

Ocean Sci. Discuss., referee comment RC3  
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## Comment on os-2021-48

Anonymous Referee #3

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Referee 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-RC3>, 2021

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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?

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

Figures: several of the panels are a little too stamp-sized as they appear; some

cumbersome units/crowded axes (e.g., 2c 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

#### 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

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

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 measure of dense water production and AMOC does the statement relate to?

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

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

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?

I.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)

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

I.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?

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

I.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?

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

I.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.

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

I.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?

I.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.

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

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

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.

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

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.

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

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

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

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