We thank the Editor, the two anonymous reviewers, and Jamie Morison for reading this paper so carefully and providing such insightful comments. We have worked very hard to address your concerns as thoroughly as possible in the revised paper. All estimates of FWC from ORAs, satellites, and in-situ observations have limitations. Our strategy has been to include all of these sources to provide a qualitative understanding of Arctic Ocean FWC in the 2010s. More details are provided about the in-situ data sources. The satellite, in-situ, and ORA FWC estimates are calculated more consistently. We appreciate the concern about not including the Barents and Nordic Seas in the satellite observations but the motivation for this is explained clearly in the responses below. A spatial map of the location of in-situ measurements is now included to compare the in-situ observations more directly with the ORAs and satellite observations. We have reorganized the section on redistribution.

Our responses are shown with blue font below each comment.

Comments from the Editor:

Line numbers from manus version 3)

1. In general there is a mix of selected isohaline for vertical integration (34.8 vs. 34) and units of FWC (km^3 vs m). Could you be more consistent in a review (perhaps reproduce figures consistently)?
   Thank you for this comment. All analyses presented in this paper now use the 34 isohaline for vertical integration. We have recalculated FWC in units of meters.
2. Li 35: do we have enhanced mixing in the ocean? Does Timmermans and Marshall 2020 cover this? I think this claim is overreaching. Perhaps supported in the eastern EB (Fig 11 of Polyakov et al. JClim 2020).
   Reference changed and sentence modified to indicate enhancement in mixing regionally.
3. In line 40 you could cite Carmack et al. BAMS paper.
   Reference added.
4. Li 43-44: it is a bold statement that ocean stratification determines sea ice growth.
   The word “determines” has been changed to “affects”.
5. Li 45: I cannot follow how the part after “;” connects to the previous sentence. Perhaps a separate sentence could help.
   The sentence has been changed to read, “Freshwater in the Arctic Ocean plays a critical role in the global climate system; by impacting large-scale overturning ocean circulations (Sévellec et al., (2017), see Figure 1 showing basins and upper circulation); by changing ocean stratification that affects sea ice growth, biological primary productivity (Ardyna and Arrigo, 2020; Lewis et al., 2020), and ocean mixing (Aagaard and Carmack, 1989); and by the emergence of freshwater regimes that couple variability in land, atmosphere, and ocean systems (e.g., Jeffries et al., 2013; Wood et al., 2013).”
6. Li 46-51: AO freshwater balance somewhat ignored the vertical mixing.
   Line 51 has been changed to read, “redistribution between Arctic basins and vertical mixing (e.g., Timmermans et al., 2011; Morison et al., 2012; Proshutinsky et al., 2015).”
7. Li 55-95: Discussion around freshwater. I am disappointed that you join many others and follow the flawed traditional approach instead of taking a step in the right direction in a review in 2021. Fine. But as you end this section (page 3) please also mention what the alternative is (e.g., ocean salinity and salinity flux divergence analyses).

We have added this sentence to clarify our approach, "This atmosphere-ocean surface freshwater flux is a key element of the global freshwater cycle, predicted to amplify with global warming; hence the importance of knowledge of this surface flux, of its impacts on the ocean, and of the ocean's redistribution (and storage) of these impacts. Salinity, in comparison, is of indirect interest, for its role in seawater density and buoyancy, etc. Bacon et al. (2015) recognize that a surface flux requires definition of a surface area." and "Since this is a review of existing literature and in light of established practice, we continue to employ here the "traditional" approach to freshwater flux calculation by use of a fixed reference salinity, but we note that – if knowledge of the surface atmosphere-ocean freshwater is required – then the approach of Bacon et al. (2015) is preferable."

We note that we already answered this point in response to Ursula Schauer's comment in the first round of reviews. This manuscript is not about creating new knowledge, it is about reporting on what has been published. And what has been published uses this concept.

