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Interactive comment on "Pending recovery in the strength of the meridional overturning circulation at 26° N" *by* Ben I. Moat et al.

Ben I. Moat et al.

ben.moat@noc.ac.uk

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Pending recovery in the strength of the meridional overturning circulation at 26°N Ben. I. Moat, David. A. Smeed, Eleanor Frajka-Williams, Damien G. Desbruyères, Claudie Beaulieu, William E. Johns, Darren Rayner, Alejandra Sanchez-Franks, Molly O. Baringer, Denis Volkov, Laura C. Jackson, Harry L. Bryden

We thank the reviewers for their time in commenting on this paper. We have prepared a detailed response to reviewers #1 and #2.

Reviewer 1 notes that we are relying on the expectations that (a) AMOC transport will increase as a result of strong buoyancy forcing in the subpolar North Atlantic and (b) that there is some relationship between the AMOC in the subpolar and subtropical

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Discussion paper

gyres. These are indeed assumptions that we are working with as they are the prevailing view of the AMOC circulation variability on long timescales. On shorter timescales, the transport variability is confounded by higher frequency/shorter period fluctuations that are wind driven. This short timescale variability presents significant challenges in identifying meridional connectivity, particularly when time series are themselves short.

We are further relying on the assumption that meridional coherence of the circulation, if it exists, will appear in transport fluctuations (e.g., Zhang 2010, Bingham and Hughes, 2009) rather than in watermass advection (e.g., Zou et al. 2016). The arrival of watermass signatures, while easier to identify in longer hydrographic records, is a complicated integral of the transport variability along spreading paths, and represents a complementary measure of ocean circulation change.

We have attempted to identify transport covariance between the best available AMOC observations at 26N, and the longest available estimates at 45N. To extend the time-series at 26N, we have used the GloSea5 reanalysis which was shown to capture the interannual variability of the RAPID array at 26N albeit with reduced amplitude (Jackson et al. 2019). Using these records, we still cannot conclude a definitive lead-lag relationship between two latitudes. However, we anticipate that the strong subpolar cooling in 2013-2015 may provide an impulse response that will generate a signal in meridional connectivity above the background high frequency 'noise'.

The GloSea5 time series has been extended to the end of 2018 by Laura Jackson at the UK Met Office, and is now included in this paper (red line Figure 6a). Laura has also contributed to the analysis in the updated paper so we have included her as a co-author.

Anonymous referee #1

Detailed responses to particular points follow: 1) If a strengthening of the subpolar water mass transformation leads an increasing AMOC at 45N by 5-6 years (line 193 in this manuscript). Then why did the AMOC at 45N already begin to increase around

2011 (Figure 6a)?

Desbruyeres et al. (2019) and the AMOC time series at 45N show an increase from a relative minimum in 2010. Similarly, the watermass transformation shows an increase from a relative minimum in 2005 (5 years earlier). However, the watermass transformation due to oceanic heat loss was not significantly greater than zero until 2010. We have updated the text in the manuscript to read:

“These localised deep convection events are part of wider and longer-term intensification in subpolar water mass transformation that was at a minimum in 2005”

2) Recent Lagrangian studies, however, show much longer time scales (> 10 years) for those dense waters to be exported to the subtropics (e.g., Jackson et al. 2016; Zou et al. 2016). A more comprehensive discussion will be needed to reconcile those different perspectives on how the subpolar water mass transformation may impact overturning variability.

Lagrangian approaches identify advective pathways between the subpolar and subtropical regions in the Atlantic, but are not ideal to capture faster boundary wave-mediated changes in transport. Indeed, Zou et al (2016) comments on this issue after finding that the Lagrangian approach did not show any relationship between watermass formation and transport variability. To get around this, they used e-floats on either side of key latitudes to match the transport anomaly signatures, finding that it propagated much more quickly than water parcels did (2 year time lag).

We have added text to emphasize that with our transport time series we are looking at how anomalies in transport propagate meridionally, rather than how water parcels propagate meridionally. “Lagrangian studies have been used to identify when newly formed dense waters from the subpolar gyre reach the subtropics, with anomalies moving with the currents via advection (e.g., Bower et al., 2009; Zou et al., 2016; Jackson et al., 2015). However, transport time series can also adjust more rapidly through a fast boundary-wave mediated response of lower latitude AMOC variability to high latitudes forcing. Such a response can potentially be identified by lag correlation or coherence analysis of AMOC transport time series, rather than hydrographic anomalies. Based on the increase in subpolar watermass transformation peaking in 2013-2015 and various time lags between the subpolar-to-subtropical AMOC strength determined from numerical simulations, we would anticipate a sign of the increasing subtropical AMOC by 2018-2022."

3) The authors then suggested that a larger AMOC at 45N leads a larger AMOC at 26N by 0-2 years. But using the same Glosea5, Jackson et al. (2016) suggested that the AMOC anomalies at 45N precedes 26N by about 10 years. How to reconcile such a significant discrepancy? Is it related to the use of the observed AMOC at 45N and the modeled AMOC at 26N?

