

Ocean Sci. Discuss., referee comment RC2
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Comment on os-2021-10

Anonymous Referee #2

Referee comment on "A dynamically based method for estimating the Atlantic overturning circulation at 26°N from satellite altimetry" by Alejandra Sanchez-Franks et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-10-RC2>, 2021

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.

Detailed comments:

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.

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

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 statistically 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

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

text.

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.

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.

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

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 η ." (lines 250-251) is insufficient, and further contradicts the description later in the same paragraph that the scale factor comes from fitting differences of η (shown in fig 5b). It is not logical that a scale factor for η is related to a scale factor for differences of η .

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.

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.

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.

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.

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.

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.

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?

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}?

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?

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

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

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

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.

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

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 it 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.

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

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