Ocean Science Discussions

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Reply to both referees

Bogi Hansen et al.

Author comment on "The potential role of Icelandic runoff in the extreme surface freshening event in the Iceland Basin around 2015" by Bogi Hansen et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-14-AC1, 2021

Response to comments from both referees

With the comments from both referees posted, it seems clear that neither of the referees finds the manuscript worthy of publication without major revisions. Some of this is no doubt because of unclear explanations in our text. In hindsight, the title should probably have been less provocative, less certain, and more direct – perhaps something like "Runoff from Iceland may have contributed to the extreme surface freshening in the Iceland Basin after 2014". It was certainly never the intention to claim absolute proof of our hypothesis. Only to argue for its likelihood, which we still feel that we have done. Neither was it the intention to claim that the Icelandic freshwater source was large compared to the western source described by Holliday et al. (2020), as the referees seem to read our text. The aim was to argue that the Icelandic source – although much smaller than the western source – was local (to the Iceland Basin) and surface intensified, which could give the surface salinity of the Iceland Basin the extra downward kick to make it so extreme (e.g., lines 22-25 in the abstract).

Apparently, these messages were not sufficiently clear, for which we apologize. If this was the only problem, more careful rewriting of the text might make the manuscript more acceptable, but you would also expect referees to take this into account. The referees raise more serious questions, however, that are more difficult to address.

One of these questions is in regard to insufficient support. In our manuscript, we presented five types of observational evidence (satellite altimetry, surface drifters, Extended Ellett Line, OSNAP moorings, and Icelandic standard sections). Referee 1 discusses each of these and finds none of them convincing. Instead, he/she recommends using satellite sea-surface salinity. This might indeed be worthwhile but, to our knowledge, these data are not reliable close (50 km) to land (Sea Surface Salinity | NOAA CoastWatch & OceanWatch) and are not very accurate (doi:10.1002/2014JC009961). A brief look at these data indicates extremely high scatter in the Iceland Basin. It does not seem likely that these data can provide evidence of an Icelandic contribution that is much more convincing than the collective observational evidence already presented. A similar

argument may be made to the suggestion by Referee 2 to use ARGO data. Again, this 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 their preference for bottom depths > 1000 m and high spatio-temporal variability can provide much more convincing evidence.

A second question raised by both referees is in regard to quantification. It seems that the referees want quantification of the Icelandic source relative to the western source discussed by Holliday et al. (2020). This can, of course, be done although with some uncertainty, but we did not include it directly since we (as mentioned above) never intended to claim that the Icelandic source was of comparable magnitude to the western source. However, if this is what the referees mean by lack of quantification, Table 1 and the discussion in Sect. 4.2 do compare the two sources indirectly (summarized in lines 409-412). Our main argument was that runoff from Iceland was sufficient to explain the extra freshening of the surface layer relative to deeper levels. As shown in lines 328-342 in our manuscript, we feel that we have done that quantitatively.

According to Referee 1: "Introducing an entirely new concept to a well-developed field takes strong evidence". Alternatively, one might argue that a new concept deserves recognition even if it is not 100% proven as long as it does not contradict any basic principles. But that is, of course, a matter of opinion.

In summary, we agree that the manuscript could have been clearer in the text, but we still feel that the evidence presented supports the basic idea of a contribution from Icelandic runoff to Iceland Basin near-surface freshening, although not providing absolute proof. We do not find it likely, however, that we will be able to supply additional evidence that can appear more convincing to the referees.

We have therefore decided not to submit a revised version of the manuscript.

Tórshavn 27 April 2021

Bogi Hansen