Ocean Science Discussions

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Reply on RC2

Matthew Clark et al.

Author 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-AC2, 2021

We would like to thank anonymous Reviewer 2, for their comments on this work as provided in "https://doi.org/10.5194/os-2021-60/RC2". We will now respond to each comment in turn, with *Reviewer 2's original comments presented in italics*. For ease of reading, we have retained the headings (**in bold**) used by the reviewer for clarity.

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!).

We acknowledge that, at times, our explanation of the limitations of both the GODAS and EN4 datasets could be stronger and/or clearer. Combining Reviewer 2's comments with Reviewer 1 (Hátún), we will be removing EN4 data from the figures, but will continue to provide a text-based description of the merits and downsides of EN4. We will provide further justification (line 107 onwards) on why GODAS is an appropriate dataset to use in this region; for the purposes of assessing the hydrography of the North Atlantic basin and determining geostrophic volume transport estimates towards the shelf edge. Whilst GODAS and EN4 do indeed incorporate Argo profiles and other similar data (to be stated as per the recommendation), the ways in which the datasets are assimilated and gridded differ considerably. Clearly this needs further and clearer explanation in the text, backed up with the relevant references.

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.*

We agree that the discussion needs to better compare our results with previous published works and the likely forcing mechanisms. This should include analysis of the sub-polar front, NAO index and surface winds. Specifically, we will discuss and further emphasize:
• how changing wind stress curl at basin scale forces changes in the strength and configuration of the subpolar gyre, of consequence for barotropic inflow to the Slope Current
• how changing wind stress in the vicinity of the European shelf break forces changes in the Slope Current via Ekman transport towards the shelf break
• how changing buoyancy forcing at basin scale forces changes in baroclinic inflow to the Slope Current

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.

We will further emphasize that density variability is dominated by temperature variability in the subpolar North Atlantic. Repeating the thermal wind analysis with climatological salinity, we can evidence that uncertainty in the GODAS salinity data does not substantially affect our results or conclusions.

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.

We will annotate figures accordingly (for example: our shelf edge transects can be added to our amended Fig. 1) and enhance the text appropriately, to clarify the choice and use of selected transects and sub-regions, throughout.

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.

We are familiar with this previous finding, and we will clarify that changes in property transport, consequential for the shelf edge and shelf sea of northwest Europe, are the combined consequence of changes in properties and flows (as everywhere!); we will emphasize that properties and flows may be correlated, as evidenced in our analysis by a weaker, warmer Slope Current in the early 2000s, while acknowledging that property changes may be the dominant factor at locations where volume transport is relatively steady (such as EEL).

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").

The minor revisions on lines 46-47 and 51 as recommended above will be implemented in full to improve clarity and readability.

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.

We will endeavour to cover the Norwegian Trench inflow in our introduction, referring back to relevant literature.

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.

Thanks to the reviewer for highlighting this paper. We will highlight declining salinity in the ESC and wider SPG region.

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.

We are not sure what the reviewer is referring to here. Equation 1 (on line 129) is simply the eastward velocity change over depth, from the thermal wind relationship. The equation was used to calculate a geostrophic velocity, which is then integrated over depth to obtain the eastward volume transport; therefore indicating transport flowing towards the shelf edge, as a measure of the inflow towards the shelf break, where geostrophic flow must turn to the north. It isn't until Fig. 10 where we introduce the concept of northward-directed ESC volume transport. Captions for Figs 6 – 10 clearly state the direction of transport being plotted, as well as the location of the transport sections (whether geostrophic or total volume transport).

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.

Equation 2 (on line 135) states that the calculation is performed with -1000m as our reference/no motion level. This can be made clearer in the accompanying text. The "e" notation is from the coding and it is sensible to replace this with "10 to power" notation.

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.

As we outline (lines 155-159), 'Particles were released proportional to the northwards transport at the shelf edge, between 50 – 60 °N; as such the exact number of particles released varied with each experiment. Releases were designed to target the "core" of the Slope Current: cells were defined as an active release location if there was northward transport present and that the bathymetry was between 200 – 250 m, taking care to exclude other shallow areas such as the Rockall Bank.' More specifically, particles are allocated at all grid cells within this definition, on sub-grids in proportion to local speed. By our definition, these grid cells do follow the shelf edge. Regarding the 'census' of particles crossing the 30W transect, this can be included in Figs. 11-13, and we will more clearly explain how we define 'crossing', in order to obtain the histograms in Fig. 14-15.

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

Within the text, we have already justified this temperature range in line 204 – "based on the mean decadal temperature anomalies presented in Figure 3". This will be improved with further justification.

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

In response to Reviewer 1, we are removing the ORCA12 time series, so in the revised manuscript we will not address these differences.

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.

We confirm that salinity as provided by GODAS is in units kg/kg, akin to absolute salinity (units g/kg); we will clarify this in the revised manuscript.

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.

This is a good idea. We will adapt Fig. 1 to show the profiles used for calculating northward transport at the shelf edge. Likewise, for the eastward transports at 30 °W, we will add the profiles to Figs. 2-4.

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.

We will revisit this text to provide more quantitative perspective, and to emphasize our key finding that the inflows feeding the Slope Current are systematically different when we consider years of 'cold/strong' and 'warm/weak' Slope Current transport. In the latter years, we find that a shift towards reduced inflows from more subtropical latitudes. We consider this to be a novel finding, via a Lagrangian methodology that is more intuitive than observations of transport time series at separate (e.g., upstream, downstream) locations.

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.

Figs 2-4 will be revised to show the shift between the warm and cool periods of pre and post 1997 respectively. This will then better align with the T-S figures presented in Figs 8-9, and also will better link back to the observed temperature shift in the previous literature and our own study.

Once again, we thank the reviewer for their comments and suggestions.

Kind regards,

Matt Clark,
Lead Author
on behalf of all authors.