8. Li 58: "the concept of" instead of "the existence of such a concept as".
   The sentence has been changed as suggested.
9. Li 60: remove prime after freshwater-
   The sentence has been changed as suggested.
10. Li 99: Here OK to mention the two different contributions from GIS but could also mention its relatively small contribution in the Arctic freshwater budget and comment on the location and rapid export through Fram Strait.
    We have added a sentence mentioning this "Given this region is adjacent to the Fram and Nares Straits, it's likely much of the discharge from northern Greenland is rapidly exported." and we have also clarified the section on glacier discharge to quantify Greenland melt and solid discharge on a regional basis.
11. Li 102-106: "Halocline" (or 'cline) concept in ocean does not only apply to the depth range there is a gradient between only two water masses. This statement from Rudels is misleading. Halocline is a depth range with strong salinity gradient (no matter how many water masses are involved). Please remove such comparisons with the "rest of the ocean". To call the halocline "a cold and freshwater mass" is not accurate- please reword.
    These sentences have been removed.
12. Fig 1 caption: Please also spell out VS and SZ.
    "; VS, Vilkiltskiy Strait; SZ, Severnaya Zemlya" has been added to the end of the Figure 1 caption.
13. Fig 2A y-axis label is missing. Fig 2C, please mark the BG region as defined.
    Figure 2C and Figure 2A caption changed as suggested.
14. Fig 3B. Please mark BG. Unit of FWC anomaly is in km^3. Fig 2c in m. Satellite observations: It seems like you cut across the Arctic Ocean along 60E. There must be more data on the "other hemisphere", particularly important for the freshwater budget. Table 2. It would help to show the time and space distribution of the new data in a figure. Table could include total number of profiles. Where is NABOS data?
    The units in Fig 2b and c are correct, but the label "FWC" is not. The figure shows freshwater inventories, in m (fraction of the water column representing pure freshwater). The y-axis label has been adjusted to read: "FW inventory difference (m). Likewise for Fig 3b, y-axis label now reads "FW inventory anomaly (m)".
    We apologize for the omission and thank the reviewer for pointing this out. NABOS data are included up to 2013; the NABOS 2015 data is not included in the analysis. We have augmented

Table 2 to that effect. All other data is listed in Rabe et al. (2014). We have added a figure to Figure 3 to show all data points used in the objective analysis. Time is shown in color of each of the location dots, making the data coverage visible, in particular for the later years. For further detail on earlier years, up to 2012, please refer to a similar plot in the Supplement to Rabe et al. (2014).

15. Li 240: require[s]
    Text changed as suggested.
16. Li 261: delete "and changes in"
    Text changed as suggested.
17. Li 274: increasing rain as climate warms- any citation on this for the Arctic Ocean?
    A publication by Bintanja (2018) has been added.
18. Li 290: instead of "up to 50%" please give a range from ORAs.
    Our apologies for this text, it is not relevant to describe Figure 5. We have changed the sentence to read, "Freshwater stored in sea ice, i.e., sea ice volume, decreased by roughly 10% for maximum sea ice and 40% for minimum sea ice over 2000-2010 (Figure 5)."
19. Li 300: You mean "New" sea ice in the Arctic forms on shelves?
    We consider it is a tautology but added the word "new" nonetheless; the copy editor can remove it at a later stage.
20. Li 308: comment why faster TD matters, by exporting more FW?
    We added the comment "thus capable of rapidly transporting sea ice out of the Arctic"
21. Li 327: in response to storms vertical mixing can also bring up oceanic heat and melt sea ice. There are several observations in the literature. Do you want to also include this in the "sea ice" section? [and/or in the vertical redistribution discussion?]
    Following the suggestion, we have added discussions about storm-driven mixing and its impacts on sea ice melt and vertical distribution of freshwater at the end of the first paragraph in Sea Ice section and the third paragraph in Redistribution of Arctic Freshwater section. To support the discussions, we have also added two most recent citations.
22. Page 18: if so important, please mark the mentioned glaciers in Fig 1 so the reader sees where they are relative to the Arctic Ocean.
    Thank you for this comment. We have opted to just remove the glacier names.
23. Li 380-395: around here, more comments are needed on the location of Greenland in the large Arctic Ocean freshwater landscape and eventually to tone down its contribution. So, how much km^3 of FWC is actually going into the Arctic Ocean freshwater balance?
    We have edited the whole section for clarity and added the regional breakdown of ice loss from northern Greenland that goes directly into the Arctic Ocean, as well as the contribution from other glaciated regions within the Arctic including Svalbard, Arctic Canada and the Russian high Arctic archipelago.
24. Li 404: define mSv
    mSv changed to milli-Sverdrup and we added the specification (1 Sv = 10^6 m3 s-1).
25. Li 436-437: an entire sentence is repeated.
    Repeated sentence removed.
26. Section 3 is not well structured. It is a mixture of different processes and regions and their role in redistribution (lateral and vertical). There is also some overlooked literature and processes.

We have restructured Section 3 by adding subsections on; Competing processes in the Beaufort Gyre, Wind-driven circulation, Ekman pumping and wind mixing, Geostrophic density driven flow and shelf cascading.

Comments from Reviewer #1:

Major comments

The authors have softened their claim that there is fresh water compensation between the Beaufort Gyre and the rest of the Arctic in the abstract, however the presentation throughout the manuscript still needs significant improvement. Studies that suggest compensation seem to be preferentially cited (See L162 comment below) and some seem to be misrepresented (see L159 comment).

The analysis of the reanalysis products is much more comprehensively presented, but it would be very helpful to include boxes indicating which areas are being integrated over in addition to describing the regions in the Figure 2 caption. The same holds for Figure 3.

