

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

**Anonymous Referee #1**

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Re: “Pending recovery in the strength of the meridional overturning circulation at 26N” by Moat et al.

The authors reviewed the AMOC variability at 26N based on the extended time series from the RAPID-MOCHA array and analyzed concurrent multi-year changes in the North Atlantic. The authors also explored the potential of an intensification of the AMOC at 26N in response to the 2013-2015 strong cooling event over the entire subpolar gyre. The central questions the authors are trying to address are very interesting and critical. But I have several concerns which are outlined below. Mainly, the manuscript presents some speculations of the AMOC changes and their broader impact that seem to be exclusively based on modeled lead-lag relationships between key physical progress derived. How robust those relationships are and over what time

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scales are still open questions and can vary considerably across models. And a discussion on how that may affect the analysis is missing. My overall recommendation is that the authors include more details on how those relationships are potentially achieved (AMOC at 26N and 45N, and AMOC-AMV-subplarOHC, etc.) and discuss thoroughly the limitations of modeled relationships and how they might affect the interpretation of the observed changes.

Main concerns:

(1). Subpolar water-mass transformation and AMOC connectivity between 45N and 26N.

The anticipated intensification of the AMOC at 26N appears to be relying strongly on (a) an ongoing AMOC intensification at 45N following the basin-wide strong cooling in the subpolar region, and (b) a subpolar-subtropical AMOC connectivity. However, I found the evidence for those two conditions (or assumptions) to be not sufficient if not inadequate.

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)? Also, of note is that Desbruyères et al. (2019) assumed an immediate export of the newly formed dense waters without any accumulations of water volumes. 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.

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

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modeled AMOC at 26N? It isn't clear why the authors used GloSea5 other than any other models/reanalysis. Some validations of GloSea5 will be needed, in particular, on how well GloSea5 reproduces the AMOC variability and connectivity. 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.

(2). 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 by  $\sim 4$  years as shown in a high-resolution model (Moat et al. 2019), but then pointed out the AMOC maximum at 45N precedes the AMV by  $\sim 10$  years in GloSea5 (lines 243-244 in this manuscript). Does it imply that the AMOC-AMV relationship is just model-specific? In addition, how does the AMOC-AMV relationship relate to the subpolar OHC changes?

The authors suggested a relationship between the weakened heat transport in the subtropics (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? 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 time series 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.

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Other comments:

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.

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.

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.

Line 189: Is section 4.2 just about the relationship to 45N? If so, better to be more specific.

Lines 190-203: Please see my main concern #1.

Line 208: Is the timing of the AMOC increase at 45N (2010-2011) sensitive to the size of the filter?

Lines 213-214: Is the difference in the variability the same between the AMOC at 45N and 26N both in GloSea5?

Line 238: The authors appear to emphasize a 4-year time lead by the AMOC. But I couldn't find any observational evidence even in this analysis for such a time lead.

Lines 253-255: Please see my main concern #2.

Additional references:

de Jong, M. F., and L. de Steur, 2016, Strong winter cooling over the Irminger Sea in winter 2014–2015, exceptional deep convection, and the emergence of anomalously

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low SST, *Geophys. Res. Lett.*, 43, doi:10.1002/2016GL069596.

Zou, S., and M.S. Lozier, 2016, Breaking the Linkage Between Labrador Sea Water Production and Its Advective Export to the Subtropical Gyre. *J. Phys. Oceanogr.*, 46, 2169–2182, <https://doi.org/10.1175/JPO-D-15-0210.1>

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