The authors of the manuscript gratefully acknowledge the anonymous reviewers for their positive feedbacks and their relevant and constructive comments on our manuscript. Our replies to those comments and the associated changes made to the manuscript are provided below.

Answers to Referee #1

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. It is well known 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

This is now shown as supplementary Figure S3. The spatial patterns of the three individual SFOC transformation across σ_0 = 27.4 are very similar (despite slightly higher values for CERES/FMASS). The quantification of the uncertainty (due to product spread) was made in

Figure 3B with the shaded areas representing the standard errors computed as the standard deviation divided by the square root of N-1, with N = the number of products used in the mean (such an uncertainty also appears on the time series in Figure 4), as stated in line 90-96.

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

The authors are not too sure to understand the request here. As for the time-mean stream function (Figure 3), the uncertainty around the time series are already included (shaded patterns). The quantities (AMOC, SFOC, OHC, …) were computed for each dataset separately, before taking an ensemble mean and computing the ensemble standard errors. Therefore, shaded envelopes in Figure 3/4/5 represent the spread (standard errors) of values between products. This is described in lines 90-93 as:

"The various integrated quantities derived from those data products (such as ocean heat content of overturning stream functions - see description below in Section 2.2 and 2.3) were then combined into ensemble mean over the period (1993-2017 for altimetry-related quantities, 1985-2017 otherwise), with associated ensemble standard errors computed as $\frac{\sigma}{\sqrt{N-1}}$, where σ is the standard deviation and N = 4 the number of data products used in the mean."

The shaded patterns in Figure 4 were probably too light in the previous version of the draft, and have now been reinforced. Also, the information was missing from the Figure 4 legend and has now been included as "Shading indicates the ensemble standard errors for each variable".

- 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?

Using interannual surface salinity has very limited repercussion on the long-term time-mean SFOC stream functions shown in Figure 3 and for most of the individual yearly estimates in the time series of Figure 4. The primary reason for using a climatological SSS field is the potential spurious signal introduced by poor salinity sampling in some years, especially before the WOCE/Argo era (1985-1990). This is particularly true near continental margins and seasonally-ice covered regions, where too sparse salinity sampling in some years can reverberate on density estimates and on the definition of isopycnal outcrops within which air-sea buoyancy fluxes are integrated. We attach to this comment a figure comparing SFOC timeseries obtained from either climatological or interannual SSS. Very good consistency is found between both estimates overall, although a few years show non-negligible differences, most particularly in the early part of the record (1985-1990). As said above, we associate those discrepancy to spurious anomalies in the historical SSS record. Moreover, surface density in the upper layer of the ESPG (where the maximum SFOC takes place) is in any case largely controlled by temperature changes and SFOC is almost exclusively driven by surface heat fluxes. For those reasons, as well for being consistent with previous published methodology (Marsh et al 2000), the seasonal SSS fields are used herein. More details have been included in line 145-147 to account for this choice:

"[…] to avoid introducing punctual spurious surface density anomalies due to poor salinity sampling (especially in the early historical record), notably near continental margin and seasonally-covered ice-covered areas. We note here that the air-sea buoyancy flux in the SPG, and therefore $SFOC_\sigma$, is largely controlled by its thermal component (Marsh, 2000)."

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 supplementary).

Minor Points

Line 60 - skill not skills

Done

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?

Indeed, the computation of AMOC depends on the altimetry record, and therefore starts in 1993. However, the computations of SFOC only depends on analysis/reanalysis products and can go further back in time. This notably enables to evidence and represent the 5-year lag relationship between AMOC and SFOC. This is now clarified in the manuscript at line 92 as:

"combined into ensemble mean over the period (1993-2017 for altimetry-related quantities, 1985-2017 otherwise)"

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?

This sentence has been modified as "This error captures the spread induced by the different methods used as of today to interpolate sparse in situ observations" (l. 94)

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

The authors thank the referee for his remark. There was an inconsistency between the two equations, which has now been corrected.

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?

Yes, temperature is used to compute surface density. This is now clarified in the text.

We characterize the AMOC stream function as "partial" as it is calculated from 0-2000m velocity fields, as stated and justified in line 113-115.

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?

Yes, the time-mean was taken over 1993-2017 and this is now stated in the legend. The finding that basin-wide subpolar AMOC (mean and variability) is dominated by transformation in the eastern SPG basins, with relatively minor contribution from the Labrador Sea, is indeed consistent with the most recent results from the OSNAP array. In fact, this results was already known before hand from independent observation-based (repeat hydrography and velocity measurements) estimations of the AMOC in the eastern SPG (Lherminier et al., 2010; Mercier et al., 2015; Sarafanov et al., 2012). Note, however, that this east-versus-west contribution to the transformation depends on the isopycnal that is being considered. Here, the isopycnal of maximum overturning is used (σ_0 = 27.4) and it is therefore consistent to see that the bulk of the transformation is occurring where the northward-flowing North Atlantic Current loses much of its heat to the atmosphere. If a similar map was plotted for a denser isopycnal (e.g. σ_0 = 27.74), however, the pattern would show strongest transformation in the Labrador Sea associated with the formation of Labrador Sea Water, which is the "end product" of the full transformation process in the SPG. In other words, the density level of maximum transformation in the Labrador Sea is well below the density level of maximum transformation for the whole zonal extent of the North Atlantic, and the transformation across those distinct density levels have distinct spatial patterns (western-intensified and eastern-intensified, respectively). Note that an additional sentence has been added in the manuscript to emphasize this point (l. 185-189):

