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Comment on os-2021-10

Anonymous Referee #1

Referee comment on "A dynamically based method for estimating the Atlantic meridional 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-RC1>, 2021

Review of "A dynamically based method for estimating the Atlantic overturning circulation at 26°N from satellite altimetry" by Alejandra Sanchez-Franks, Eleanor Frajka-Williams, Ben I. Moat¹, and David A. Smeed

This study examines a new dynamically-based technique for estimating the temporal variability of the AMOC and its constituents utilizing satellite altimetry, with the main goal of providing the base for efficient long-term backups for continuous estimates of the AMOC across different latitudes, as opposed to single mooring lines at key latitudes along the Atlantic Ocean. They focus their analyses on the RAPID/MOCHA/WBTS array at 26.5°N, by examining the vertical structure of the flow from the RAPID dynamic height moorings combined with satellite altimetry to evaluate a new method for estimating the AMOC variability at this particular latitude. The authors analyze the AMOC meridional transport variability from satellite altimetry and compare it with the transport estimates from the RAPID Array, they investigate the vertical structure of the circulation by analyzing the vertical baroclinic and barotropic modes from the moored measurements, and estimate the upper mid ocean transport and AMOC from historical altimetry data available since 1993 by developing a dynamically based method, which incorporates information on the full-depth vertical structure of the flows. The latter is the most novel aspect of the study.

There is a large amount of work involved, the methods are adequate, and the manuscript tackles a very important topic of research given the necessity of developing new strategies for cost-effective, long-term sustained monitoring of the AMOC. The analysis presented is of importance for the wider scientific community.

However, the manuscript is quite often imprecise, while the methods are adequate it is quite under referenced, and I think that a better flow between the different sections could be achieved. Most sections seem like separate studies and there is very little guidance for the motivation/background for each of the analyses. My general impression is that that the authors do not reach to a precise conclusion about the implementation of this new

technique to evaluate AMOC variability at 26.5°N.

In general, the manuscript would benefit from more discussion of the results in the context of the previous studies to aid in the interpretation. It is also not clear if all of the analyses described in the current manuscript need to be presented to reach a final conclusion. Another major concern is that the manuscript needs revision to highlight the new aspects of this study and to provide a discussion on how (and why) their new satellite based dynamical method improves other previous AMOC estimates using satellite altimetry, or if that is not the case it should be clearly formulated, and to explicitly mention what is new in this study compared to the other studies. This will strengthen the outcomes of the paper. In particular, the summary and conclusions section read like a list of the results from the previous sections but providing very little interpretation on how meaningful/important these results are, and most importantly, an assertive statement is missing about the advantages/disadvantages of this new method compared to other previous studies. This section needs an interpretation of the implications of the results for long-term AMOC monitoring using satellite altimetry at 26.5°N (and possibly at other latitudes).

My overall recommendation is that the manuscript cannot be accepted in its present form but it may be accepted for publication after moderate to major revisions.

Please find specific comments below:

Title: I suggest adding "meridional" between "Atlantic" and "overturning" to be more specific.

Lines 1-2: Here and throughout the text. I suggest not using "surface" when referring to the upper 1000 m. While this may be somehow a commonplace for the AMOC community this could be potentially confusing for other readers/communities. Especially in the context of this paper this is confusing because you also refer to "surface" velocity for the altimetry estimates at the ocean's surface (e.g., Line 51). Maybe just use "upper 1000 m" when you refer to the 1000 m AMOC layer carrying northward flow.

Line 7: I suggest changing "High global" for "Near-global".

Line 10: Here and throughout the text, do you mean that the method is valid only for 18-month periods or also for time scales longer than 18 months? This should be clear here and elsewhere.

Lines 10-11: I think that the "compares well" statement is arguable. I suggest this

information to be provided as how much variance is recovered by the satellite estimates for the different AMOC constituents rather than providing the correlation coefficient. For instance for UMO the variance recovered from the altimetry product is about 56% and for the Gulf Stream transport is 50%. This leaves a considerable portion of the variance unexplained by the satellite-based estimates. But for the full AMOC the variance increases to 64% (the reason for the increased explained variance by ~8-14% for the full AMOC estimate is something that should be discussed in the Results section). I feel this is much more useful information for the reader.

12-13: Is it really 17% variance for the barotropic mode? What about the other BC modes?

Lines 14-15: In the abstract these lines are the only reference to the method that gives the title to the manuscript "Finally, the UMO and the AMOC are estimated from historical altimetry data (1993 to 2018) using a dynamically based method that incorporates the vertical structure of the flow." But there is no information about the outcome of this analysis. I suggest revising in light of the novel aspects of this study compared to other studies.

