not presented in a clear manner. It is not correct to correlate two quantities that have a common signal (SLA) and interpret statistially significant correlation as "the variability at the sea surface is a good measure and coherent with variability to at least 1000 dbar". The reader could readily counter by stating that the correlation is meaningless, as correlating A (=SLA) and A+B (=SLA+dynamic height) just shows that A is coherent with A. Either correlate A=SLA and B=dyn height and discuss those results, or discuss that SLA provides greater than 64% of the variance of the surface-reference dynamic height, and thus that SLA is more important for transport than vertical structure of dynamic height

The point of figure 2b is to illustrate how well and to what depth SLA captures sub-surface variability. As suggested by the reviewer, we have also added a new plot in the supplementary materials (Fig. S2) showing the dynamic height anomaly thickness (0 -1100 dbar) which provides a measure of shear independent of the reference level. Corresponding text is added in L378-384: For completeness, the SLA is also compared to dynamic height anomaly thickness for the 0-1100 dbar layer (Fig. S2). The dynamic thickness is calculated from the difference in dynamic height (referenced to 4820 dbar) at 0 and 1100 dbar and gives a measure of the shear between those levels (independent from choice of reference level). In both the western and eastern basin, the dynamic height thickness anomaly has a standard deviation smaller (0.30 $m^2 s^{-2}$ at WB3 and 0.07 $m^2 s^{-2}$ at East) than the SLA multiplied by gravitational acceleration (0.70 $m^2 s^{-2}$ in the west and 0.14 $m^2 s^{-2}$ in the east). This result suggests that the baroclinic structure in the upper 1100 dbar is not overwhelming the SLA signal, and the SLA can be used for transport estimates in the upper 1100 dbar.

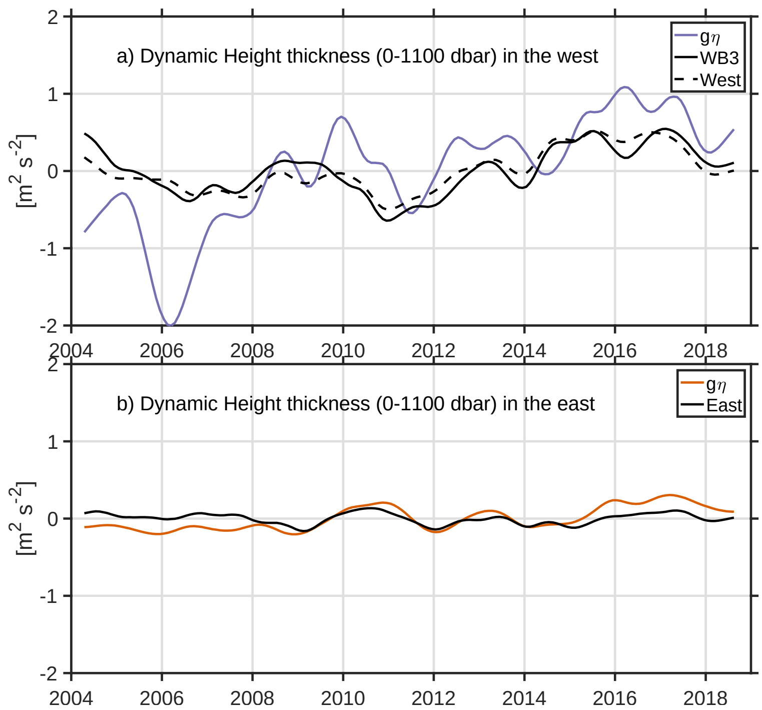


Figure S2. The dynamic height thickness anomaly at moorings (a) WB3 and West, and (b) EB moorings. The dynamic heigh thickness is compared with the SLA scaled by gravitational acceleration in the western and eastern basin, respectively. Units in $m^2\ s^{-2}$.

