

**Responses to RC2:** ['Comment on os-2021-14'](#), Anonymous Referee #2, 13 Apr 2021

**Overall response to both referees.** Our original response to the two reviews was that it would probably be a futile effort to submit a revised version of the manuscript as explained in our comment (**AC1:** ['Reply to both referees'](#), Bogi Hansen, 27 Apr 2021). After reconsideration and correspondence with the editor, we have decided to resubmit and the following text details our responses to the original reviews and the revisions implemented. We acknowledge that the original text (and title) was probably open to misunderstanding. We have tried to make the revised version clearer as to the aims, analyses, and results of the study. We have not, however, included any additional types of data sources as suggested by the referees. As elaborated below, we do not believe that any of the suggested sources (satellite-salinity, ARGO-profiles, or ocean reanalyses) would provide substantially more convincing evidence than the sources that we discuss in the original manuscript.

### **General referee comments from RC2**

**Referee comment:** This paper uses observational data (satellite altimetry, trajectory of surface drifters, in situ salinity measurements) to suggest an overlooked freshwater source off the Iceland coast. The authors argue that this addition of low-salinity waters likely contributed to the recent freshening in the eastern subpolar North Atlantic that was described by a previous study (Holliday et al. 2020). Despite the title referring to “freshwater source”, the manuscript does not argue for a source of freshwater, but rather a phenomenon where fresher surface waters off the coast of Iceland were diverted into the Iceland Basin due to an anomalous circulation pattern south of Iceland.

**Response:** The title has been changed (New title: “The potential role of Icelandic runoff in the extreme surface freshening event in the Iceland Basin around 2015”) to emphasize that we are referring to runoff from Iceland.

The study includes a comprehensive analysis of various data sets and observations and I acknowledge the attempt to connect these diverse datasets with each other. However, these results do not convincingly show the relevance of such a freshwater source to contribute to the overall freshening. Therefore this complicates without suitable justification the story that Holliday et al. (2020) laid out to explain the freshening.

**Response:** Our intent was not to explain the “overall freshening”, but rather the enhanced freshening of the surface layer. Hopefully, this is clearer in the revised text.

**Referee comment:** The main issue with this study is the lack of quantification. This type of analysis essentially requires a budget analysis in order to provide a clear freshwater estimate and establish that the redirection of low-salinity water off the Iceland shelf is a relevant signal in the eastern subpolar North Atlantic.

**Response:** With Table 1 and the discussion in Sect. 4.2, we did quantify the contribution from the Icelandic runoff to the freshening of the Atlantic inflow to the Nordic Seas and it was indeed small (original lines 409-412). As stated above, our main aim was, however, to explain the enhanced freshening of the surface layer relative to deeper waters. With the discussion in the original Sect. 4.1, we believe to have done that quantitatively. In the revised version, we have tried to emphasize this in Sect. 4.1 and Sect. 4.2.

**Referee comment:** The analysis presented here does not meet current standards given that there is now readily available data that goes beyond mooring and hydrographic ship data. In particular it is surprising that Argo profiling data has not been utilized to complement the other salinity observations. Especially for recent time periods, salinity profiles from Argo floats should provide a better picture of the spatiotemporal variability in that regions. There are also a number of gridded salinity products derived from Argo and other profile data as well as satellite-derived surface salinity estimates which are publicly and readily available. In particular, the use of ocean reanalysis products (e.g., SODA, ECCOv4) would be essential in such investigation as these allow closed budget analyses that can establish underlying mechanisms. It is essential to include these analyses, which then can be compared to the present hydrographic data to see if the picture is still consistent with the freshening and hypothesized pathway of freshwater over the Iceland Basin.

Response: Using data from profiling ARGO floats might be worthwhile, but we do not find it likely that the relatively few ARGO floats that at any time are present in the Iceland Basin with typical parking depths of 1000 m and high spatio-temporal variability can provide much more convincing evidence. As to ocean reanalysis products, their quality will depend not only on the observational data, but also on the model used. For the present purpose, we would need a model with sufficient resolution and realism to describe the flow over the Icelandic shelf and the shelf-basin exchange in a realistic manner. We do not know of any existing reanalysis product that satisfies this criterion.

#### **Specific referee comments**

**Title:** The title is misleading since it describes a “freshwater source”, but in fact the hypothesized mechanism is a change in circulation.

Response: The title has been changed to: “The potential role of Icelandic runoff in the extreme surface freshening event in the Iceland Basin around 2015”.

**Line 34:** What is meant by the term “buffering region”?

Response: The term “buffering” has been changed to “transition”

**Lines 43-46:** This statement needs references. I am not aware that the subpolar North Atlantic is nutrient limited and would expect it to be largely light limited. Thus, a reduction in vertical mixing could also lead to higher productivity due to a decrease in light limitation.

Response: Elsewhere in the original manuscript, we quote references suggesting that the central Iceland Basin (not the SPNA as a whole) may be oligotrophic. Here, the text was intended as a general statement to justify the importance of stratification for biological production. And we agree that stratification may increase productivity, which was the intent with the first part of that sentence: “increased stratification may affect instantaneous primary production”.

**Section 2:** I think this section would read better without the different subsection and instead have a single section with complete paragraphs. Also, I feel there is a lot of important information missing. For example, the EOF analysis is a key method in this study, but it is only described in the supplementary material. A shortened description should be included in the Method section too. As well, what is the

calculation method for freshwater thickness? What analysis software has been used (Python, Matlab etc.)?