In general, there should be a hyphen between "long" and "term".

Line 29: I don't think it is necessary to add " $\text{m}^3 \text{s}^{-1}$ " after volume, or perhaps add Sv. Please add "and heat" before transport and add a reference to Johns et al. (2011) (and/or newer reference if available).

Line 33: moorings are deployed on both flanks of the Mid-Atlantic Ridge, this is not clear in the text.

Line 34-35: This part of the sentence "interior flow going through them" is not clear. I suggest "basin-wide interior flow". Please add key references (e.g., Cunningham et al 2007; Kanzow et al., 2010; McCarthy et al. 2011).

Line 35: I don't think that a basin-wide density field is extracted from the rapid moorings but rather the vertical density structure at each mooring site from which the density field can be somehow estimated at the western and eastern parts of the array and to some extent near the Mid-Atlantic-Ridge.

Line 36-37: Please add a few words about the times scales for these Rossby-wave like features.

Lines 38-40: While this work is focused only in the North Atlantic MOC at 26.5°N, I think it is important to provide key references for the other international efforts to measure the AMOC from in situ arrays at other single latitudes as this is important for the main objective of this study. Frajka-Williams et al. (2019) provide a very nice review of all the arrays but at least one key reference for each of the basin-wide observing arrays should be included. I suggest: Lozier et al., (2019) for OSNAP in the subpolar North Atlantic, Hummels et al. (2015); Herrford et al. (2021) for the TRACOS/Tropical MOC array at 11°S, and Meinen et al., (2018); Kersalé et al. (2020) for the SAMOC/SAMBA array in the South Atlantic at 34.5°S. It may also be worth including a brief statement here about the limitations for studying meridional coherence of the AMOC for instance based on the large meridional distances among the arrays to provide more context for the goals of your study.

Line 41: I suggest changing "Altimetric measurements collect data..." -> satellite altimetry measures data...

Line 41-42: Change "means" for "mean" or "tool". This sentence requires references and a more extensive explanation or discussion about the limitations of using SSH or satellite altimetry for estimating boundary flows given their relevance for accurate AMOC estimates.

Lines 46-50: Please provide more information about time scales, percentage of variance explained by the proxies developed in previous studies, as these are really important aspects when computing velocities/ transports from altimetry or other ancillary data with lower temporal resolution than that provided by the in situ continuous-in-time mooring measurements.

Line 48: I think it is important to mention on which time scales.

Line 52: "While this is enough to infer AMOC variability (due to the baroclinic nature of the surface intensified ocean)". This sentence is not clear, please revise. AMOC variability on which time scales? Please provide a key reference for this statement.

Line 54: Please, change "some success" to more precise information about the results from previous studies and on what is new in this study and how this studies has improved the previous efforts.

Line 55: Please add "in the North Atlantic at 26.5°N" at the end of the sentence since that is the latitude you are exploring and the results may differ for different latitudes.

Lines 55-60: I strongly suggest highlighting the new aspects of this study here.

Section 2-5: In general throughout all the results sections, I suggest highlighting what is different/new in this work compared to the previous studies (e.g., Kanzow et al 2009; Szuts et al., 2012; Frakja-Williams et al., 2015; Volkov et al., 2020).

Line 73: Without providing information about the vertical distribution of the observations in the array this is hard to follow and not very informative. As this information is relevant for the modal decomposition at the EB (Szuts et al., 2012), please clarify here that there are a set of moorings, which are concatenated to yield a single profile (Chidichimo et al., 2010).

Lines 75-76: Please revise this sentence. It does not clearly formulate how dynamic height is computed from the T/S measurements and how the MicroCATS attached to the moorings provide continuous-in-time CTD measurements at discrete depth levels, or please provide a reference where the methods are explained. This is better explained in Line 160, probably part of that text could be moved here.

Line 83. Please add "after Kanzow et al., (2007)" at the end of the sentence.

Line 90: It is not clear why the authors choose 18 months. How sensitive are the results to the choice of the 18-month period for the Gaussian filter? Please add a brief discussion.

Line 92: Please provide threshold values of the correlation coefficient r for the significance of the correlations at the 90 and 95% confidence limit.

Line 96: Please add more references for the methods to estimate transports from the RAPID array, especially please refer to the initial papers where the methodology was developed (e.g., Kanzow et al., 2010).

Line 99: Please add a reference to previous calculations using the Florida Straits cable measurements (e.g., Meinen et al., 2010)

Equations (1)-(3): please provide the limits for the integrals (I assume they are not identical for each calculation).