4. Equation (5) is incomplete because it doesn't reflect that SLA is added, as described in the text.

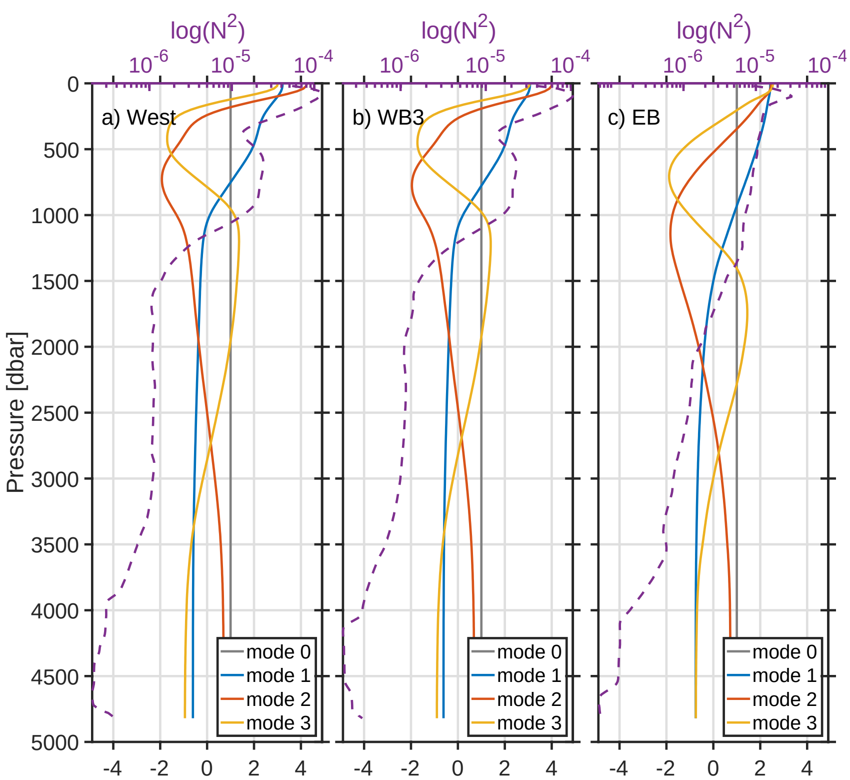
Equation 5 shows how dynamic height is estimated from the moorings and was moved to the methods section (section 2.1, L176), to clarify how dynamic height is computed, where it is more appropriately placed. It is now equation 1 and used before the idea of adding SLA was introduced.

5. Figure 2b. The two-tailed t-value confidence limits on correlation seem inappropriate for this case: is the confidence limit of correlating SLA with itself (at p=0) truly non-zero? Maybe this reflects that it is hard to get physical meaning out of correlating A with A+B, and that the statistical question or the conclusion needs to be posed a different way.

Please see reply to comment 3.

6. line 219. It is not physically possible for the buoyancy frequency to be zero, unless the water is perfectly homogeneous (which it isn't). Suggest plotting N(z) in Fig 4 in log-space to more clearly show its structure.

Apologies, this was indeed an error in interpretation based on the linear N(z) plot. N(z) is now plotted in log-space on the X axis, and the comment in the text has been updated (L447: "before decreasing to background stratification levels around 1100 dbar").



7. lines 250-265. This is a very dense paragraph with lots of numbers and shifting references that was difficult to comprehend.

