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Comment on os-2021-14

Anonymous Referee #1

Referee 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-RC1>, 2021

Review of "An overlooked freshwater source contributed to the extreme freshening event in the eastern subpolar North Atlantic after 2014" by Hansen et al.

This manuscript describes a connection between Iceland-sourced freshwater and the freshening of the eastern subpolar gyre from 2014-2018. This freshening event has been previously identified by Holliday et al. (2020) as the largest freshening event in the past 120 years, thus the topic is of great interest to the oceanographic community, and is suitable for publication in Ocean Science Discussions. The authors of the present study use satellite altimetry, surface drifters, repeat hydrographic sections, and mooring data to support their claims of increased Iceland-sourced freshwater in the Iceland basin. The compilation of this many sources of data clearly took a lot of effort, and deserves recognition.

The idea that freshwater from Iceland can have an appreciable impact on the large-scale salinity structure of the subpolar North Atlantic is certainly a novel idea that is worth researching. However, the Iceland-sourced freshwater could be overlooked for good reason – possibly because it is too small to have an effect on the large-scale. It is a tall task to convincingly demonstrate that the oceanographic community has largely ignored an important phenomenon, and it requires strong evidence to do so.

I did not find that this paper presented such evidence. I found that the authors' central thesis lacked quantification (and thus was difficult to understand and prove/disprove), and the results were insufficient to support the authors' claims. I explain these points further below.

Lack of quantification: From the outset, it was unclear what signal(s) the paper is attempting to explain. Holliday et al. (2020) describes a freshening of the upper 1000 m of the Iceland Basin and its forcing mechanisms, without mention of freshwater emanating from Iceland. In particular, Holliday et al. (2020) find a remarkable agreement between the magnitude of the salinity increase of the Scotian Shelf/Gulf of Maine region with the magnitude of the salinity decrease of the eastern SPG, implying that the whole of the freshening in the eastern SPG can be explained by freshwater coming from the Grand Banks region. Yet this paper seems to assume that the mechanisms presented in Holliday et al. (2020) are insufficient to explain the freshening. What evidence is there of this? How much of the freshwater signal from Holliday et al. (2020) is unexplained? And can the magnitude of the freshwater flux from Iceland explain the unexplained portion?

Later in the text, the authors focus on the near-surface freshening as evidence of the Iceland-sourced contribution. But the authors acknowledge that the OSNAP moorings they use to track the vertical structure of the salinity anomalies lack surface instruments, so it is again unclear what signal the authors are trying to explain. If indeed surface salinity is of interest (and it appears that it is), why not use satellite sea-surface salinity, which has near global coverage from 2009-present? From this, one could construct a time series of surface salinity for a given region, and then do an analysis of the relative roles of various mechanisms.

Results insufficient to support claims: I will address each of the data sources individually:

- Satellite altimetry – the authors use satellite altimetry to demonstrate that there is a mode of variability in which the height of the central Iceland Basin is anti-correlated to the height of the shelf/slope region. This mode is especially strong in the 2014-2018 period, indicating that the cyclonic surface circulation around the Iceland Basin was strengthened during this period, “representing enhanced anticlockwise circulation that might bring water from the shelf/slope region south of Iceland in southwest-ward direction towards the western Iceland Basin” (line 176). However, there are two issues with this logic: (a) if the water were to follow the SSH isolines indicated in either Fig 2 or 3a, then the fresh water would flow almost directly into the Irminger Sea and not affect the salinity of the Iceland Basin, and (b) there is no indication that this mode is at all related to the shelf-basin exchange (geostrophic or ageostrophic) around Iceland. The authors acknowledge “the near surface flow... has no indication of an average flow from the Icelandic shelf/slope region into the central Iceland Basin” (line 156) and “it seems likely that the MDT and ADT are not sufficiently accurate in the shelf/slope region south of Iceland to reflect the actual flow...” (supplementary materials). Given these statements, the motivation for looking at altimetry to document shelf-basin exchange is unclear. What information does altimetry provide? Could adding the Ekman

