Ocean Science Discussions

Ocean Sci. Discuss., author comment AC3
https://doi.org/10.5194/os-2021-48-AC3, 2021

Reply on RC3

Tillys Petit et al.

Author comment on "Role of air–sea fluxes and ocean surface density in the production of deep waters in the eastern subpolar gyre of the North Atlantic" by Tillys Petit et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-48-AC3, 2021

We thank Referee RC3 for his/her insightful review that have helped us improve the manuscript.

Review of the manuscript "Role of air-sea fluxes and ocean surface density on the production of deep waters in the eastern subpolar gyre of the North Atlantic" by Petit et al.

This is a concise piece of work on the important aspect of large-scale water mass transformation. However, I find the apparent implications to AMOC and climate weak. Also, I have issues with some of the analyses and related statements. I thus find the manuscript in need of a major revision. I hope the authors find my comments useful in doing so.

GENERAL COMMENTS

AMOC/AMOC-link: inference/assertions related to AMOC are relative prominent in the MS, particularly in the abstract and conclusion. But any direct link, at least explicitly - and particularly related to variability, is absent from the study. Actual AMOC (co-)variability must be added or the AMOC implications must be much less prominent and anyway presented as no more causal than the study warrants. What is the relation of, e.g., Fig 6a, to a relevant AMOC time series?

The study analyzes the factors that drive variability in the production of Subpolar Mode Water (SPMW) in the Iceland Basin (Figure 6a). Past studies have demonstrated that SPMW is a major contributor to the total waters carried within the lower AMOC limb (Chafik & Rossby, 2019; Lozier et al., 2019; Petit et al., 2020) and that its formation pre-conditions the formation of dense water further downstream (de Boisséson et al., 2012; Brambilla et al., 2008). In this paper, we reveal the key role of the surface density field in the production of SPMW. This understanding of the mechanism that drives the variability of SMPW will aid predictions of AMOC variability in the years and decades ahead. For these reasons, the link between SPMW transformation and AMOC is discussed in the manuscript.

While we believe that the link between AMOC and SPMW transformation is sufficiently and clearly discussed in the conclusion, we have modified the abstract for clarity. Lines 16-17 now read: “We analyze these contributions to the transformation in order to better understand the connection between atmospheric forcing and the densification of surface water.”

strength of (co)relations: there is a use of qualitative statements, e.g., "weak" (l.178), "strong" (l.206), "weakly dependent" (l.323) that are unjustified by the specific numbers presented. (r=0.52 is quite substantial in real interannual data; if the former is "weak", r=0.67 is not "strong"). Relatedly, there is no reference, not to say quantification, of significance, confidence levels nor confidence intervals

We now indicate the significance of each correlation throughout the manuscript by adding the associated p-values:

l.192: “All correlations have p-values < 0.05.”

l.207: “(0.66 and 0.63, respectively, with p-values < 0.05)”).

The interpretation of those correlations is also clarified in the text, as indicated below.

Figures: several of the panels are a little too stamp-sized as they appear; some cumbersome units/crowded axes (e.g., 2c

Panels in Figures 4 and 5 are now larger and the color bar labels are bigger in all the figures. The axes in Figure 2(c) have been detailed for a clear comparison with Figure 2(d).

papers to add?

From the top of my head, here's a few papers (non-exhaustive list) the authors could benefit from relating to in revising their manuscript:

Johnson et al (2019). Recent contributions of theory to our understanding of the Atlantic Meridional Overturning Circulation. Journal of Geophysical Research: Oceans 12 (8), 5376–5399.

Langehaug et al (2012) Water mass transformation and the North Atlantic Current in three multi-century climate model simulations. J. Geophys. Res., 117, C11001

We now refer to Langehaug et al (2012) in the introduction and to Johnson et al (2019) in the conclusion. We also cite Desbruyere et al. (2019) in section 2.

Desbruyères, D. G., Mercier, H., Maze, G., & Daniault, N. (2019). Surface predictor of overturning circulation and heat content change in the subpolar North Atlantic. Ocean Science, 15(3), 809–817. https://doi.org/10.5194/os-15-809-2019

SPECIFIC COMMENTS

l.10: "convection", is this the specific term you want to use? First, this seems the only place its used in the MS, and, second, at least to this reviewer, it gives the association to outdated an understanding of AMOC etc via specific convective sites that

The term "wintertime convection" refers here to the process of light water densification that forms the lower limb of the MOC at high latitudes, not to specific convective sites.

l.22–23, "... is set by ... advection, wind-driven upwelling...": neither are addressed herein?

We clarified the sentence that now reads:

"This surface density is set by a combination of advection, wind-driven upwelling and surface fluxes. Our study shows that the latter explains ~30% of the variance in outcrop area as expressed by the surface area between the outcropped SPMW isopycnals."

l.31–34, interannual variance in AMOC rooted in subpolar NA rather than in the Nordic Seas: What is the evidence/refs for this. What meassure of dense water production and AMOC does the statement relate to?