We agree that this is an inconsistency that cannot yet be reconciled from the observations. Given the short duration of the records available and the conflicting time lags identified within GloSea (26N to 45N) itself and between GloSea5 26N and observational estimates at 45N, we are removing the '0-2 year' lag estimate and replacing it with, 'consistent with a possible 0-2 year lag'.

"With the relatively short duration records and the absence of a clear impulse anomaly to track between latitudes, it is not yet possible to identify the timescale of adjustment between the subpolar and subtropical AMOC strength. It appears, however, from comparing the 45°N observational estimate of the AMOC and 26°N from Glosea5, that the adjustment timescale may be short (0-2 years). In contrast, within the GloSea5 reanalysis itself there was a mean lag of 7 years between a peak in Labrador Sea density and the AMOC at 26°N (Jackson et al., 2015). This discrepancy is difficult to reconcile. While GloSea5 has been validated against the 26°N observations, there does not exist an equivalent long AMOC record in the subpolar gyre to verify GloSea5: the OSNAP estimate of the AMOC is too short (21 months) to verify interannual variability of reanalyses (Lozier et al., 2019) and the method used at 45°N with altimetry and gridded hydrography may be subject to errors particular in resolving higher frequency anomalies at the boundary. "

4) In addition, the authors cited Zou et al. (2019) on the connection between the subpolar UNADW and subtropical LNADW transport anomalies. Note that Zou et al. (2019) suggested that such a latitudinal AMOC connection can be due to gyre-dependent forcing; only strong LNADW transport anomalies can propagate southward from the subpolar region to the subtropics in 4 years. A discussion on this and a reconciliation with the presented analysis are currently missing in the manuscript.

The analysis in Zou et al (2019) relies on a single large anomaly during a relative short model run (1991-2004). They find that for this single large anomaly, that a UNADW transport anomaly in the subpolar gyre leads to a subtropical LNADW anomaly 4 years later. This timescale is, however, inconclusive even within their paper where they have 3 different analysis with three different timescales. We don't believe there is anything substantially new to reconcile with our analysis, as we do not separate 45N into layers, and our AMOC anomalies at 26N are (as they are in Zou et al. 2019, and previous RAPID papers) due to anomalies in LNADW. We have therefore removed this reference to Zou et al. 2019.

5) AMOC-AMV-subpolar OHC relationships. It is a bit confusing about the relationships of AMOC-AMV-subpolar OHC. The authors first suggested that the AMOC lead the AMV byâĹij5 years as shown in a high-resolution model (Moat et al. 2019), but then pointed out the AMOC maximum at 45N precedes the AMV byâĹij10 years in Glosea5 (lines 243-244 in this manuscript). Does it imply that the AMOC-AMV relationship is just model-specific? Moat et al., (2019) shows that in a high resolution model the AMOC leads the AMV by ∼5 years at 26N and ∼9 years at 50N (Moat et al. 2019, figure 3a), which broadly agrees with the reviewers comment above. Although Moat et al., (2019) found these correlations to be significant at the 95% level, they do not account for all the AMV variability (Rˆ2 = 0.33) and other processes could contribute to the variability independent of the AMOC, e.g. Atmospheric teleconnections from the tropics, and variability of the Arctic sea ice and snow cover. From this study the

AMOC leading does seem to be robust. Given the short length of the time series in observations we cannot yet be sure about the absolute lag between the AMOC at 45N and 26N. Here we are presenting the broad scale response of the North Atlantic to changes in the AMOC at 26N.

In addition, how does the AMOC-AMV relationship relate to the subpolar OHC changes? A paper on the full heat budget of the North Atlantic is currently being written by the authors, so we have removed the discussion on the ocean heat content changes from this manuscript.

6) The authors suggested a relationship between the weakened heat transport in the sub-tropics (i.e., in relation to a weak AMOC state) and the cooling subpolar gyre during 2013-2015. Should it be focused on the heat transport at 45N that is at the southern boundary of the subpolar gyre?

45N is spanning the subpolar gyre and intergyre-gyre region, so there is no clear break between the subtropical and subpolar gyres. While 26N is near the middle rather than the north of the subtropical gyre (Fig 1), it is expected that AMOC fluctuations in the subtropical gyre are coherent, so that the heat transport through the middle of the subtropical gyre is proportional to the heat transport through the northern edge of the subtropical gyre (Zhang 2010, Bingham & Hughes 2009).

Another manuscript is in preparation to do a detailed heat budget for the North Atlantic. We are therefore reducing references to the heat transport variability, including removing the OHC time series in Fig 6b.

7) The AMOC at 45N appears to be strengthening after 2011 (Figure 6a), indicating an increasing northward heat transport during the cooling period. How to exclude the impact from the strengthened atmospheric forcing during 2013-2015 (e.g., de Jong and de Steur 2016)? My suggestion is to add a timeseries of the surface heat flux over the subpolar region during the overlapping period of 1985-2018 and discuss accordingly their potential impact on the oceanic changes. Otherwise, in my opinion, it is hard to

draw any conclusions on how the AMOC changes lead the changes in the subpolar OHC.