The regions in Figure 3 are now marked.

The satellite and in-situ fresh water estimates are very different during the period that this study focuses on, yet this is not discussed. The methods used for the satellite calculation are not presented (what empirical constants are used? What isohaline/reference salinity is being targeted?), so it is unclear how we might expect them to compare with in-situ measurements.

Details about the satellite calculations are now included in the text.

Compensation in the satellite estimate appears to decrease in the last part of the record. This is also not discussed. It is unclear why half of the Arctic is left out of (part of?) this analysis, and also unclear why an insitu estimate for the Beaufort Gyre is not presented. Presumably it is a subset of the full Arctic dataset? Please explain these analysis choices.

It is well-known that, while the sea surface height variability in the BG region is dictated by the variability in salinity, the same in the Nordic Seas and the Barents Sea is controlled by Atlantic Water temperature in comparison to salinity (Raj et al., 2021). Hence the methodology to estimate FWC from sea surface height data is not recommended in those two regions. Our study included the rest of the Arctic excluding the Canadian Archipelago, Nordic and the Barents Sea.

My recommendation is for major revisions because these observational analyses are a foundational part of this study and it is important that they be presented clearly, but note that the changes are much less significant than those recommended in the last revision.

Specific Comments

Note: line numbers refer to the tracked-changes version of the manuscript.

1. L67: Generally deltaS=S_ref-S, please check and revise.
   We have changed the sentence to read, "It usually manifests as a small fraction of the seawater volume or flux, where the fraction takes the form ($\delta S/S_{ref}$), and where $\delta S = S – S_{ref}$ is the deviation of the seawater salinity S from a reference value S_{ref}, and where the sign in the numerator is conventionally reversed, so that a positive scaled salinity anomaly reflects a freshwater reduction, and vice-versa."
2. L131: Only Greenland ice sheet contributions are discussed, this could be a good place to also introduce GIS runoff contributions.
   We have added further detail on the sources of runoff from Greenland in this section.
3. L139: Not sure that adding the depths of the halocline and Atlantic layer is useful, as they vary substantially across the Arctic.
   We removed the depth information.

4. L159: Which studies are you referring to? (Proshutinsky et al., 2009; McPhee et al., 2009; Rabe et al., 2011; Haine et al., 2015)? It is inaccurate to state that these studies do not take redistribution or the Greenland Ice Sheet into account.
   Thank you for this comment. We have revised the sentence to correct this inaccurate statement.
5. L161: Remove "(among other processes)" or explain which processes you are referring to.
   "Among other processes" removed as suggested.
6. L162: Rabe et al. 2011 and Morison et al. 2012 do not quantify the degree of compensation between the Beaufort Gyre and the rest of the Arctic Ocean. Wang et al. 2019 "Recent Sea Ice Decline Did Not Significantly Increase the Total Liquid Freshwater Content of the Arctic Ocean", show an updated time series from Rabe, which suggests that there has not been compensation between the Amerasian and Eurasian basins.
   Thank you for pointing this out. The sentence is not fully correct as is. We have deleted the sentence, as redistribution of freshwater is discussed in detail in Section 3.". Wang et al. (2019) show timeseries of liquid freshwater content (FWC) based on observational analysis for: the whole Arctic deep basin (down to 34 isohaline); the Amerasian Basin; and the Eurasian Basin. Wang et al. argue that most of the changes in liquid FWC occurred in the Amerasian Basin, they don't conclude from the observational timeseries that there is no compensation between the Beaufort Gyre and the remaining Arctic. The Amerasian Basin encompasses a significantly larger area than the Beaufort Gyre typically covers.
   Indeed, Rabe et al. (2011), showing a comparison between two time periods, do actually not conclude that there is a full compensation between the Beaufort Gyre and the remaining Arctic Ocean basin. Rather, they state that there is a general increase in FWC, accompanied by a decrease in specific regions (e.g. the Eurasian Basin north of the Laptev Sea). They also conclude from an ice-ocean model simulation that the overall liquid freshwater content change is related to an Ekman Pumping in the Amerasian Basin.
7. Figure 2: It would be helpful to show which areas are summed over to produce panel D in panel C.
   The area used to define the average over the Beaufort Gyre is now shown in Figure 2c.
8. L226: It would be useful to provide a brief review of the method used in Giles et al. 2012 and Armitage et al. 2016, and now in this paper, including the empirical constant used, for clarity.
   We have added this text to include a review of the Giles et al. (2012) and Armitage et al. (2016) methods, "It derives from the perceptions (1) that the sea surface height change (as observed by satellite altimeters) is the sum of two components: mass addition (or loss) and steric expansion (or contraction); and (2) that observation of mass changes (by satellite gravimetry) enables separation of these two components. A two-layer model is assumed, where the sea surface height and interface depth are variable, where the upper layer represents the halocline (and surface mixed layer) and the lower layer all underlying waters. Upper layer thickness changes (per unit water column area) are then a function of changes in sea surface height and water column mass, with assumed layer densities; and changes in freshwater content are then the thickness changes scaled by ($\delta S/S_{ref}$), with reversed sign. Giles et al. (2012) assume S_{ref} (their S_2) = 34.7 and upper layer salinity 27.7. While this is a very simple model, the observed signals are significantly larger than the uncertainty, as shown in their thorough uncertainty assessment (Giles et al. 2012 Supplementary Information)."
9. L265: Why aren't these areas included in Figure 3? Please explain this further.