Line 114: Please define the gravitational acceleration g (not defined before) for Eq. (4)

Line 126-133: Please provide more references for the key circulation aspects in the region. The results need to be discussed and put into context of previous observations.

Line 132: add "the" before "RAPID array"

Line 134: and other places: sometimes the latitude of the RAPID array is referred to as 26°N and sometimes to 26.5°N . Please pick one and be consistent throughout the text.

Line 135: Please define "rms" as "root-mean-square error" when it is first mentioned (I am aware you define RMS below, but it should be defined here as well).

Lines 142-143: Are these time series filtered? This should be clear in the text.

Lines 156-158: Please see my comment above. The definition of upper ocean (0-1000 m), surface, sub-surface should be carefully revised and consistent throughout the paper. Please also revise Lines 172-173.

Line 165-166: The updated correlation between the dynamic height from the moorings and SLA should be discussed in the context of the previous studies and also some interpretation is needed. How does a longer mooring record improve the correlation?

Line 180: "18-month smoothing" Is this a running mean? Is data lost at the beginning and end of your records? Please provide more details of the calculation.

Lines 180-181: Please also add recent references.

Line 219: should be 1000 dbar?

Line 264-265: do you mean that high frequency variability becomes more important (?)

Line 265: Which are the limitations of the value of the slope of 0.25 (is this valid across all the array at 26.5°N or at a specific location). The accuracy in the calculation due to selecting this particular slope value should be discussed.

Line 288: Perhaps reference Szuts et al. (2012)?

Line 290: add "the" between "at" and "eastern"

Line 312: Are these correlations only marginally significant at the 90%? It would be informative to know the threshold r for the significance for each confidence level to facilitate interpretation of the results. This information could be in an Appendix if it cannot be included in the main text. More interpretation of the correlations is needed.

Line 317: Is this trend significant?

Lines 316-317: How large are these trends? Are they significant?

Line 332: Please add "constructed" between "is" and "using" (second mention)

Line 339-34: Please add reference to the previous study for the correlation between the satellite estimate and the cable time series. I suggest adding "Similar to a previous study (Volkov et al., 2020)...."

Lines 381-382: "This suggests that AMOC variability can be estimated from surface geostrophic velocity and the time-averaged vertical structure of the flow" On which time scales? Which fraction of the AMOC variability can be recovered? I suggest including this information in the abstract as well.

Line 450: In general, it would be interesting to know how correlation increases/decreases for different filtering windows to provide a context for the choice of the 18-month window.

Line 454: Given that the time-varying vertical modes do not necessarily improve correlation between the RAPID AMOC and the satellite-derived AMOC, what does this mean for the new dynamical method proposed? It is not clear if this is an improvement from the Frajka-Williams (2015) study or not. If I understand correctly, their satellite-based estimate recovers over 90% of the interannual variability of the MOC

measured by the RAPID 26°N array, but if I interpret correctly with this new method developed in this study a smaller fraction of the variance is recovered from the satellite based estimate. The Summary and conclusions section misses interpretation and does not give any conclusion about the usefulness (or not) of this new method and for which time scales of variability, and whether the recommendation is to apply it or not.

I miss a discussion on how these new estimates from the satellite-based dynamical method accounting for the full-depth vertical structure of the flow at the mooring sites would be validated should there be no moorings to provide this information. Would the first BC mode and BT mode recovered from the data since 2004 enough?

I suggest the authors discussing the advantages/disadvantages of this new dynamically-based method in comparison with the previous published work and providing a recommendation about which method they propose for substitute for AMOC estimates at 26.5°N and on which time scales.

Line 462: This is perhaps a matter of taste, but I suggest replacing "monitoring" for "estimates".

Figures:

Figure 2: The black square at the EB is hard to visualize. I suggest either zooming in for this region or choosing another color for the squares (magenta?). The mooring positions are very hard to see. I suggest using smaller symbols so that the moorings symbols do not appear on top of each other. I suggest labeling West, WB3 and EB on the map to guide the reader (specially fro WB3 position).

Figures 1, 2: The moorings at the shallowest region near the EB are very difficult to visualize

Figure 3: Please add information in the figure caption about the SLA product used or refer to the section in the paper where that information can be found. The colorbar is not clear. Is the last level contoured from 0.13 to 0.14 m? (if so the last level should be labeled on the colorbar) or the last level refers to values above 0.13 m? Please revise. The a), b) labels for the left and right panels are difficult to visualize.

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