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Ocean Science
Discussions

Interactive comment on "Surface predictor of overturning circulation and heat content change in the subpolar North Atlantic" *by* Damien G. Desbruyères et al.

Anonymous Referee #1

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In this paper the authors use observationally based products to estimate the Atlantic Meridional Overturning Circulation at 45N, and to relate the variability to changes surface forced density changes. They show that based on the observational evidence the AMOC was at a maximum 45N in the mid-1990s, before declining to ∼2010. This variability was led by changes in the surface forced density changes and transformations, which the authors show leads the AMOC by ∼5 years. They then use this 5 year lead time to make a crude prediction of the AMOC and its impacts, arguing that recent intense cooling of the North Atlantic will lead to an increase in the AMOC, and a subsequent warming of the Subpolar North Atlantic.

This is a nicely written and presented short paper on a relevant and interesting subject. The results, and especially by putting the changes in a prediction framework, would ensure that this paper was of interest to a wide community of scientists. Therefore, I believe this paper is certainly appropriate for publication in Ocean Sciences. However, I do have a number of points that I think the authors should address before acceptance.

Major points There is substantial uncertainty in the observational products, which I think has not been adequately addressed, at least in the submitted paper. In particular, the authors have done a good job in bringing different datasets together, but have not, at least to this reviewer, provided all the relevant evaluation of those datasets. For example, the main results of the paper focus on variability in AMOC and SFOC, but, only show the uncertainty in the long-term mean of AMOC_sigma. Grist et al, 2014, showed that there is considerable uncertainty in the SFOC from different atmospheric data sets, which would not be well represented by assuming gaussian uncertainty - However, this is not addressed here. It's also not entirely clear whether the Authors have computed the time series for these quantities each dataset separately and taken the mean, or combined the data first?

Furthermore, I wasn't sure about the use of climatological salinity in the computation of SFOC. Its well know that salinity and temperature changes often compensate in anomalies of density - does this lead to important inaccuracies in your method of computing SFOC?

I would expect to see in a revised manuscript

- Some estimate of the uncertainty in the location and amount of SFOC - i.e. figure 3 - in simple terms how different does the spatial pattern and the resulting timemean SFOC stream function look

- A representation of the uncertainty in the variability of the AMOC_sigma and SFOC - i.e. figure 4

I'd also like the authors to elaborate on the impact of assuming climatological salinity, including why they have done it. Does figure 3 or 4 change substantially when they include changes in S?

I will leave it up to the authors about where to include the results of this further analysis in the manuscript (e.g. in the main paper, or in the suplementary).

Minor Points Line 60 - skill not skills

Section 2.1 - it is not entirely clear why the calculation is only done for the period 1993-2017. I assume this is because of the use of AVISO data (which starts in 1993) but the table S1 says that EN4 data was used from 1985 onwards - could you clear this up?

L93 - 'This error captures the incompressible spread between all possible methods used as of today to interpolate sparse in situ observations' - I'm not sure I totally understand the point being made - what is incompressible spread?

L109 - clarify the difference between MAX(AMOC_sigma) and AMOC_sigma_m

L136 - it is not clear where Temperature is used in the equation for SFOC - do you mean for the calculation of isopycnals (sigma)? L162 - Why partial AMOC?

Figure 3 - I was quite surprised to see that so much of the SFOC was generated in the eastern SPG, and very little in the west, and particularly in the Irminger and Labrador basins. How sensitive to recent extreme winters is this picture (i.e. 2014, Josey et al, 2018) and how important is the climatological S? Is there any insitu observational constraints for this region other than the results of Lozier et al, 2019? Also is the time-mean the 1993-2017 time mean?

L192 - why would there be a 8 year time-scale?

Figure 4 - what is the grey bars in panel A?

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