It is well-known that, while the sea surface height variability in the BG region is dictated by the variability in salinity, the same in the Nordic Seas and the Barents Sea is controlled by Atlantic Water temperature in comparison to salinity (Raj et al., 2021). Hence the methodology to estimate FWC from sea surface height data is not recommended in those two regions. Our study included the rest of the Arctic excluding the Canadian Archipelago, Nordic and the Barents Sea.
The northward extent of the state-of-the-art altimeter SSH data developed under the ESA CCI SLBC project is 82.5N. The 'polar gap' is still a major issue. There are ongoing projects (ESA funded CRYO-TEMPO) which is trying to address this issue.

10. L278: Does this mean that the in-situ estimates at the end of the record may be underestimated? In this case, the discrepancy between the satellites and in-situ estimates may be even larger than what is presented here.
The mapping procedure uses a moving window of +- 3 years from the month of mapping. Those monthly maps are then annually averaged to obtain the liquid freshwater content. As the main increase in liquid freshwater occurred between , we cannot say if 2015 is underestimated, overestimated or the same as if we had used data up to 2018. We have adjusted the text to read: "Due to the method the annual values are biased towards the prior three years near the end of the timeseries, as the mapping analysis only includes data up to 2015; and 2012, 2013 and 2014 show similar levels as 2015. This could lead to an under- or overestimate of the annual mean value for 2013, 2014 and 2015."
11. Figure 3: Please indicate where your Beaufort Gyre region in panel B. In the caption you state that the time series are calculated to the 34 isohaline using a reference salinity of 35. This is likely not the case for the satellite calculation, which uses an empirical constant to relate steric changes to freshwater changes and should be clarified. Again, why is the in-situ BG time series not shown?
The bold black frame in the revised Figure 3b indicates the location of the BG. Details regarding the estimation of satellite derived FWC listed above in major comments.
12. L329: Please justify this statement and include a citation: "Precipitation over the Arctic is the main source of freshwater into the Arctic Ocean", including that from river discharge from the large continental drainage basins".
The total continental runoff into the Arctic Ocean is about 0.1 Sv (see Table 1, Haine et al., 2015). The remaining sources are lower, those of similar order of magnitude are Precipitation-Evaporation and Bering Strait liquid inflow. According to Zhang et al. (2013), river discharge from the surrounding continental drainage basins is predominantly contributed to by precipitation, though land surface processes (e.g., thawing permafrost, decreasing vegetation transpiration) may have small contributions. So, the total precipitation directly falling down to the Arctic Ocean and sea ice surface and indirectly through river discharge is much larger than the oceanic freshwater input through the Bering Strait. We have revised the sentence in the manuscript with more detailed discussions on this.
We have added appropriate citations (Haine et al., 2015; Serreze et al., 2006).
13. L456: Could reconsider the use of the word "Discharge" here as it is usually associated with only the solid component and you also discuss liquid runoff.
As the most recent studies explicitly call their data product discharge (see e.g. Mankoff et al., 2019) we prefer to keep the term, but we have added a line to make it more explicit that we include both solid and liquid discharge in this term.

14. L497: It would be useful if you could explain and lay out why the total flux is larger than the net mass loss in a simpler fashion.
    We have revised the whole section and made it shorter and simpler to read and included a clearer explanation of the difference between total flux and net mass loss.