The statement relates to measures of overflow transports along the Greenland-Scotland Ridge from Bringedal et al (2018), and to their comparison with the AMOC variability at OSNAP East by Petit et al. (2020). The references have been modified accordingly.

l.39: awkward combination of range(?) and uncertainty/interannual(?) variance

The average overturning has been shown by Petit et al. (2020) and now reads "6.6 ± 3.8 –7.6 ± 3.8 Sv" in the manuscript. The range of 6.6–7.6 Sv is explained by the two estimations of volume budget for the upper and lower layers between the Greenland-Scotland Ridge and OSNAP East; ± 3.8 Sv indicates the error associated with both volume budgets, as explained in their section 2.

l.40, "drives AMOC variability": assertion, please specify in what sense, by what evidence

The assertion refers to the interannual variability over the OSNAP period (2014-2016), as shown by Petit et al. (2020), which is cited at the end of the sentence. A striking correspondence between the low-pass filtered variability of the AMOC and transformation rates has also been shown by Desbruyeres et al. (2019).

l.53–64: here you raise several interesting aspects regarding how AMOC varies and what may (not) cause it. However, I cannot see that you return to these aspects at all. E.g., to what extent does your study add to resolving the issue(s)?

Regarding what measure of AMOC?

This paragraph summarizes past work that has shown large property changes in the subpolar gyre over the past several years. Our study investigates the role of these property changes on the transformation of surface water, by focusing on SPMW densification. As indicated above, the densification of SPMW is a key process in the pre-conditioning of dense water formation downstream (de Boisséson et al., 2012; Brambilla et al., 2008).

l.65–73, fixed PV and density-class boundaries: how adequate are such fixed classifications in a variable/warming climate? And in general, AMOC is not fixed to overturn within a fixed density "window"? (general question, by no means specific to this study)

We use fixed density boundaries because we are interested in the evolution of this specific layer over 40 years, which is called SPMW over the Iceland Basin. More precisely, we include various class of SPMW by considering a large density window, thus we are less impacted by the climate change at longer time scales.

l.103: why no more recent OSNAP data than May 2018?

At the time of the study, the data provided by the OSNAP program were only available until May 2018 (Li et al., 2021). OSNAP data from 2018 to 2020 are still being processed.

l.110,113: are ERA5 and EN4.2.1 consistent/compatible data sets? Could you have settled for using only one? And how about AVISO in defining the gyre/area?

The ERA5 atmospheric reanalysis does not provide salinity at the surface, and EN4.2.1 does not provide air-sea fluxes. Thus, we combine these two data sets to estimate the transformation. Unfortunately, there is no observational dataset that provides both air-sea fluxes and hydrological properties at the sea surface. Sea surface height from AVISO was used to define the gyre boundary following Foukal & Lozier (2017).

l.160: what's the time range for the AVISO data

The time range for the AVISO data has been added, and the sentence now reads:

"To estimate the subpolar gyre boundary, we use monthly absolute dynamic topography fields from the gridded ¼° AVISO (Archiving, Validation and Interpretation of Satellite Oceanographic data center) altimeter products distributed by CMEMS (Copernicus Marine Environment Monitoring Service) in 1993–2019."

l.177-179: I disagree with this qualification, in my experience this is quite a substantial correlation in climate-related data. Relatedly – "lower than expected", what was expected? And how about the compatibility between the

two data sources used to diagnose the respective fields?

We agree that a correlation of 0.52 is non negligible in climate-related data, but we can consider these variables as weakly correlated because they explain less than 50% of the variance between the buoyancy flux and the change in SPMW thickness, in opposition to the correlation of more than 0.6 found between the buoyancy flux and the SPMW transformation in Figures 3(a), which explains more than 50% of their variance. The sentence is now clarified:

"Despite these agreements, the linkage between the SPMW thickness and the buoyancy flux over the period 1999-2019 is relatively weak (< 50% of their variance), with a correlation of 0.52 (Fig. 2c)."

l.198–199, "explain only 30%": I disagree with this qualification

According to the suggestion from Reviewer 1 and Reviewer 2, we changed "only 30%" to "less than 30%" because R2 is closer to 0.27 than 0.30 (R = 0.52), and the sentence now reads:

"Though a strengthening of the buoyancy forcing generally leads to an expansion of the surface area, the buoyancy flux in a given winter explains less than 30% of the surface density change in the Iceland Basin."

l.202–204, "sufficiently small... so that we can separately explore": I disagree, follows from the above. Use regression or similar to remove covariance and do statistics on what remain. Now it appears to be assertion in the place of analysis.

Thank you for the suggestion, we estimated a linear regression between the surface area and the buoyancy flux, and removed it from their correlations with the SPMW transformation in Figure 3(a) and (b). The correlations are now of 0.63 and 0.67, respectively. The sentence in l.202-204 now reads:

"To conclude, we removed the weak dependence of the surface density field on the air-sea fluxes so that we can separately explore the contributions of the air-sea fluxes and ocean surface densities on the interannual variability of SPMW transformation."