Response: Subsections in Sect.2 have now been combined. The EOF analysis is a standard method and we did not find it necessary to describe it in detail. In the revised version, we have added somewhat more detail and changed the reference from Preisendorfer (1988) to a more well-known methodology text (Emery and Thomson, 1998).

**Line 128-129:** How has potential density been derived?

Response: In the standard way by using potential temperature and referring to the surface (Emery and Thomson, 1998).

**Line 130-134:** The location of these sections should be included in the map (Figure 1).

Response: Fig. 1a is already rather crowded and the sections are shown on Fig. 7. A reference to that figure has now been added to the caption of Table S1.

**Line 135-137:** This is confusing phrasing. ERA5 should be stated as the atmospheric reanalysis product and CDS the repository from which the data has been obtained.

Response: We used the text recommended by the data supplier

**Section 3:** Instead of calling the subtitles by the data product, it would be better to have an actual subject that refers to the finding/processes etc. For example, instead of the title “satellite altimetry”, it should be called something like “geostrophic flow pattern”.

Response: We found it difficult to find alternative headings for all the subsections and have not done this.

**Line 157:** Need to explain more how the spatial pattern in Figure 2 can be interpreted to identify the flow.

Response: The beginning text of this section (new Sect. 3.2) has been modified to: “In the geostrophic approximation, the slope of the sea surface is proportional to the speed of the surface layer”.

**Line 159-163:** I think the description of the EOF analysis fits better in Section 2.

Response: We have added some more text on the EOF method to Sect. 2, but have kept the results from the analysis in Sect. 3.

**Line 160:** Please specify the spatial domain that has been used in the EOF analysis (cite lat and lon bounds).

Response: Done

**Line 166:** “Anomalous behaviour” is unnecessarily vague. Just describe what is anomalous about it.

Response: Has been done.

**Figure 3 b-d:** Units are missing on the axis labels. Is it "cm" as in panel a?

Response: The product of the spatial mode and the principal component should have the same dimension as the original data set, that is cm (e.g., Eq. S1). As explained in the Supplementary methods, we assign the dimension to the spatial mode, which means that the principal component is dimensionless.

**Lines 174-176:** How is the connection made between zero to negative PC values and enhanced clockwise circulation? This connection has not been clearly explained.

Response: In the new version, an arrow is added to the new Fig. 4a and explanatory text added to the caption.

**Line 203:** I'm not sure if the term "streamlines" is appropriate here. This would set the EOF equivalent to a stream function, which I don't think is the case.

Response: This term is no longer in the manuscript.

**Lines 211-213:** It is not obvious to me how a fairly weak correlation between the zonal drift and PC time series leads to that conclusion. Maybe it helps if the authors actually describe the process behind the apparent correlation.

Response: The correlation has been improved in the new version and the text clarified.

**Lines 224-226:** The fact that the drop in salinity is confined to the middle of the basin indicates to me that the freshening is sourced from the south, as this region corresponds to the pathway of the northward flowing NAC.

Response: The text has been modified.

**Lines 248-249, Fig 7c:** How's the freshwater content calculated? This needs to be included in Section 2.

Response: The explanation is hopefully clearer now

**Line 254:** Cite the actual value that is chosen as the reference salinity.

Response: As explained in the revised version, the term "reference salinity" was misleading in this context. The new text should be clearer.

**Line 274:** The freshwater flux of 5 mSv needs to be put in context. This is quite small compared to any other freshwater flux estimate over the subpolar North Atlantic.

Response: In the revised version, this point is emphasized in the Introduction, as well as in Sect. 4 and Sect. 5.

**Lines 276-277:** Please clarify the relevance of this statement.

Response: The text has been modified.

**Section 4:** A majority of the content of this section can be regarded as results. Usually, the Discussion is for bringing up previous studies, raising potential concerns and caveats, and restating the main findings.

Response: The first part of Sect. 4 has been completely re-written and hopefully clarified.

**Lines 296-299:** If the freshening is explained by just deviation in the circulation, then shouldn't we expect salinification in the downstream region where the Icelandic source usually ends up?

Response: That may well be, and we have added a sentence to that effect in the new Sect. 4.3, but we do not have the data to assess that more quantitatively.

**Line 305:** This statement need to be supported by quantitative analysis.

Response: This statement (and the accompanying old Fig. 8) have been removed.

**Line 308:** The phrasing "more normal flow" is odd. It should be clarified what normal is.

Response: This sentence has been removed.

**Line 324:** The phrasing "drained of freshwater" does not make sense in the context of oceanic freshwater content.

Response: The text has been modified and the term "drainage" removed.

**Line 332:** Clearly describe the steps used to get to the estimate of 0.5 m.

Response: This is hopefully clarified in the revised text (new Sect. 4.2).

**Section 4.3** This suggest only a minor influence of the Icelandic freshwater source and thus contradicts the whole premise of this study.

Response: We assume that the referee means the old Sect. 4.2 (not 4.3) and we never intended to claim that Icelandic runoff was the main cause of the overall freshening of the eastern SPNA or the Iceland Basin. Only that this could explain the surface freshening became so extreme. This section has been substantially re-written (and abbreviated).

**Line 415:** Statement "improves conditions for primary production" needs references.

Response: The text has been rephrased.

**Line 427:** Specify a quantitative criterion to determine convection depth.

Response: The text has been rephrased and a precise definition is provided for the values in Table 2.

**Page 20:** This is a strange way to end the paper. A section containing Conclusions is missing.

Response: A new Conclusions section (Sect. 5) has been added.