

Dear editor and referees,

As the lead on this manuscript, on behalf of all authors, I would like to thank everyone who has contributed to the reviews. We appreciate and value these diverse perspectives on our work and their helpful suggestions. We have tried to address all remaining comments and thereby improve our manuscript. Please find our detailed responses below.

Kind regards,

Yavor Kostov

On behalf of all authors

Referee #1

The authors have done an excellent job revising the paper, especially in terms of the literature and fitting the work into the context of previous studies. They have also done a good job explaining things more clearly. At this point I think the paper is basically suitable for publication. My only comment is I think the paragraph added on the ECCO representation of LSW, and the new figure 2, would be better if moved to section 2.1 (where ECCO is discussed), as it feels a little out of place in the introduction.

We thank Referee #1 for the helpful comments. We have moved Figure 2 and the text from the introduction that referred to Figure 2 to lines 147-172 in Section 2.1, as you suggested.

Referee #2

The authors have addressed / incorporated many of the reviewers' comments and suggestions. I think the manuscript is much improved, putting the present work in better context with the previous literature. I have several additional comments and suggestions as discussed below. I recommend acceptance after consideration of the items listed below (minor revisions).

We thank Referee #2 for the helpful reviews.

While some additional information has been provided on ECCO (and ECCOv4), what the differences are between the three releases have not been given, including the integration periods. This information should be provided in Section 2.1.

We have expanded this description in lines 137-147 to elaborate on differences between releases, including integration periods.

Also, please be consistent with the release notations throughout the text and figures, that is, either use, for example ECCOv4 release 4 or ECCOv4r4.

Thank you, we have corrected this to be more consistent in lines 137-147 and throughout the rest of the text.

The analysis considers certain months, but no justification is provided for why those months were chosen. For example, Fig. 4 uses months 37 and 61 without saying why they were chosen.

In line 405, we now say that we consider sensitivities on interannual timescales (e.g., three and five years). Similarly, in lines 476-479 we now say, “ We use our freshwater flux perturbation experiment to explore the adjustment of the subpolar North Atlantic and the response of LSW volume on intermonthly and interannual timescales (e.g., one, three, and five years after the surface perturbation).”

Then, another piece of analysis uses month 38 later (Fig. 6). Why? Why not use the same month 37.

For consistency, we have now used Month 37 in Figure 6. This is exactly 3 years after the end of the perturbation sustained throughout Month 1. This is now mentioned in the caption.

Please include justifications for all these choices. Otherwise, it feels like there is some cherry picking going on.

In line 405, we now say that we consider sensitivities on interannual timescales (e.g., three and five years). Similarly, in lines 476-479 we now say, “ We use our freshwater flux perturbation experiment to explore the adjustment of the subpolar North Atlantic and the response of LSW volume on intermonthly and interannual timescales (e.g., one, three, and five years after the surface perturbation).”

For consistency, we have now used Month 37 in Figure 6. This is exactly 3 years after the end of the perturbation sustained throughout Month 1. This is now mentioned in the caption.

We have, however, displayed an additional result that does not correspond to a whole number of years after the freshwater flux perturbation. In Appendix E, Figure E1a, we point out that in April, 15 months after the freshwater perturbation, there is an increase in LSW volume in the Irminger Sea. The transient response of the Irminger Sea complicates our picture, but leaving out this result would constitute an oversimplification, which we want to avoid.

The manuscript promotes / implies enhanced predictive skill associated with the NAC pathway in several places. This is summarized on 1.595-597. The implication is that there will be high skill perhaps up to pentadal timescales, but skill even for the first year is rather low. In my view, the results do not support what is implied / promoted. These sentences should be accordingly modified.

We gave modified the new lines 614-615 to read “The existence of such large delayed responses in the ocean system implies that surface conditions along the NAC pathway are a major source of LSW variability.” (rather than a major source of predictability)

Similarly, in lines 567-568 we say that “The existence of long characteristic lead times in the system motivates us to explore the predictability of LSW.” We do not say that sensitivity with long characteristic lead times necessarily gives rise to large predictability. Furthermore, in line 618 we talk about “limited” predictability (as we had already done in line 20 in the abstract.)

Finally, is it possible to link LSW thickness to some sort of overturning transport, say in Sv? The implication in the text is that changes in the LSW thickness will translate into “downstream” transport changes.

Thank you for this suggestion! In this work we focus on the volume of accumulated LSW rather than on the export of LSW as an element of the overturning circulation. The accumulation of a water mass and its export are not trivially related, as shown by Petit et al. (2021), cited here. There can be periods of enhanced production but unchanged export.