Thank you for pointing this out. That paragraph (now L511-528) has been rewritten for clarity: Fig. 5a shows the RMS dynamic height and sea level anomaly (η multiplied by gravitational acceleration) at two latitudes across the Atlantic. At the eastern boundary, the RMS of gη is about twice (0.18 m^2 s^{-2}) that of the dynamic height from the moorings (0.08 m^2 s^{-2}) (Fig. 5a). Moving west, RMS values of gη peak at 74°W and 75°W (RMS of gη(27.875°N, 74°W) = 0.70 m^2 s^{-2} and RMS of gη(26.125°N, 75°W) = 0.59 m^2 s^{-2}) before decreasing to the western boundary (RMS of gη(27.875°N, 77°W) = 0.56 m^2 s^{-2} and RMS of gη(26.125°N, 77°W) = 0.39 m^2 s^{-2}). The rapid decrease in sea level variability at the western boundary is due to mesoscale suppression associated with the continental slope (Kanzow et al., 2009). The moorings, in contrast, show lower variance in dynamic height at both the eastern and western boundaries (RMS of ϕ (26.5°N, 76.75°W) = 0.31 m^2 s^{-2} and RMS of ϕ (East) = 0.08 m^2 s^{-2}). A scatterplot of the east-west difference in surface ϕ (i.e. $\Delta\phi = \phi_{East}(z=0) - \phi_{West}(z=0)$) and the equivalent from sea level anomaly ($\Delta g\eta$ = g(η(27.875°N, 13.125) − η(27.875°N, 74.375))) shows general agreement between the two values. The slope of the regression line is ~0.25, and the intercept goes through the origin (1.3e-17) (Fig. 5b). Fig. 5 suggests that the satellite altimetry alone does not capture the same magnitude of signal observed by the moorings. One of the reasons for this discrepancy may be due to the proximity of the moorings (e.g. WB2) to land, where variability experiences changes due to coastal processes (Kanzow et al., 2009). Kanzow et al. (2009) find that the signal is reduced near (within 100 km) to the western boundary, thus altimetry may overestimate variability in this region. The value of the slope of the regression line, i.e. 0.25, is thus used as the scale factor for the satellite where indicated (e.g. Eq. 10).

8. equation 11. What is the physical reason for adding a scale factor to this equation? What purpose does it serve? This seems like an ad hoc decision, and it needs to be justified. How the scale factor is computed also needs to be described clearly. The sentence "The scale factor was determined ... against eta." (lines 250-251) is insufficient, and further contradicts the description later in the same paragraph that the scale factor comes from fitting _differences_ of eta (shown in fig 5b). It is not logical that a scale factor for eta is related to a scale factor for differences of eta.

Further justification and an additional figure (S3) has now been added to the text (L503-509): Where $F_1(z=0)$ is the surface value of the first mode, and s is a scale factor equal to 0.25, needed to adjust η to the correct magnitude. SLA does not account for baroclinic shear in the upper 1100 m of the water column: if we integrated the SLA over the upper 1100 m to obtain geostrophic transport, the SLA would overestimate the transport magnitude compared to dynamic height (e.g. S3). For this reason, it is necessary to combine the SLA with the vertical structure of the first baroclinic mode; however, a comparison between the dynamic height from the RAPID moorings and the SLA indicate further correction/scaling could be needed. Thus a scale factor is determined empirically by examining the signal from ϕ at the surface (z = 0) at moorings West and EB against η.