And the caption in Figure 3(a) and (b) now reads:

"Correlations between the SPMW transformation to densities higher than 27.4 kg m^{-3} in the Iceland Basin and the (a) surface area of 27.3–27.5 kg m^{-3} in the Iceland Basin, and (b) buoyancy flux over the surface area of 27.3–27.5 kg m^{-3} in the Iceland Basin. The dependence between the surface area and the buoyancy flux was removed to compute their correlations with the SPMW transformation. All correlations are computed using winter values (December to April)."

l.236, "observation-based experiments": is "experiment" the most appropriate term here? Isn't it more like a diagnostic/analysis?

Done. We now use the term "sensitivity analyses".

l.306–307, advection and re-emergence: this is a possibility, but what's the evidence? Can you add some independent analyses or at least some robust arguments?

We have now clarified that the possibility of re-emergence comes from the work of Grist et al. (2016). The sentence now reads:

“Thus, it is possible that the large formation of SPMW in winter 2013–14 over the southern part of the Iceland Basin contributed to the large source area of SPMW found over the northern part of the Iceland Basin the following winter 2014–15.”

l.309–310, "impact … on regional climate". This is unsubstantiated as it stands. And even if the Grist et al-paper referred to alludes to it, I don't find it substantiated there either.

Grist et al (2016) provides “evidence for the re-emergence of anomalously cold SPMW in early winter 2014/2015” by generating ensembles of particle trajectories in ORCA12 (their Figure 16). Thus, they assess the regional impact of extreme air-sea interactions over the eastern subpolar gyre.

l.311–313, "Thus...": is there a larger/AMOC/climatic implication to be substantiated here?

The implication here discusses the possibly strong indirect effect of buoyancy flux on the SPMW transformation (e.g., the advection of dense water formed by large buoyancy fluxes outside the study area during previous winters). An investigation of the impact of these events on the AMOC could be an interesting follow-up study, but it is out the scope of our analysis.

Section 5, here and elsewhere: I sometimes miss an explicit reference to the quantifications done. Does this refer to the (non-numbered) Eq 1?

Equation 1 is now numbered in the manuscript and referenced in section 5 (see answer below). The caption in Figure 6(a) now reads:

“Figure 6. (a) Transformation anomaly at the SPMW isopycnal 27.4 kg m^{-3} between 1980 and 2019, as estimated in Equation (1).”

Fig. 6 and its description/quantification: where does the Sv come from (see above comment)? And how can the reader translate/compare this to eventual AMOC (or similar) anomalies? And what AMOC (or similar) anomalies would those be? None are provided in the manuscript as far as I can see.

Figure 6(a) shows the transformation anomaly at the SPMW isopycnal, as estimated in equation 1. Following this equation, the buoyancy flux (term in square brackets) is integrated over the surface area of a density bin, which is associated with the SPMW density-range. The transformation is then in Sverdrups (Desbruyères et al., 2019).

The SPMW transformation over the Iceland Basin cannot be directly compared to AMOC anomalies at OSNAP East because the transformation is estimated at a lighter density than the AMOC isopycnal and does not include deep waters formed in the Irminger Sea. Moreover, we cannot directly compare the transformation over 40 years with the AMOC anomaly at OSNAP East as the AMOC measurements at the OSNAP line are available over 4 years, which highlights the necessity to obtain longer direct measurements.

l.320, 40 years of observations: I apologise, as this may reveal my sloppy reading. But the observational focus is still not perfectly clear to me by now – is it 1980–2019 and/or 2014–2018. This could maybe be even clearer, eg in the introduction.

Our study analyses the transformation of SPMW over the 40 years from 1980 to 2019, as indicated in the introduction (l.84-85).

l.322–323: same as above, downplaying what to me seems substantial correlations

See the answer above.

l.324–325, "We thus infer": same as above, inference as a more or less reasonable statement, but with no real analyses or substantial reasoning offered.

We clarified the sentence that now reads:

“We thus infer that other mechanisms influencing density changes, including ocean advection, mixing and wind-driven upwelling (Johnson et al., 2019), account for the remaining variability in the surface area of the source water.”

l.336, and elsewehere, "re-emergence": it's not clear to this reviewer what is specifically referred to, nor how the authors arrives at this conclusion/suggestion

See the answer above. Re-emergence refers to Figure 16 of Grist et al. (2016).

l.342: NAC had been found literally "to drive" or rather "to advect" anomalies?

Done. The sentence now reads:

“The NAC is shown to advect large salinity and temperature property changes at the surface and subsurface (Holliday et al., 2015), with strong property changes attributed to changes in the fraction of water from the Labrador Sea that reaches the Iceland Basin (Holliday et al., 2020).”

Fig 1: you may want to zoom out a little in Fig 1a to provide more regional context (and a convenient intro illustration)

Done.

Fig 6, caption "during years indicated by color code": too implicit/subtle

The caption in Figure 7 has been clarified and now reads:

"(b) Blue (red) contours show the subpolar gyre boundaries in January during winters of 'small' ('large') area, as identified in panel (a). (c)-(d) Surface density area 27.3–27.5 kg m^{-3} averaged during winters of 'small' (blue) and 'large' (red) area, as identified in panel (a)."