- velocities onto the surface geostrophic velocities from altimetry be more informative?
- Surface drifters – the authors use 11 surface drifter tracks that crossed from the Iceland shelf into the western Iceland Basin between 1995 and 2018 to demonstrate that Icelandic shelf water influences the western Iceland basin. But these drifters represent a small portion (1/8) of the total surface drifters that crossed into the Iceland shelf region during this period, thus the western Iceland basin is not a primary pathway for the freshwater around Iceland. There is the possibility that at certain times, this pathway is more important than others (potentially important between 2014-2018), but the authors acknowledge that “With only 11 drifters taking this path, it would be hard to determine temporal variations” (line 207). The authors then attempt to tie the surface drifter pathways to the altimetry EOF, but their argument falls flat because the altimetry EOF does not indicate shelf-basin exchange around Iceland (i.e. the motivation for using the drifters). Thus I believe the surface drifters demonstrate the opposite of what the authors contend: that the majority of the freshwater on the Iceland shelf does not flow into the Iceland Basin, and that it either goes eastward into the Nordic Seas or westward into the Irminger Sea. The small number of drifters that flow into the western Iceland basin are a small percentage of a small freshwater flux from Iceland (~5 mSv, no reference salinity provided), and therefore likely represent a very small quantity.
 - Extended Ellett line – the authors contend that the salinity structure in the upper 200 m across the Iceland basin indicates an input from Icelandic-sourced freshwater. The salinity structure varies between years, and during periods of low salinity (2015-2017), the salinity of the central Iceland basin was lower than the eastern and western boundaries (in contrast to the other years, which increased almost monotonically from Iceland to Scotland). But again, this result seems to fly in the face of the authors’ argument that the freshwater is coming off Iceland – if the input from the Iceland shelf is confined to the western Iceland basin, as indicated by the surface drifters, then shouldn’t the salinity of the western Iceland basin decrease the most? The low salinities in the central Iceland basin (in regions of northward velocities) indicates that these low salinity waters are sourced from the south, rather than the north.
 - OSNAP moorings – the argument here is that the high-frequency variability in the vertical salinity structure cannot be explained by far-field forcing from the western boundary and instead requires more local forcing (line 300). However, I do not follow this logic. The Iceland basin is full of eddies and small-scale structure that advect property anomalies – why can’t these cause the high-frequency salinity variability seen at the OSNAP moorings? If the argument from Holliday et al. (2020) were that a ‘pool’ of freshwater came off the Newfoundland/Labrador Shelf and moved coherently into the eastern SPG, then I would agree with the authors that the vertical structure of this salinity anomaly would be eroded by the time it arrived to the eastern SPG. But I don’t believe that’s the argument in Holliday et al. (2020).
 - Hydrographic sections south of Iceland – I found the timeseries of freshwater thickness compelling, but given the infrequent coverage of the hydrographic sections, these data should only be considered ancillary, and not central to the argument. In other words, I would be more convinced of the authors’ arguments presented in this section if the altimetry and drifter data were stronger. Furthermore, the lack of velocity data diminishes what can be inferred from these sections, particularly regarding the strength of the freshwater fluxes. It was unclear to me even which direction these freshwater fluxes at each line are directed considering that Section IH “seems to be located in a divergence zone between eastward- and westward-flowing waters” (line 276). Does this imply that the freshwater flux at Section ST is eastward? How does that align with Fig. 1a? And what evidence is there to multiply the salinity fields by a constant 10 cm/s velocity (line 273)?

Despite my reservations about this manuscript, I am not convinced that the Iceland-sourced freshwater does not play a role in the large-scale salinity structure of the subpolar North Atlantic. In particular, Figs. 2 and 3 from Holliday et al. (2020) indicate that there were fresh anomalies in 2014-2016 south of Iceland that seemed to contribute to the freshening of the eastern SPG. Whether those fresh anomalies are sourced from Iceland is unclear, as well as why the anomalies appear to move against the cyclonic subpolar circulation into the eastern SPG, but it does deserve further study. This mechanism is particularly important given how much water mass transformation occurs in the Iceland basin, and the potential role of meltwater from Iceland.