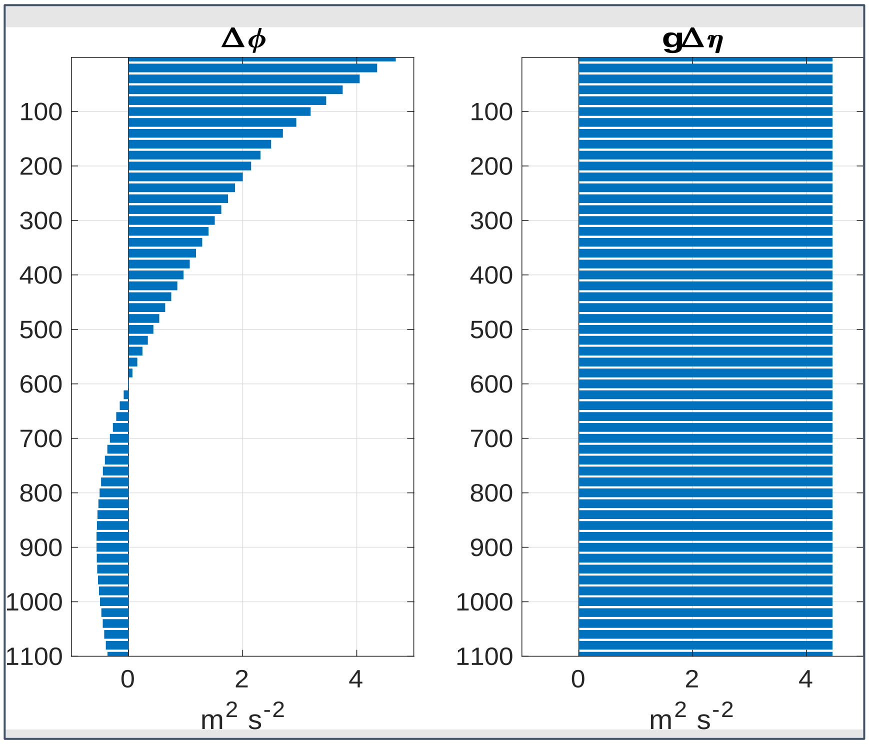


Figure S3. Time averaged east-west difference in dynamic height, $\Delta\phi$ (a), and SLA multiplied by gravitational acceleration, $g\Delta\eta$ (b), at every pressure level. Units in $m^2 s^{-2}$.

Also please see reply to comment 7.

9. Please provide confidence limits for the slope shown in 5b, providing a correlation coefficient R would be nice too. To my eye, this plot seems to show a deficiency of least squares in underestimating the slope because there is assumed to be no error in the dependent values. A principal component analysis or alternate least squares formulations are needed to account for uncertainty in both dependent and independent variables.

Confidence limits for the slope now included in the Fig. 5b.

10. lines 263-265 "The comparison between phi(z=0) ... observed by the moorings". This logic doesn't make sense to me. To play devil's advocate, if the altimetric SLA has a higher signal than the moorings (fig 5a), then shouldn't the interpretation be that the SLA (being larger) captures more of the signal than the mooring? There's lots more going on, of course.

Thank you for pointing this out, we have now clarified in the text to say "satellite altimetry alone does not capture the same magnitude of signal observed by the moorings" and the comment on the sources of 'missing' variability in L525: Kanzow et al. (2009) suggest that near (within 100 km) of the western boundary, variability becomes influenced by a combination/mixture of barotropic-baroclinic flow over the slope, reduced eddy variability, and coastally trapped waves.

11. Fig 5a. This figure shows dynamic height, but, because dynamic height is less than the rms of SLA, it seems like dynamic height is _not_ referenced to SLA. In contrast, previous discussion (section 3.2, fig 2b) clearly do reference dynamic height to SLA. Please make

clearer what quantity is being used, and preferably use the same quantity consistently throughout the paper. If there's merit to use dynamic height referenced to SLA in some cases, and straight dynamic height in other cases, then please add reasoning to explain what insight is provided by using both methods.

Thank you for pointing this out, we have now clarified in L348 that dynamic height is referenced to SLA only in section 3.2 and Fig. 2b, and have assigned a different symbol for dynamic height when referenced to SLA at the surface: ϕ_η.

12. line 274-275 For the trend in mode values to be real, as stated here, requires that the mode fits are completely stationary over the 15 years of records. Two factors need to be investigated before this conclusion can be reached. First, does the stratification changes over the time series? Using a constant stratification for the mode fits assumes stationarity. Second, do the sampling depths on the moorings remain constant from 2004 to 2018? Changes in sampling depths can easily change how the CTD profiles project onto vertical modes.

Mention of the trend has been removed. They are not likely significant and not an integral part of the results.

13. lines 317-318 "All three transport estimates show slowly increasing trend over the 13 year period at West and EB". Trends like this can also result from the comment above, about how either sampling depths or stratification is not stationary over this 13 year span.

Thank you for these suggestions. Mention of the trend has been removed. They are not likely significant and not an integral part of the results.

14. line 323, fig 9f. It is practically impossible for 2 independent time-series to have a correlation of 1. Please provide more significant figures for this R value instead of rounding it up.

Figure 9 now has 3 digits of precision for the r values.

15. line 323-325. Instead of saying the barotropic mode "plays a non-negligible role in the total variance", why not quantify its variance, as can easily be done with the numbers presented here?