15. L509: Does Mankoff consider only solid freshwater flux? Or total? Please clarify.
    Mankoff considers both solid and liquid, hence why we prefer to keep the term. We have explicitly stated this in the text.
16. L544: Translate Gt to mSv to make comparable with other sources.
    Thank you, we appreciate this comment but have chosen to keep Gt to be consistent with the source references.
17. L559: Could remove "as we do" since you are actively noting it in that sentence.
    Wording "as we do" removed.
18. L628: Odd phrasing. Steric height changes do not redistribute fresh water, rather they are reflective of freshwater redistribution.
    Steric effects change the sea surface height, which in turn drives barotropic geostrophic currents (here ignoring the effect of internal density gradients adding to the total geostrophic flow).
    We added "by driving near-surface geostrophic currents"
19. L640: Sentence is repeated.
    Repeated sentence removed.
20. L696: I didn't follow the last clause of this sentence, please clarify.
    We split the sentence in two so that we could more clearly explain the last clause:
    Further, Liang and Losch (2018) show not only that the positive feedback loop "enhanced vertical mixing = less sea ice" reduces the halocline-to-Atlantic Water (AW) stratification, but also leads to a colder AW. They argue that this colder AW combined with enhanced vertical mixing leads to an increased mixing between AW and deeper water masses, thus bringing more salt from AW to the deep Arctic.
21. L706: Suggest weakening "is due" to "is potentially due", or "appears to be".
    We feel "is due in part" is sufficiently weakened.
22. L714: Unclear why "to distinguish trends from low frequency variability" is included here.
    Here we meant that continuing monitoring would help distinguish climate change forced long-term trends from low frequency variability. The sentence in the manuscript has been reworded.

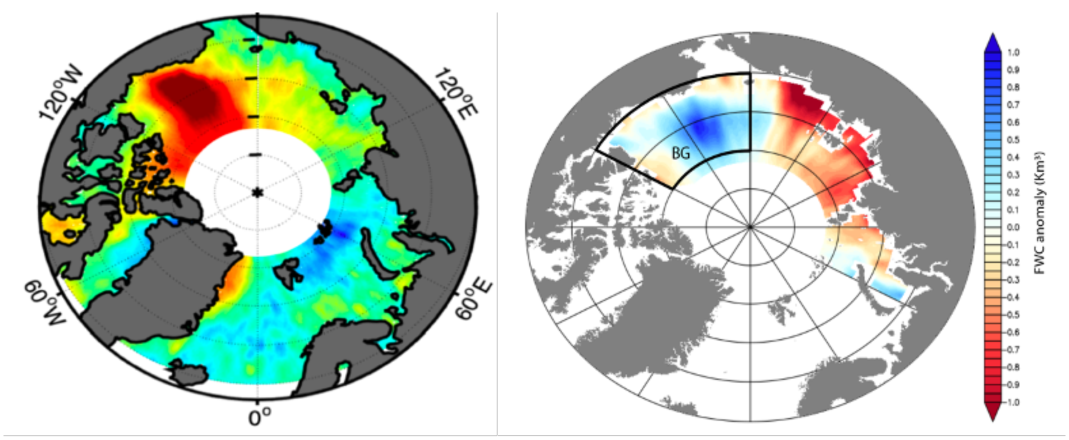
Comments from Reviewer #2

1. Line 105: The statement that in the Arctic "the halocline is a cold and fresh water mass" is not that accurate. As everywhere, the Arctic halocline is still dominated by strong vertical salinity gradient (e.g. it is not fresh). And the composition of Arctic halocline is not simple. In the Eurasian Basin, halocline is composed of cold halocline layer (where temperature is close to the freezing point and salinity increases with depth) and lower halocline water (where both temperature and salinity rapidly increase with depth). In the Amerasian Basin, this structure is further complicated by the presence of Pacific waters.
   We have deleted the wording, "As noted by Rudels et al. (2004), the term "halocline" is misleading yet common practice. In the rest of the world ocean, halocline denotes the depth range where salinities abruptly change as two water masses mix; in the Arctic, the halocline is a cold and fresh water mass.".
2. Line 305: I am not sure that one-year long travel time from the Laptev Sea to Fram Strait is a realistic estimate.
   During MOSAiC, ice formed north of the New Siberian Islands in December 2018, then reached the Fram Strait in late spring 2020 (Krumpen et al., 2020; The Cryosphere, 14, 2173–2187, 2020; https://doi.org/10.5194/tc-14-2173-2020). This was an extreme year, but demonstrates that a 1.5 year travel time is realistic. We have changed the wording to "1.5-3 years".
3. Section 2.4. Even though this section is mostly about the Greenland Ice Sheet, the contribution to the Arctic FW balance comes (and is discussed) from the Greenland and other glaciers. So, the title may be changed to better reflect these various sources of FW. I found that this part is disproportionally long relative to other sections. A lot of discussion which is not directly relevant to the Arctic FW can be omitted. I recommend to compare contributions of different sources (PE, sea ice, riverine water, including Greenland and glacier ice melt) to the overall Arctic FW (a summary Table?). E.g. there is no direct comparison of the Greenland ice to the Arctic FW balance. One can deduce it from the text comparing 26Gt versus sea ice decline from Fig 5, for example. But it will be beneficial for the paper to have such a summary.
   We have thoroughly revised this section and renamed it "Freshwater Flux Discharge from glaciers and the Greenland ice sheet" including adding in some context from other glaciated parts of the Arctic that also contribute freshwater and some more easy to compare numbers.
4. Line 393: "where the identify" needs edits.
   This section has been changed to read, "The Bamber study estimates that including land ice from other parts of the Arctic as well as the Greenland ice sheet, the total freshwater flux is around 1300 Gt per year in the period since 2010. They also identify a marked increase in runoff and discharge compared to a climatology period of 1960 to 1990."
5. I agree with the other reviewer, that the paper (still) lacks comprehensive discussion of spatial redistribution of FW within the Arctic Ocean proper.
   Please see Review #3 comment #12. We have added the suggested text about redistribution for the 2010-2019 period and the relation to atmospheric variability to the manuscript.

