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).