Thank you for this suggestion. The contribution of the barotropic mode is now quantified in the abstract, results, and conclusions. There is also a new figure (S5) quantifying the contribution from the barotropic and the second baroclinic mode separately (see section 4.4) in L750: Analysis of the barotropic component shows that it accounts for 86% and 63%, respectively, in the total variability at West and EB (Fig. S4) and could explain some of the discrepancies in the satellite-based estimates, as satellite altimetry only captures baroclinic variability. The second baroclinic mode is shown to account for 18% and 0.34% (not statistically significant) of variance at West and EB moorings (Fig. S4).

And in L782-785: These results suggest that the time-averaged first baroclinic mode accounts for most of the interior geostrophic transport variability; while the barotropic mode accounts for 82% of the variability, and the combined barotropic and first baroclinic mode accounts for 98% (Fig. S4). The barotropic mode is reflective of changes in the deeper less stratified ocean.

16. Section 5. There have been studies done by people at AOML about using sea level to estimate the Gulf Stream Transport, and perhaps even using SSH, that would be good to reference in this section. Many details are skipped over here - such as the SLA difference going across the Bahamas, the mismatch of time-scales between the cable voltage measurements, the SLA values, and the satellite altimeter results of Volkov et al. If these details are not important for this section, then say what the goal is succinctly - to identify the most accurate altimetry-based proxy for T_GS?

Thank you for this suggestion. Further background information of the Gulf Stream has been added to the start of this section (5) and an additional figure showing a correlation map between the SLA and cable-based GS time series has been added to the supplementary materials (Fig. S4): The Gulf Stream within the Florida Straits has a mean of 31.2 Sv and is balanced by the UMO and Ekman transports, mean of -18 and 3.74 Sv, respectively, to yield the total mean AMOC transport of 17 Sv (McCarthy et al., 2015). The Gulf Stream time series is based on a submarine telephone cable that has been recording data the Bahamas and Florida at 27°N since 1982 (Baringer and Larsen, 2001). Principles of geostrophy have been previously used to provide alternative mechanisms for estimating the cable-based T_{GS} using satellite altimetry (Volkov et al., 2020) and pressure gauges (Meinen et al., 2020).

Here, the east-west difference in η ($\Delta\eta$) in the western end of the basin (i.e. west of 77°W) is compared with the submarine cable data (Baringer and Larsen, 2001). Maximum correlation between T_{GS} from the cable data and the satellite $\Delta\eta$ (r = 0.70, statistically significant at 95% level) is found when using η_E located at 27.625°N and 77.125°W, and η_W located at 27.625°N and 80.125°W (Fig. S4). Similarly, Volkov et al., (2020) used along-track satellite altimetry to infer T_{GS} measured at the level of the Florida Straits, where they found that satellite altimetry captures 56% of variability observed from the T_{GS} estimated from submarine cable records for the 2006-2020 time periods, where the cable-based transport estimates were subsampled at 10-day intervals to coincide with the along-track satellite passes. We use gridded altimetry with a similar objective (to determine the Gulf Stream transport via the Florida Straits from satellite altimetry) but with a focus on the lower-frequency variability rather than an instantaneous comparison with cable measurements.