Introducing an entirely new concept to a well-developed field takes strong evidence. It's possible that freshwater from Iceland is indeed overlooked but to prove that it is, the authors need to quantify its impact and rigorously demonstrate that it plays an important role.

To provide more guidance, I am including more specific comments below:

- Line 48-49 "most of the entrainment into overflow also occurs in the eastern SPNA" are you referring to most of the water mass transformation across the isopycnals of maximum overturning?
- Line 63 where is the Faxaflói line? Please mark this on Fig 1a.
- Line 85 – what is the reference salinity for the 5 mSv of freshwater carried by the Iceland Coastal Current?
- Line 87 – "In addition to this" – what does "this" refer to?
- Section 2 – Is it necessary to have so many subpoints? Can all the data be summarized in a single paragraph?
- Line 131 – it would be instructive if Supplementary Table S1 also included the seasonal and interannual timing of these cruises.
- Line 148 – Please elaborate on "This makes the result independent of any assumptions used in generating the MDT...". To what assumptions are you referring?
- 2 – If the black line delineates the contour that separates the flow north and south of the Faroes, why doesn't it intersect the Faroes?
- Line 154 and elsewhere – replace "anticlockwise" with "cyclonic" (or "clockwise" with "anticyclonic").
- Line 157 – what is meant by 'distorted'? The average flow pattern incorporates all time scales, including synoptic variability. The horizontal resolution of the SSH will not resolve the mesoscale, but why does that mean it's distorted?
- Line 165 – what is meant by long time scales? Fig 3c outlines the seasonal component of this mode... does the seasonality come into the argument at all?
- 3c – it would be useful to use a box-and-whisker plot here to show the median, quartile ranges, outliers, etc. Currently, it is tough to determine the strength of the seasonal cycle purely from the overlapping markers.
- 3d – is this panel a zoom in on panel b? If so, this should be highlighted in panel b

(maybe draw a rectangle in panel b around the bounds of panel d, or alternatively just make a note of this in the caption for panel d).

- Line 175 – It should be noted that this sentence refers to reduced cyclonic circulation, rather than an actual anticyclonic circulation in the Iceland basin.
- Line 210 – why is only longitude considered?
- Lines 207-214 – the argument that the altimetry and surface drifters are well correlated would be significantly strengthened if there were an accompanying figure. Can you produce a set of maps of altimetry and surface drifter tracks for ~4 time steps? Or alternatively show the two curves that are correlated? I am surprised that there is such good agreement between these data sets considering that the Ekman velocities are not added into the surface geostrophic velocity field. Would these comparisons improve if the Ekman velocities were added?
- Line 230 – remove comma after ‘months’.
- Figure S7b – at mooring M2, why does the salinity decrease from 50 m to 100 m when it increases in panel a and Fig. 6b?
- Lines 250-253 – It is not clear why the freshwater content is valid here. Schauer and Losch (2019) discuss the arbitrary use of reference salinities in the calculation of freshwater transport, and their arguments hold whether the freshwater came from ‘pure freshwater’ or not. The only way around this issue is to use a closed volume in which the mass budget is balanced. Another method is to report the freshwater fluxes relative to two reference salinities to demonstrate their sensitivity to the choice of reference.
- 7 – why is 200 m chosen as a bottom limit?
- Line 285 – There is no evidence presented here that the drifters followed different paths in different years, and the authors admit as much (line 207).
- Line 351 – how is the importance of the precipitation trend assessed relative to other mechanisms? Given that there is no explanation of what signal the authors are trying to explain, it is hard to assess whether the precipitation is small or large comparatively.
- Line 356 – This explanation of the errors in the MDT near land and steep topography should go in section 3.1. This is much more clear than the current explanation on lines 147-149.
- Line 361 – Two drifters followed this pathway in 2015 from table S2, but earlier the authors admit that they cannot use the drifters for temporal variability due to insufficient coverage. Along these lines, there were three drifters that followed this pathway in 1996 when the principal component was near zero.
- Line 364 – “we find it most likely that the Icelandic freshwater source was the primary cause of the near-surface freshening events in the 2015-2017 period.” I don’t know of any evidence to support this claim, particularly in regard to the ranking of roles that various mechanisms played.