Comments from Reviewer #3 (Jamie Morison)

This is a review of "Freshwater in the Arctic Ocean 2010-2019" by Solomon and others. The authors review relevant literature and make their own computations of freshwater content (FWC) for the Arctic Ocean from hydrography and remote sensing (altimetry and GRACE). They argue that Arctic Ocean freshwater content has stabilized since 2010. The authors have responded to prior review comments and the revised paper is a substantial improvement over the earlier version. However, the authors themselves raise questions about adequacy of their data for reaching solid conclusions. As they point out, inadequate spatial coverage is a likely reason freshwater content from their in-situ observations do not agree with their remote sensing after 2010.

Further, although they are using altimetry-derived sea surface heights from DTU that they argue uses a retracker suitable for detecting sea surface height in sea ice regions, they have no remote sensing-derived freshwater content in major important regions: north of 82°N and the whole Fram Strait quadrant of the Arctic Ocean. Therefore, it isn't clear from the data analysis what freshwater content has done in the 2010s. Consequently, as it stands the utility of the paper is its timely review of research. Additional references suggested below or a closer look at existing ones might help improve the data analysis and provide additional insights into changing freshwater content.
We thank the reviewer for pointing this out. We agree that there are few points not been made clear in the previous versions of the manuscript. The spatial distribution of the ESA CCI SLBC SSH data produced by DTU, used in this study is shown below (left panel; Figure from Raj et al., 2020). Note that the latitudinal extent of the data is 82.5N. In the manuscript Figure 3b (right panel shown below) shows the region in which the FWC is calculated from satellite data. The Nordic Seas and the Beaufort Gyre is purposefully excluded because the sea surface variability in these regions are dominated by the changes in Atlantic Water temperature in comparison to salinity changes (Raj et al., 2020). Hence the methodology used (detailed in the revised version) may not be accurate in these two regions, in comparison to BG region where SSH variability is due to changes in salinity (e.g., Raj et al., 2020).

