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. The shaded patterns in Figure 4 were probably too light in the previous version of the draft, and have now been reinforced.

- 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.

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.

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”

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).

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.