"This pattern is consistent with recent mooring-based analysis of the diapycnal overturning in the SPG showing a relatively minor contribution of the Labrador Sea to the basin-wide maximum transformation rates (Lozier et al., 2019). This is because the density level of maximum transformation in the Labrador Sea is well below the density level of the basin-wide AMOC$_\sigma$ (or SFOC$_\sigma$)."

L192 - why would there be a 8 year time-scale?
This very interesting question is far from being an easy one. If one assumes that SFOC is largely "forced", then the dominant large-scale atmospheric regime (the NAO for instance), or a combination of them, must ultimately contains this 8-year time scale. But the picture is probably more complicated, involving complex retroactive loops between the atmosphere, preconditioning and transformation, the AMOC, etc. In any case, we have added the word « apparent » in the text, to be more prudent regarding the statistical significance of this "8-year signal" given the shortness of the time series considered here (l. 208).

figure 4 - what is the grey bars in panel A?
The grey bar was the NAO index. It has been removed from the figure as it is not discussed in the manuscript.

Answers to Referee #2

In this paper, Desbruyeres et al. find a lagged correlation between surface buoyancy flux over the subpolar North Atlantic and the strength of the AMOC at 45N, with the surface forcing leading by about 5 years. They also find that low-frequency ocean heat content variability in the subpolar region similarly follows the surface forcing by several years since for example, an increase in AMOC strength brings on an increase in meridional heat transport into the subpolar region. An exception to these trends seems to have occurred in 2014 and 2015, when extreme air-sea heat flux overwhelmed a warming trend due to increasing MHT, and the subpolar heat content temporarily decreased. Based on the observed correlations over the past couple of decades, the authors predict continued AMOC strengthening through at least 2022, with a surge in ocean heat content and upper ocean temperatures in 2019 and 2020. Another interesting result is that most of the decadal variability in AMOC is driven by surface forcing variability over the subpolar region, not over the Nordic Seas.

To estimate the observations-based AMOC, MHT and OHC, the authors used an ensemble of four hydrographic data bases (common depth coverage 0-2000m) to first compute absolute geostrophic velocity profiles at 45N from thermal wind, referenced with altimetry-derived sea surface velocities. The light-to-dense water mass transformation was estimated using the theoretical concept developed by Walin (1982), using the methodology of Marsh (2000). Three atmospheric re-analysis products were combined to compute the time series of the surface-forced transformation rate. The manuscript is well-written and organized. The data and methods are described in detail in the supplemental material. The discussion is straight-forward to follow, and the conclusions are important and will be of interest to those studying the AMOC and low-frequency (decadal) ocean-atmosphere climate variability of the northern North Atlantic.

In my opinion, the paper can be accepted for publication more or less in its present form, although I have two questions/comments for the authors to consider. First, the authors do not explain why they chose to estimate the AMOC at 45N. Why not 50N? or 40N? Would the correlations between the surface forcing and the AMOC have been better or worse if a different latitude were chosen? If the best correlations are achieved at 45N, why is that so?

The reason for choosing 45°N as a reference latitude for the calculation of AMOC is three-fold. First, this particular latitude represents the approximate southern (geographic) boundary of the subpolar domain so that the bulk of the light-to-dense transformation of NADW is fully captured north of 45°N. In other words, the surface-forced stream function SFOC reaches a plateau near 45°N, and no further transformation (for the NADW density range) occurs south of it. This latitude is also very often used in model-based estimation of the AMOC, so the present choice permits easy comparisons with those studies (e.g. Jackson et al 2016, Nature Geoscience). Finally, data distribution is also key for the computation of AMOC. The near-45°N latitudinal band is a relatively well-sampled region and, as a result, lateral density gradients are relatively well defined in the ocean analysis products enabling consistent circulation estimates. This is not the case everywhere. At other latitudes (e.g. 50°N), the data entering the interpolation scheme may be sparser and sometimes insufficient to represent correctly the local dynamics and its variability, and hence lesser correlation with SFOC would result. Those points are not explicitly stated in the manuscript (l. 101-106).

Second, in the last sentence of the second-to-last paragraph of the introduction, the authors state, "…with details on the capability of the *in situ* OSNAP array in monitoring the basin-wide AMOC." I didn't see in the discussion or conclusion sections where the capability of the OSNAP array to monitor the basin-wide AMOC was assessed, at least not explicitly. Maybe the authors could either be more explicit about this assessment, or drop the phrase in the introduction indicating that they will make this assessment.

We agree that the capability of the OSNAP line to capture the bulk of SPG transformation was only described in the Result section (l. 184-190 and l. 236-243, Figure 3 and Figure 4). We have now added a summary statement in the discussion/conclusion as the reviewer suggests (l.300).