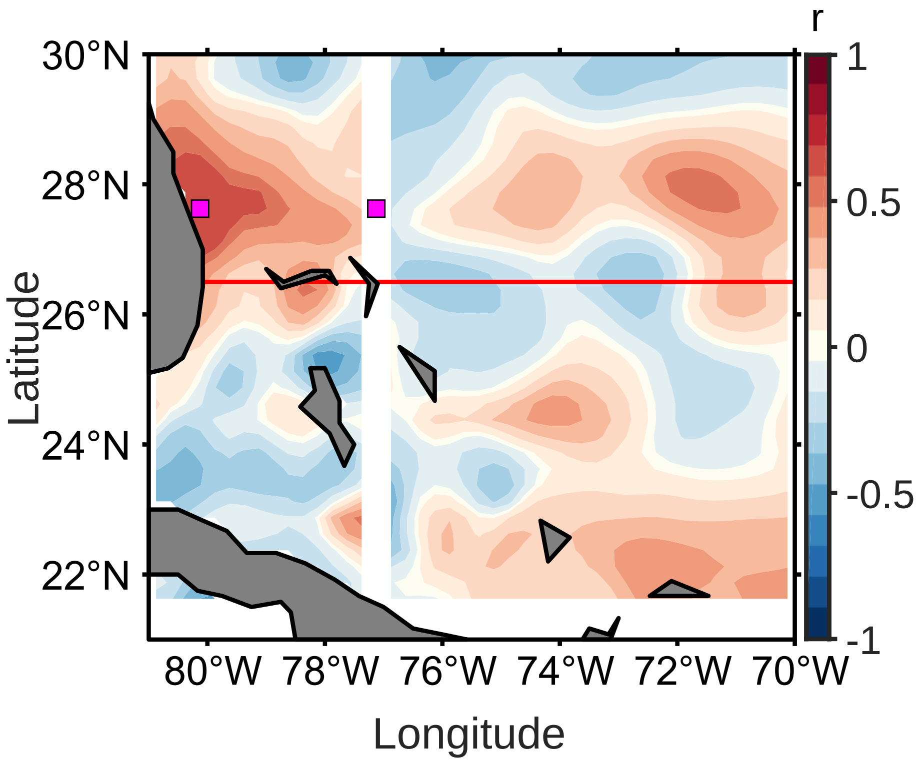


Figure S4. a) Correlation map between east-west difference in SLA ($\Delta\eta$) and the telephone cable Gulf Stream transport (T_{GS}). Magenta squares in the western and eastern part of basin indicate region of maximum correlation between $\Delta\eta$ and T_{GS}.

17. equation 18. This does not make sense, the units on either side of the equation are inconsistent. What is the "8"? How is it calculated? What is its uncertainty? In any case, what is the physical reason for adding a scale factor into this equation?

The satellite-based Gulf Stream transport was estimated from linear regression using the cable-based Gulf Stream transport and the $\Delta\eta$ where correlation was maximum (Fig. S4). The regression coefficients were then applied to $\Delta\eta$ to produce a final estimate for the satellite-based Gulf Stream transport. This has now been clarified in the text L820: Here, linear regression is used against the cable-based T_{GS} to obtain the regression coefficients (*a, b*) needed to produce a final estimate for satellite-based GS transport:

$$T^*_{GS}(t) = a\Delta\eta + b, \tag{17}$$

Where a = 8.0087, b = 0.0026, the eastern and western SLA points that correspond to $\Delta\eta$ are located at 27.625°N and 77.125°W for η_E, and at 27.625°N and 80.125°W for η_W. The accuracy of T^*_{GS} was determined using a Monte-Carlo technique, where 90% of the time series was randomly sampled 10,000 times, performing a linear regression to obtain an upper (mean value of 0.0614 Sv) and lower bound (mean value of -0.0614 Sv) for the 90% confidence intervals. The satellite-derived T^*_{GS} captures 49% of the variability of the in-situ T_{GS} and generally underestimates the amplitude of variability (Fig. 10a,b).

We have looked at the sensitivity of the GS transport to the regression by using a Monte Carlo method which used the confidence intervals from the regression (L826). Figure 10 shows a measure of spread (blue shading) which indicates the GS transport is not very sensitive to the regression we have used.

18. line 345. Please provide the upper and lower bound referenced in this line.

The upper and lower bound mean values are now given.

19. lines 364-367. How does the Monte Carlo method give an error that varies with time (as plotted in fig 10)?

The mean upper and lower bound values estimated from the Monte-Carlo technique (L676-678) are multiplied by the time series in question.

20. line 383. Since neither "geostrophic velocity" nor "vertical structure of flow" was presented, suggest replacing with "SLA" and "dynamic height".

We have now made this change.

21. line 396. "... to provide a more physically robust method." This is debatable, especially given the seemingly ad hoc decisions made earlier involving scale factors. An important advance I see is that it gives a methodologically consistent (satellite only) method that would be straight forward to apply to other latitudes.

We have now modified this sentence as suggested (L913): Here it is fitting to compare the EFW15 method with the method derived in sections 4 and 5, which relies on altimetric data and geostrophic balance, and therefore gives a methodologically consistent (satellite only) method that incorporates climatological stratification and has the potential to be applied to other latitudes. This is in contrast with EFW15 which was a statistically based method that assumes the existence of in situ observations. Further work is needed to determine the origins of the scale factor required to balance the transports, whether due to sampling issues or uncertainties in satellite sea level anomaly near the boundaries.