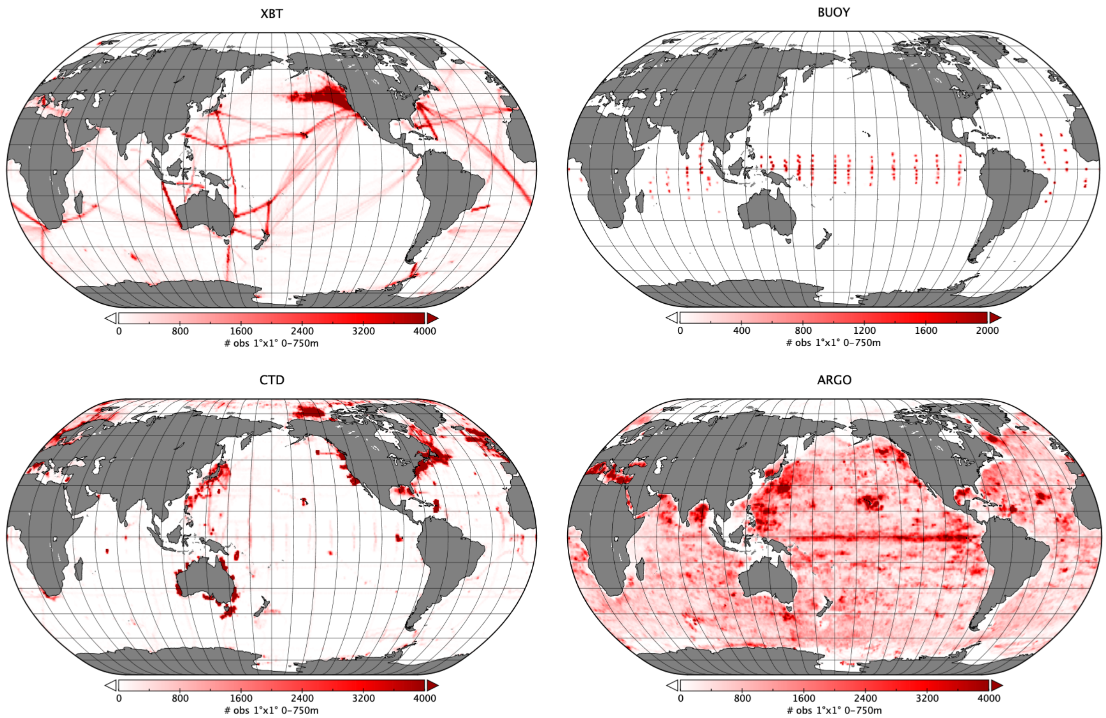


-Best regards,
Jamie Morison

Detailed Comments

1. L130 and Captions Figure Labels Fig. 2a - Units? Freshwater content in meters? What are the offsets for the different lines?
   Figure 2a units were cropped out and the figure has been corrected. The offsets are due to different FWC estimates in each model.
2. L132-137 - This is an important point. Fig. 2b shows the increase in FWC in the Beaufort Sea is compensated at least to some degree by FWC loss on the Russian- Eurasian side of the Arctic Ocean. This is characteristic of the cyclonic mode of circulation [Morison et al., 2021; Morison et al., 2012; Sokolov, 1962]. The wide variability in FWC change among the ORAs on the Russian side is likely due the paucity of observations there in recent years. Morison et al., (2021) speculate that the in situ observations have had an increasing spatial bias toward the Beaufort Sea. It would be a great contribution if this paper could address this by somehow illustrating the locations observations that went into the ORAs.
   Thank you very much for these comments. The text now reads, "This is characteristic of the cyclonic mode of circulation (Morison et al., 2021; Morison et al., 2012; Sokolov, 1962). However, there is a significant spread in estimates of freshwater content in the Beaufort Gyre and the rest of the Arctic Ocean (Figure 2d), which prevents a definitive estimate of the degree of this compensation. The wide variability in FWC change among the ORAs on the Russian side is likely due the paucity of observations there in recent years. Morison et al. (2021) speculate that the in-situ observations have had an increasing spatial bias toward the Beaufort Sea. This highlights the need to be able to estimate the redistribution of freshwater when assessing changes in Arctic Ocean freshwater, as well as the recent reduction in total Arctic Ocean freshening relative to the 2000-2010 period."

   The ORAs used here do not share datasets and the assimilation schemes treat data in different ways. Locations of assimilated observations are different among products because the original observational datasets may differ and the quality check criteria that are applied to data are also different. The distribution of all in-situ datasets used in this set of ORAs cannot be presented in the paper, but the following figures show the data distribution for the period 2004-2017 as given to the C-GLORS system (please note that Argo were not used before 2003). Assimilated data in other reanalyses might be similarly distributed but not exactly collocated.



3. L166-169 - Yes, but sea ice was one of the main motivations for launching CryoSat2, and there are efforts in addition to the ones used in this study that discuss dynamic ocean topography from CryoSat-2 for the ice-covered seas, e.g., Kwok and Morison, (2016, 2017); Armitage et al.(2018a, 2018b).
   We thank the reviewer for pointing this out. We agree that the statement above may not be correctly postulated. We have revised the text as follows:
   "CryoSat-2, launched in 2010, is a satellite altimeter that provides coverage up to 88°N with much better spatial resolution than before. Several studies have utilized this source to study the sea level variability of the Arctic (Kwok and Morison, 2016, 2017; Armitage et al., 2018a, 2018b; Rose et al., 2019; Raj et al., 2020). However, constructing precise altimeter derived sea level data in the Arctic Ocean is still a challenge, one of them is the effect of melt ponds during summer on the waveforms which dominate the reflected signal. A better understanding of the radar altimeter response over the different ice types must be gained to improve the quantity and quality of the range retrievals in the Arctic Ocean. One of the ongoing efforts is the currently ongoing CRYO-TEMPO project, funded by the European Space Agency."
4. L170-179 - Why is there no DTU satellite data north of 82°N or in the Fram Strait quadrant? The various versions of the DTU mean sea surface (MSS) don't have gaps this large. These are critical regions, particularly the Eastern Arctic, where we think in situ observations may be lacking.
   The northward extent of the state-of-the-art altimeter SSH data developed under the ESA CCI SLBC project is 82.5N. The 'polar gap' is still a major issue. There are ongoing projects (ESA funded CRYO-TEMPO) which is trying to address this issue.

