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Interactive comment on "A 30-year reconstruction of the Atlantic meridional overturning circulation shows no decline" *by* Emma L. Worthington et al.

Anonymous Referee #1

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General comments

This manuscript introduces a methodology to increase the AMOC time series beyond the RAPID period. The method uses a linear regression between the density anomaly in the thermocline, intermediate, upper and lower deep layers to compute the AMOC. The strongest result of this study is that the extended time series shows no overall AMOC decline in the period 1981-2016.

Although this result is plausible, my main reservations are related to the method used. Once the authors explain the below questions and introduce the required modifications, the paper is suitable to be published.

Specific comments

Lines 41-57. The authors describe the well known water masses in the North Atlantic Subtropical Gyre (NASG) but several papers should be referenced. For example, Hernández-Guerra et al. (2014) (and references herein) show a carefully description of water masses at 24N that should be mentioned.

Line 42 (Figure 1a). Which year does it correspond the figure to?

Figure 1. I think Figure 1a should show neutral density instead of potential density.

Lines 51-52. It states that AABW flows along the western side of the MAR but Figure 1 suggests that AABW flows in the western and in the eastern side of NASG. The potential density and other properties not shown in this paper do not confirm the presence of such a large amount of AABW at this latitude East of the MAR (and in line 52 it is defined only West of the MAR). I think the plot should be redone.

Lines 52-55. Include References.

Lines 77-78. Zero net flow holds on timescales longer than 10 days was first demonstrated by Kanzow et al (2007).

Line 87. Explain the selection of 4820 dbar as reference level, related to the change from northward AABW to southward LNADW. Include references previous to McCarthy (2015), (Bryden 2009, Kanzow 2007).

Lines 92-93. Smeed et al 2014 stated that "the majority of the change in the AMOC is associated with the UMO transport". Therefore, the UMO is the main contributor to AMOC changes, as the main contributor to the AMOC is the Florida Strait transport (with higher net values). Repeated in lines 120-122.

Section 2.2. I have a very strong concern about the model. Figure 2 plots the thermocline transport anomaly on the temperature anomaly at 400 dbar and a linear regression adjusting the data. What I can see in Figure 2 is a strong scattering of points that any linear regression could adjust as the authors find (only 20% of the variance is adjusted). From here, authors try to find another depth which could explain a higher variance. They find that at 780 dbar, the explained variance increases to 51% but any figure is shown with the data and the linear regression.

Lines 125-128. After Chidichimo et al. (2010), several papers have appeared dealing with the seasonal cycle of the AMOC and the eastern boundary of the NASG. Pérez-Hernández et al.(2015), Vélez-Belchí et al. (2017), Hernández-Guerra et al. (2017) and Casanova-Masjoan et al. (2020), among others, have found a seasonal behaviour of the Canary Current in the Lanzarote Passage that explains the seasonal cycle of the AMOC. I think these papers deserve at least a brief comment in the manuscript.

Lines 128-131. The method uses four variables to relate the AMOC and density anomalies at different depths. The use of the density anomalies related to the intermediate layer significantly increases the R2 from 0.49 to 0.74. In contrast, the deep density anomalies (UNADW and LNADW) only account for 2% of R2. This 2% explained by these two variables could be below the noise of each variable. I suggest to carry out a Monte Carlo method to estimate an uncertainty and to check that this 2% is above the statistical uncertainty.

Line 149 (Figure 3). Include uncertainties of RAPID measurements.

Lines 166-167. I am wondering if the western boundary density anomaly could also be replaced with monthly climatology as in the eastern boundary.

Line 174. That is not the typical definition of the standard deviation.

Lines 188-191. Would it be possible to assess the cross correlation with a wavelet transform to add information to the coherence? That way we could assess the change in power spectra for each frequency through the years. If authors think that this study is going to take too long, please, do not do it. It is only a suggestion.

Figure 5 (right panels). The phase or lag in degrees does not provide very useful information. It could be expressed in time (days), so that we could estimate the time lag between each signal. Moreover, the dashed line of “out of phase” at 180° may induce

to errors in the reader, as any signals are out of phase once the phase is different from zero.

Line 193. I think it should be written: ‘. . . shows significant coherence with the observed UMO transport at periods of . . .’

Line 197. What is the consistency between a 180° coherence phase and a negative coefficient in the model regression? Relate to the peak/valleys in each signal

Line 200. Please, explain the negative phase. Previously, signal A was anticipated to signal B (positive phase), while now signal A is delayed with respect to signal B (negative phase). Make sure to define which are signals A and B, so that we may understand if the UMO transport or the boundary densities are lagged with respect to each other.

Lines 203-204. What is the relation between a 90° phase coherence and a weakening of the southward UMO transport?

Line 229. “Losing a little of the explained UMO transport variance”. Would there be any way to assess the contribution of the seasonal component to the UMO variability?

Minor comments:

Line 23. (IPCC) says

Line 154. Change doesn’t to does not.

Line 183. The Florida Current data have a gap.

Lines 212-214. Missing the verb in the sentence.

Line 241. If there are 5 CTD profiles within the group, with distances from the slope at 740 dbar of

Lines 244-245. those selected for use in the model, show that the majority were

Line 247. Transatlantic sections at 24.5°N (consistent with previous sections).

Line 250. The uncertainty for the 1957 section model estimate was much larger

Lines 252-255. Could be separated into two sentences.

Line 273. Badly worded.

Line 284. Almost as weak as

Line 285: December 2010 and March 2013, respectively.

Line 295: The Gaussian-weighted four-year rolling mean also suggests that

Line 297: It also suggests

Line 302: suggests a non-monotonic weakening trend

Line 305: maximum

Lines 306-307: RAPID mean southward values for 2004-2008 and 2008-2012 are stronger than the model by 0.9 and 0.3 Sv respectively, but for 2012-2016 it is 1.8 Sv weaker.

Lines 320-321: How low are the targeted low frequencies? The model time series only allows for decadal changes ($\sim$30 years of model).

Line 326: Using four-year means, how can you observe multi-year variability?

Lines 175-179, 206, ... I am not sure why the authors start to use the past tense (calculated, used, suggested, ...)

References

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