

Author's response to reviewer comments, from the second review

Many thanks to Hjálmar Hátún and an anonymous second reviewer for their constructive comments on our manuscript. We will begin by responding to Hátún's annotated PDF of our manuscript, before moving onto the comments provided by Reviewer 2. Original reviewer comments are provided in *grey italics*.

*The authors have responded to my comments/concerns in satisfactory ways.
A few additional comments are provided as annotations to the attached pdf.*

We were happy to hear that Hátún was satisfied by our previous responses, and grateful for the provided further annotations on our manuscript. The annotations highlighted some minor points which we had overlooked, such as minor typos, formatting issues with symbols used in the figures (1, 2, 4, 6, 7) and equations, and incorrect (or incomplete) references. These have all been amended as recommended. In response to the comment:

I guess the series stop in 2010, since this is when the model run terminated, right? It is a pity that that we do not get the chance to see if the transport might have intensified after 2015. I, however, understand that there will not be time to run this model up to date.

This is correct. Sadly due to computer and data access issues with the JASMIN computer system we had earlier in the analysis, we were only able to obtain the model run up to 2010.

Moving onto Reviewer 2's comments, we were disappointed that the reviewer found our previous responses unsatisfactory.

1) GODAS Description

In my opinion, it is incorrect to call GODAS an "observational dataset" (section title, line 116). It is an ocean reanalysis product, and should be termed as such. Apologies for not noticing in the initial submission. My not noticing in the initial submission led me to make some comments which were not necessary. However, I believe it's critical to acknowledge ocean reanalysis correctly. Line 111-112 needs revision to reflect that they are using "an ocean reanalysis product and an eddy-resolving global ocean model hindcast".

The reviewer is correct in saying that GODAS is an ocean reanalysis product. We have now amended the manuscript to reflect this, primarily in the methods section.

At the start of this description, the authors refer to "measurements of..." This is not strictly true, as the measurements have been assimilated into a numerical model (the GFDL Modular Ocean Model). Therefore the output of the reanalysis is not a measurement. I suggest replacing with "values of ..." when referring to the GODAS outputs (sentence line 118-119). On line 119-121, the correct verb in this sentence should be "assimilates". Line 126-127: GODAS does not interpolate, it is a reanalysis product, using a data assimilation scheme (according to the documentation a 3DVAR one), it is not an interpolation scheme.

Throughout the manuscript, we have now stated that GODAS data are "values" instead of "measurements". Line 119 has been amended to state "assimilates". In the methods section 2.1, we further clarify that GODAS assimilated measurements using 3DVAR assimilation.

On lines 183-184, the authors refer to GODAS “altimetry data”. This is not strictly correct, while GODAS assimilates the satellite altimetry data after 2007, this is not altimetry data. Even after 2007, the GODAS SSH field will be a numerical solution that balances all input data in the data assimilation scheme. The GODAS product refers to a “sea surface height relative to the geoid”, and the description should refer to this.

The reviewer is correct and we apologise for not spotting this in our initial submissions. We have now changed the description on line 179 to now read “GODAS sea surface height relative to the geoid (SSHG)”.

Finally, further suggested changes relating to this comment are: line 186-187, line 384-386, line 423; where the authors refer to “observational data”. On line 409, the authors use “observations” to mean “results”. I suggest replacing; “observations” as it is a loaded word in our subject area (I hope my comments above sufficiently illustrate), particularly where comparisons with numerical model outputs are made.

Where appropriate we now refer to “values” rather than “observations”.

2) NCEP wind fields.

The authors have now also applied a new data source, wind fields from NCEP. It is unclear whether they use this to refer to wind fields which may be included as part of the GODAS data distribution, or whether they have sourced this elsewhere. Suitable reference/acknowledgement to the underlying data and its provider should be made, as well as a short description (a meteorological reanalysis, or an observational dataset?).

In Section 2.2 lines 160 – 163, we now introduce the NCEP wind with a description of the grid resolution and data source.

3) Lines 146-167 / Transport calculation

The authors have much improved this section, however, I still found it implicit as to how they have calculated the total transport. Based on the description and equations (1) and (2), it is implied that the total eastward volume transport is the integral in equation (2) through the section from the eastward velocities from the GODAS reanalysis product, and the geostrophic eastward volume transport is based on thermal wind. It was my own error in not realising the reanalysis product is a model which also provides the absolute velocities, and therefore my comments in the initial review referring to no mention of the geostrophic referencing were due to not realising this.

We have now further amended the manuscript to clarify the differences between total and geostrophic transport. Equation 2 has been updated to reflect that this is the eastward geostrophic transport equation, and lines 158 – 159 reflect how we use the GODAS eastward component of currents to calculate the total absolute transport through the same region.

4) The choice of sections for calculation of transports

The choices of sections are at times difficult to interpret based in knowledge from the circulation of the region. At 30 W, the authors define transports between 45 N and 60 N, over 0 to 1000 m. This will include some of the recirculating branches in the sub-polar North Atlantic, and will include some transport which does not contribute to the ECS. Based on the histograms in Figures 12 & 13, 55 N as the northern boundary would have been a more considered choice in terms of how circulation at 30W feeds the ECS transport.

It is also not clear to me why the authors decided to define the slope current transport as solely the northward transport component. In my opinion, it make it less meaningful to compare to established calculations of slope current transport in the published literature (Porter, Hopkins, Huthnance and others).

We have shown in our results that the transports seen at our 30 °W 45 – 60 °N meridional transects show strong similarity in terms of changes to the transport over time in our northward transects at the shelf edge (Figure 8). Of course, not all the transport in this region will reach the shelf edge.

5) Other minor comments

Lines 196-237. The description of particle tracking is much improved. However, it would still be helpful to have a range of the number of particles released in each experiment.

In the methods section 2.4, lines 206 – 208, we have now stated the number of particles released in our experiments. We have also highlighted that the large difference in particle releases is directly related to the reduction of transport observed in our results.

Lines 248-249: I am presuming TEOS10, but this choice of words leaves that open for interpretation. I suggest omitting "... using the equation of state of seawater" and referring back to the methodology "(Section 2.2)"

Our analysis does indeed use TEOS10. We have referenced back to Section 2.2, and mentioned we are using TEOS10.

Other typos mentioned have been amended. The following figures were amended in response to:

Figure 1. Previous comments relating to this figure have not been adequately addressed. The image quality in the PDF file was still poor. I would suggest improving the scaling of the quiver to make arrows more obvious. There is no indication on months used for summer/winter. The caption states "relative velocity" but not what to; the text states the figure shows "quiver plots of velocity".

Figure 8. Caption states that transects are both ~100 km (first sentence) and 50 km (second sentence) long. Please clarify.

Figure 1 has now got arrows that are thicker and scaled by 2. The caption now states that summer refers to July, and winter refers to January.

The caption in Figure 8 has been amended to state the length of each zonal transect.

We hope that these amendments satisfy both reviewers and the editor. We look forward to hearing back.

Matt Clark, on behalf of all co-authors.