Desbruyeres et al. (2019) discuss the heat budget in the North Atlantic. They use the time-accumulated MHT relative to a reference period from 1996-2013 and determine that the OHC anomaly is initially entirely explained by MHT, and then (during the development of the cold blob) is not. The time-accumulated quantity is, however, sensitive to the choice of reference period; using a different reference period (1993-2017) results in a change in slope of the time-accumulated quantity (integral of a constant with time is a trend). As we are presently involved in another, more detailed, heat budget analysis, we don't believe we can add significantly to what Debruyeres et al. (2019) already showed.

Other Comments: 8) Lines 162-163: To utilize the lengthy record, the authors could put error bars on the monthly values and comment on how robust the seasonal cycles are.

This has been done. we have updated figure 3 and added the following to the text: "There is a substantial seasonal cycle with an amplitude of 2.0$\pm$0.16 Sv and 0.7$\pm$0.16 Sv (mean and standard deviation from Monte Carlo estimation) for the annual and semi-annual harmonic, explaining 11% and 2% of the variance, respectively. The residual timeseries, likewise, retains substantial variability with a range of 21.6 Sv and a standard deviation of 3.4 Sv. About 20% of the residual variance is associated with the estimated error of $\pm$1.5 Sv for the 10-day binned data."

9) Lines 180-181: Need more information on Figure 5. How to understand the different change points defined by Mean+CP and Trend+CP? Mean+CP shows an earlier change point around 2008. Also, it is not clear from Figure 5 why Mean+AR(1)+CP is the overall best fit. Please add more details on how this was determined. More details explaining the methodology and how the best model is selected have been added in Section 3.2: "For the models with changepoints, we find the number and locations us-

ing the pruned exact linear time algorithm (Killick et al., 2012), which performs an exact search considering all options for any possible number of changepoints and select the optimal number/location balancing the overall fit against the length of each segment. The most appropriate model is selected according to the Akaike Information Criteria (AIC). The AIC differences between each model included in the comparison and the model with the smallest AIC are also computed to assess plausibility of all models. As a rule of thumb, a difference larger than 10 indicates that there is essentially no support for a model given the data and the other models at play (Beaulieu & Killick, 2018). To verify sensitivity to the choice of information criterion, the Bayesian Information Criterion for each model is also computed."

Given that the AIC differences between each model and the one with the smallest AIC are all large (>10), we can conclude that no other model amongst those compared fit the data reasonably well.

10) Line 184: The standard deviation clearly varies with the time scales over which it is derived. I would suggest the authors show the standard error in the mean instead, which seems to be more helpful when determining how distinct the time-mean transports are between two years or any two periods. We have quoted the standard error in the text. The values in Table 1 have been left as the standard deviation as this is more relevant to the AMOC variability, but we have added the de-correlation time scales into the Table 1 caption to enable the standard error to be calculated if required.

11) Line 189: Is section 4.2 just about the relationship to 45N? If so, better to be more specific. Title of section 4.2 has been changed to 'AMOC relationship between 26°N and 45°N'

12) Lines 190-203: Please see my main concern point 1). This has been addressed above in point 1).

13) Line 208: Is the timing of the AMOC increase at 45N (2010-2011) sensitive to the size of the filter? The timing should not be sensitive to the filtering. Desbruyeres

et al. (2019) made a quick test on how their comparison AMOC was influenced by the filtering and concluded that: Lowpass filtered time series presented throughout the paper use a 7-year Hanning window and endpoints are therefore truncated at ± 3 years. The impact of low-pass filtering AMOC and SFOC time series on the lagged auto-correlations were studied by varying the size of the filtering window (0, 3, 5, 7, 9 and 11 years). While the raw annual time series show small correlations at all lags ($R < 0.4$), maximum correlations for smoothing windows of 3 years and above were reached at a consistent lag of 5-6 years.

14) Lines 213-214: Is the difference in the variability the same between the AMOC at 45N and 26N both in Glosea5? Like the observations the AMOC- Ekman using GloSea5 at 45N (in density space) does have higher variability than Glosea5 at 26N, but Glosea5 at 45N does have slightly less variability than observation at 45N. The standard deviation of the AMOC- Ekman in GlosSea5 at 45N is 1.02 Sv and at 26N is 0.63 Sv. The standard deviation of the observations is 1.58 Sv (45N) and 0.77 Sv (26N). As we do not make reference to the GloSea5 time series at 45N we have removed the line, "With these two time series, the variability in the GloSea5 estimate of AMOC-Ekman at 26°N is more markedly lower than at 45°N."

15) Line 238: The authors appear to emphasize a 5-year time lead by the AMOC. But I couldn't find any observational evidence even in this analysis for such a time lead. The 5 year time lead is from a coupled climate study by Moat et al. (2019), calculated using fields over a 300 year period. Given the short length of the high quality time series at RAPID 26N (2004 to 2018) is it hard to directly show this lag between AMOC and AMV. In this paper (figure 6) using the GloSea5 reanalysis we show that AMOC leading AMV is robust, but there is a bit of variation in the precise lags.

We have rewritten Section 4.3 to make the description of the lead lag relationship between the AMOC, AMV and NAO clearer.

16) Lines 253-255: Please see my main concern #2. This has been addressed above

in point 2) and 15) and in the text.

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