Ocean Science Discussions

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Comment on os-2021-60

Anonymous Referee #2

Referee comment on "Weakening and warming of the European Slope Current since the late 1990s attributed to basin-scale density changes" by Matthew Clark et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-60-RC2, 2021

In "Weakening and warming of the European Slope Current since the late 1990s attributed to basin-scale density changes." Clark, Marsh and Harle present an analysis based on gridded reanalysis products and numerical model simulations of the changes in the European Slope Current (ESC) in the past 30 years or so. The analysis currently falls short in a few areas, and I would suggest some major revision is needed prior to acceptance. I think the authors do have access to the required information and outputs from their analysis, and I think many of my concerns can be addressed through clarification in the writing and through incorporation of information from the published literature.

The data products used

The ESC region is one of the best observed regions in the sub-polar North Atlantic. While data products such as EN4 and GODAS provide a full 4D overview of the region of interest. They are also often not great. The authors acknowledge this to some degree, although I find the statements on this quite confusing. In Section 2.1, the authors highlight that GODAS salinity is mostly "synthetic" and "seriously under estimates salinity variability", but in the discussion in Section 4.1, the EN4 lack of salinity data and gridding methods is flagged as a potential issue. I also find the assumption in lines 107-111 requires further evidence that it is appropriate.

Line 114-115: The two data products are stated to be independent of each other, but I doubt this is truly the case (e.g. if both incorporate Argo profiles). Particularly the following sentence highlights that these are likely the same four sources (please state here which ones also!).

The lack of consideration for the forcing mechanisms

The paper is highly descriptive of what is going on, but lacks to place this into the context of the forcing mechanisms. For example, there is no consideration for the positioning of the sub-polar front in the North Atlantic, there is also no consideration for the wind-forcing of the circulation of the wider SPNA and how this influences "recruitment" into the ESC or otherwise. Especially given the discussion on zonal current variability, I find these quite major omissions in the analysis. The discussion is more a continued description of the results presented, rather than any contextualisation in terms of previous work and/or forcing mechanisms. Section 4.2 is more speculative on implications, and a repeat of what has already been stated in the introductions.

The reductionist statements on salinity and lack of error estimates

The authors rely on the data products to provide accurate baroclinic transport, but based on potentially erroneous salinity data. There is little quantification of salinity error or overall error analysis, it is therefore difficult to know whether this really is of no significance to the results presented.

Lack of a general figure with key circulation features and locations

The paper lacks visual cues of the lines/boxes etc used, as well as a figure that highlights of the focus area of the study sits within the Sub-Polar North Atlantic (SPNA). Even for someone with expertise in the region, it is difficult at times to follow which transect has been used or across which box particles have been quantified. None of the figures show the "analysis region" (line 165) in full, for example.

Further Comments (some, but not all, minor)

Line 34—36: Johnson and colleagues find that the changes in the water mass properties and nutrients concentrations at the Extended Ellett Line are related to changes in properties of the circulation. To my mind, this is not the same as changes in concentrations in upstream flows, as the authors state.

Line 46-47: Suggest rewrite for clarity "The Gulf Stream flows between these two and eventually …"

Line 51: "However, not all of the water follows this pathway." (missing "of").

Line 52-56: This description neglects some of the other exchanges in the northern North Sea, particularly the Norwegian Trench inflow. The authors have spent great length emphasising the importance of the ESC to the marine ecosystem of the continental shelf, so a correct description here is warranted.

Line 67-69: This reduction in temperature was also accompanied with some very strong reductions in salinity. The region of the ESC was at its freshest for more than 120 years. Please see Holliday et al., 2020.

Line 131-132: This is solely the N-S directed component of the ESC volume transport. There is no further indication of this assumption in the paper.

Line 133: Has no reference velocity or assumption of Level of No Motion been applied? Further in the paper, there is discussion of the baroclinic and barotropic components of transport. I think this could be clarified here in the methodology.

Line 135: Why not state x 10^{-6}. The e-notation seems a relic of the coding.

Line 150-167: I found the description of the particle tracking methodology could be better: it is very detailed about some things (e.g. reference the initial positions file), but lacked details on other. Are particles released from all grid cells? Or only grid cells following the continental shelf edge? Later in the paper there is also mention of particles crossing certain transects. I would recommend some major rewrite of this section to ensure transparency and repeatability.

Line 204-205: This is quite a narrow temperature range. Please consider justification or broadening.

Line 209-211: Is this difference in transports an artefact of the referencing or the fact salinity is poorly constrained in the reanalysis?

Line 231-247 (and probably throughout): Practical Salinity is a unitless quantity. It should be used as such. Therefore text should say "Practical salinity in the range 34.25-36 …). That being said, oceanographers agreed to adopt the TEOS-10 convention, and the authors already use the Gibbs Python functions, so Absolute Salinity should be used.

Line 250-252: Recommend to plot these transect locations on a map. If not on a general overview map, at least on one of the pre-ceding figures.

Line 250-288 (Section 3.3): This section is very descriptive, but lacks interpretation (here or later in the discussion) on how this relates to what is already known of the region's circulation. I was unclear what the authors consider the novel finding from this analysis.

Figure 1: A continuous colour bar is not helpful to the reader. I would suggest using fewer, more discrete intervals in the colour scheme. The quiver is also quite difficult to see and may need to be scaled up. Which months do the authors consider winter/summer? [I note the colour bar does improve in future figures]

Figure 2/3: The decadal distinctions are a human reflection of the calendar, rather than a reflection of the physical ocean climate. The "warm/cold phases" are not specifically associated with the changes from the 90s to the 00s – for example with major changes happening mid-1990s. The authors should consider using a more objective way of combing years into more meaningful "warm/cold" or "strong/weak" composites.