5. L180-189 - It seems like a comparison with the cyclonic mode of Morison et al. (2021) would be appropriate here. For 2010-2019, their results suggest a brief initial shift out of the cyclonic mode in response to a record low winter AO index in 2010 that tended to return freshwater to the Russian side of the Arctic Ocean. This was followed by a return to the cyclonic mode for most of the decade tending to shift freshwater once again out of the East longitudes and into the Beaufort Sea. The mix of circulation regimes would make it hard to draw a general conclusion about FWC change in the 2010s, especially not having regular repeat in situ observations well distributed across the Arctic Ocean.
   We include a brief discussion of these results as a response to your comment #12.
6. L199-201 - Yes, I agree "a good part of the difference after 2009 may be due to the contribution by the rest of the basin outside the Beaufort Gyre."
   In situ observations outside the Beaufort Gyre have become increasingly rare.
   In a review it would be impactful to illustrate this with a figure showing the locations of the hydrographic stations used in the analysis.
   We have added a map of station locations used for the objective analysis to Figure 3. A subset of this map can be found in the supplement to Rabe et al. (2014).
7. For the original research part of this paper, a good way to combine remote sensing and in situ observations is to first compare them say for FWC (or for altimetry derived DOT and in situ derived dynamic heights [Morison et al., 2018; Morison et al., 2012]) at the locations and times of the in-situ observations. Then after making reasonable corrections, apply the remote sensing FWC to the whole Arctic Ocean and for other times with some confidence that these remote sensing results represent a good proxy for in situ observations [Morison et al., 2018; Morison et al., 2012]. The authors could do this by combining the data in Table 2 and the DTU-derived FWC. It would also be illuminating and relatively easy to compare DTU-derived FWC to the FWC of the ORAs and see how the regions of good and bad comparison relate to the distribution of in situ observations.
   We appreciate this suggestion and it would be a very interesting and insightful study but have decided it is beyond the scope of this review paper.
8. Table 2 - Why are there no NABOS data? Except for perhaps the Polarstern data, there isn't much data at all along the Russian margins of the Basin where Rabe et al. (2011) and Morison et al (2012) found freshwater decrease offsetting the Beaufort Gyre increases in the 2000s. Again, it would be helpful to show charts of the data locations to illustrate if there is a geographic sampling bias.
   We apologize for the omission and thank the reviewer for pointing this out. NABOS data are included up to 2013; the NABOS 2015 data is not included in the analysis. We have augmented Table 2 to that effect. All other data is listed in Rabe et al. (2014). As we do not want to duplicate information in a publication, we only include a comprehensive source list in Table 2.
9. L260 - Figure 4 is an analysis by the authors, not Thompson and Wallace, correct? In Figure 4, the positive correlation after 2010 is clear but the negative correlation before is not so obvious.
   "Figure 4" has been removed from the sentence to prevent misunderstanding.
10. L383 - The Hill et al. reference is missing.
    The reference has been added to the paper:

Hill, E. A., Carr, J. R., Stokes, C. R., and Gudmundsson, G. H.: Dynamic changes in outlet glaciers in northern Greenland from 1948 to 2015, The Cryosphere, 12, 3243–3263, https://doi.org/10.5194/tc-12-3243-2018, 2018.

11. L385 - The glaciers drain into the shelf regions around Greenland and are carried south, so their contribution to Arctic Basin is undoubtedly very small. It is hard to see the point in considering Greenland melt in the freshwater balance of the Arctic Ocean, even more so considering the complete lack of FWC data shown for the region around Northern Greenland.
The freshwater flux from Greenland has been estimated by models and satellite derived estimates of ice velocity but it is true that the flux is small into the Arctic Ocean compared to other contributions. However, this flux has been increasing and will likely increase further in coming decades. We have made this point more explicit in the text and we have thoroughly revised the section to make it more concise. We have also renamed the section and included figures for other glaciers in the central Arctic to set it in context.
12. L424 and Redistribution of Arctic Freshwater section generally - The results of Morison et al. (2021) provide a longer-term perspective on the effect on freshwater distribution of the cyclonic mode of circulation epitomized in the changes of the early 90s (Morison et al., 2000) and 2007-2008 (Morison et al., 2012). The cyclonic mode is characterized as the first EOF of ocean surface height variability (dynamic heights, 1959-1989 and satellite DOT, 2004-2019). In the cyclonic phase, surface depression, reduced freshwater content, and cyclonic circulation are imposed on the whole Russian side of the Arctic Basin. This offsets the gains in freshwater in the Beaufort Gyre. The cyclonic mode is related to the AO index with order 1-year latency, and has become more prominent under enhanced winter AO since 1990.
These comments have been added to the end of the first paragraph of Section 3.
We agree on the comment that the cyclonic mode is also important in additional to the conventionally emphasized Beaufort Gyre. We have therefore added the text following the reviewer's comment in the frist paragraph in the section of Redistribution of Arctic Freshwater.
13. L464-466 - Dewey et al. (2018) indicate the Ekman pumping and ice drag feedback mechanism stabilize ice and ocean velocity at time scales of less than a week while eddy propagation feedback has time scales measured in years.
Thank you for this comment. This sentence has been added to the text.

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