22. lines 423-426, 429-430. When I look at the plotted data, I do not see the conclusions as stated in the text.

Thank you for pointing this out. "All three TMOC reconstructions show a weakening in the southward flow in 1996 or 1997, followed by an intensification toward the late 90s. This general mid-to-late 90s strengthening is in agreement with changes in the Atlantic Multidecadal Variability phase, which was marked by a positive phase in the late 1990s (Zhang et al., 2019). " has now been corrected to "The EFW15 T_{MOC} reconstruction shows a weakening in the southward flow in ~1996-1997 (positive anomaly), while the T^{*}_{MOC} shows strengthening in the southward flow in 1997(negative anomaly). None of the reconstructions quite agree with the general mid-to-late 90s strengthening in the AMOC, associated with changes in the Atlantic Multidecadal Variability phase, marked by a positive phase in the late 1990s, discussed by Zhang et al. (2019)."

23. lines 431-433. The discussion could be advanced by mentioning ways in which these 3 data sets are non stationary. The poor agreement between the 3 methods in the first half of the altimetry record reminds me of the problem of overfitting. If a training data set is used to fit a model, such as done over the years of mooring measurements, then if that model is fit too closely to the data then in will not be very predictive when applied to data outside of the training set (that is 1993-2004). The ad-hoc scaling factors used to reach this point are consistent with overfitting.

Thank you for this suggestion. We have now added discussion to this effect in L965-970: Reasons for this disagreement may be due to overfitting. In the case of the satellite-based transport reconstruction, EFW T_{MOC}, the statistical method used to estimate transport relied on RAPID mooring data and resulting time series in the period pre-dating RAPID, 1993-2003, may be biased by overfitting of data during the RAPID period, 2004-2013. Similarly, T^{*}_{MOC} may be biased by choice of the scale factor, here based on comparisons between satellite and moorings during the RAPID time period.

24. summary point, lines 445-47: Why was is necessary to add a scale factor to SLA, and what is its physical purpose?

New text has been added to conclusions and summary section (as well as section 4.3 – see reply to comment 8) to clarify the point of the scale factor L1019: Because the altimetry has no knowledge of vertical shear, estimating transport over the upper 1000 m using only satellite altimetry results in an overestimation of the transport's magnitude (Fig. S3). Thus, normal mode decomposition was investigated using the RAPID moorings to answer 2 questions: a) How much does the vertical structure of the flow contribute to the upper-mid ocean transport (T_{UMO}) variability, and b) can the pressure modes be combined with the satellite to provide an improved way of estimating mass overturning transport. In the first instance, we find that the first baroclinic mode accounts for 83% of the observed interior geostrophic transport variability, the barotropic mode accounts for 82% of the variability, and the combined barotropic and first baroclinic mode account for 98% of the total variability. In the second instance, we find that combining the satellite altimetry with the vertical structure (from the 1st baroclinic mode) improves the magnitude of values for the altimetry-based transport. However, a scale factor is still needed to further correct the values to capture the magnitude of the T_{UMO}. We posit the need for a scale factor is the result of the satellite altimetry not capturing the full signal observed by the moorings, because of proximity of moorings to land, where variability is also influenced by coastal processes (Kanzow et al., 2009). It is also important to note that combining the altimetry with the vertical structure (from 1st baroclinic mode) does not improve the amount of variance captured by the satellite-based T^{*}_{UMO} compared to RAPID T_{UMO} variability, and using time-varying pressure modes (instead of time-averaged) do not necessarily improve correlation between the RAPID T_{MOC} and the satellite-derived T^{*}_{MOC}. Further discrepancies between satellite-based transport and RAPID transport may be due to the barotropic component, which the satellite-based method does not account for.

25. summary point, line 450: There was no use/discussion of Rossby Wave theory. Normal mode decomposition, yes, but that's different.

This is now corrected in text.