Desbruyeres et al. (2019), cited here, are able to relate surface-forced watermass transformation to transport variability. An important part of their analysis is identifying the timescale on which transformation and transport are related. In our case pinpointing the timescale on which LSW accumulation translates into transport variability would merit a separate dedicated study because of its importance.

Other comments and suggestions:

l.53: “Iriminger” -> “Irminger”.

Done in line 53.

l.159: Insert “meridional” after “Atlantic”.

Done in line 147.

l.214: Insert “arbitrary” after “large”.

Done in line 224.

l.219: “per forcing units”?

Done in line 229.

l.220: “depends” -> “depend”.

Done in line 230.

l.220: “in” -> “for”.

Done in line 230.

l.247: Switch the order of tau_N and tau_E and add “, respectively”.

Done in line 257.

l.283: Both here and elsewhere, I think “drive” should be replaced with “contribute to”.

Done in line 293 and all instances where we interpret our own results. We have kept the word “drive” when discussing previous literature on driving mechanisms in the introduction.

l.373-375: Both here and perhaps elsewhere, please provide some observational values so that the reader can judge how large the ECCOv4 biases are.

Unfortunately, almost all previous literature that we cite considers only section-based or profile-based LSW thickness results rather than basin-wide volume. Li et al. (2019) do provide a volume estimate that we now refer to in lines 163-165.

l.396: Define SSH here, not later on line 415.

Done in line 408.

l.411: “the Denmark Strait”.

Done in line 422.

l.426: No need to relist the figures again here as they are just given one line above.

Fixed in line 437.

l.444: “larger” -> “large”.

Fixed in line 455.

l.446: I think it will be good to use a different word than “pole”. You are referring to the maximum loadings or maximum / minimum values.

Following your suggestion, we have replaced the phrase “our positive and negative poles in the applied perturbation pattern” with the phrase “our positive and negative values in the applied perturbation pattern” in line 472. However, we cannot use the word “loadings,” as our sensitivity patterns are not EOFs, and that may confuse readers. That is why, we have no choice but to say “the poles of the sensitivity pattern are aligned with dynamical barriers such as inter-gyre boundaries” (lines 474-475).

Figure 7 and caption: It looks like the sign convention changed for this figure compared to the others. Please use the same sign convention for all the figures.

Thank you for pointing this out. We have changed the sign convention in Figure 7 for consistency, as you suggested. Also, in order to avoid focusing on arbitrary response times, we have now shown anomalies exactly 12 months after the applied perturbation.

l.475-490: This paragraph discusses the figures in Appendix E very extensively. I suggest moving these figures here.

Thank you, but we have to strike a balance, and the main text already has enough figures.

Also, introduce SST and SSS on l.475 and 476.

Done in lines 493 and 494.

Please connect the sentence on 1.477-478 to the contents of the very next sentence.

Thank you, we have now tried to make the logical transition in lines 496-505 smoother, as you suggested.

1.480: “the Denmark Strait”.

Done in line 505.

1.499: Delete “at each grid point”.

Done in line 533.

Figure 9: Correct the title for panel b to say “surface freshwater flux” in both occurrences.

Done in the title of Fig. 9b

1.525: Specify what is meant by lead and lag.

Done in lines 543-544.

1.580-581: It think it will be good to add something like “Although it has been found in previous literature,” to clarify that your claim to be the first pertains to use of a state estimate.

Done in lines 598-599. (We now say “highlighted” in previous literature).

1.728: “is” -> “are”.

Done in line 748.

Figure D1: Please make the actual plots larger. As is, there is a lot of empty space between the panels.

Done in Figure D1.

1.743-744: I am not really seeing what is stated here in Fig. 1E. Where is the increase in LSW production in the Irminger Sea in Fig. 1Ea? There is not much in the Irminger Sea. Thickening of the LSW layer near Cape Farewell is rather small in Fig. 1Eb. Please rewrite the discussion for this figure, both here and in the main text.

In Appendix E and the new Figure E1a, we now point out that in April, 15 months after the freshwater perturbation, there is an increase in LSW volume in the Irminger Sea. The transient response of the Irminger Sea complicates our picture. We would have a cleaner set of mechanisms had the Irminger Sea not adjusted. However, leaving out this transient result would constitute an oversimplification, which we want to avoid. At the same time, we do want to strike a balance in terms of the amount of information in the main text. That is why, we have left Figure E1 in the Appendix.

1.780: Delete “defined in Appendix F”.

Done in line 800.

1.780: “Labradror” -> “Labrador”.

Done in line 800.

1.781: “Greenalnd” -> “Greenland”.

Done in line 800.