The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-247-AC1, 2019


The Cryosphere Discussions



# *Interactive comment on* "Arctic freshwater fluxes: sources, tracer budgets and inconsistencies" *by* Alexander Forryan et al.

**Alexander Forryan et al.**

af1c10@soton.ac.uk

Received and published: 6 June 2019

Tom Armitage (TA) and Wilken-Jon von Appen (WA) provided reviews of our manuscript, and also Paul Dodd (PD) wrote an interactive ("short") comment. These three will be well aware that not all reviews are helpful. In the present case, all three caused us to examine certain matters more closely, with the result that our Section 4 (Discussion and Summary) has been overhauled and is now quite different, and there are major edits elsewhere. We also became conscious that aspects of our use of language were opaque in some cases, in particular around: ice-modified waters, where we now simplify to "sea ice melt water" and "brine"; the distinction between what we now call "oceanic water", meaning the complex of all components, distinct from ocean (Atlantic / Pacific) seawater sources, which we now call "seawater"; and the terminol-



Printer-friendly version

Discussion paper

ogy around "freshwater fluxes" and also around methods is now made explicit. The relevant material appears in Sections 1 and 2, and specific instances are detailed below. We believe that the manuscript is significantly improved, and we express our sincere thanks. We reply to specific comments (in italic font) below, after first replying to a general point.

Regarding choice of journal

We selected The Cryosphere for a number of reasons. On The Cryosphere's home page (https://www.the-cryosphere.net), it is stated that the journal is "dedicated to ... all aspects of frozen water". Furthermore, The Cryosphere remit includes publishing articles "including studies of the interaction of the cryosphere with the rest of the climate system". The Cryosphere is about more than ice – it is about the cryosphere, which is what has made it a distinctive and excellent vehicle for the publication of articles about the cryosphere since its foundation over a decade ago. As a relatively early example of a paper that took an approach analogous to ours, we cite Serreze et al. (2009, https://doi.org/10.5194/tc-3-11-2009), entitled “The Emergence of Surface-Based Arctic Amplification.”, and which is "about" the Arctic atmosphere, while treating sea ice as an essential component of the Arctic climate system. This article has been cited over 400 times since its publication. As another instance, we note the publication by TA and one of us (SB) of an article in The Cryosphere in 2017 entitled "Arctic Ocean surface geostrophic circulation 2003–2014" (https://doi.org/10.5194/tc-11-1767-2017), which is mainly "about" the (liquid) Arctic Ocean.

In the present case of our manuscript, sea ice is a fundamental component of our analysis. It appears per se as a key element of the net freshwater budget of the Arctic ice and ocean system, and without the impression made by sea ice processes on the oceanic concentrations of oxygen isotopes, our analysis would not function. We believe that by presenting our work through The Cryosphere, our article will directly reach the audience that can best appreciate it.

Reviewer 1 (TA)

P2L2 and p23L11 – “traditionally divergent” to me implies that the divergence is somehow inevitable, or done on purpose historically. Maybe use “generally divergent” instead.

Done

P2L3 – split the sentence: “...reconcile. The...”

Done

P3L4-10 – I’m not sure the discussion of mid-latitude linkages and AMOC disruption by Arctic FW are really warranted here. Also, my (admittedly limited) understanding of both of these phenomena is that they are highly contentious, and an accurate mention of them would have to also say that some researchers claim there is no evidence that they are occurring or will occur.

These references are included as part of the introduction to inform the reader of the potential wider impact of changes in the Arctic. Both the relevant statements have caveats (underlined below):

P3 L12 Changes in the Arctic heat budget may affect the strength ...

and

P3 L16 Arctic freshwater export also has the potential to change ...

These statements are uncontentious and we prefer to leave them unchanged.

P8L5-7 – could you give some indication of the uncertainty associated with the optimal interpolation of the geochemical data?

The section covering optimal interpolation has now been revised.

P8 L22 The $\delta$18O and nutrient data were optimally interpolated (Roemmich, 1983) vertically in pressure and horizontally in distance to match the TB12 model domain (Fig.

2). The interpolation recovers the measurements for each sample point and interpolates between values to fill the unsampled areas of the domain. The resulting nutrient fields show typical features, including low concentrations in the upper, sunlit layers as a consequence of nutrient utilisation during primary production, and concentrations that increase with depth due to remineralisation and/or dissolution of sinking particles; see also Torres-Valdés et al. (2013). The $\delta$ 18 O sample resolution is mainly adequate to capture the significant Arctic Ocean features, although in the Fram Strait section around 6 âŮę W, there is only a single station to represent the East Greenland Current, so that horizontal gradients to either side of this station will only be approximate, therefore.

P9L5 – What is a Redfield nutrient ratio? Certainly my lack of knowledge, but I'm probably fairly representative of the Cryosphere audience...

A fair point. A gloss on the Redfield ratio is now included:

P5 L27 Concentrations of dissolved inorganic nutrients in seawater and the elemental composition of phytoplankton populations are observed to occur at broadly the same stoichiometric rations (Redfield et al., 1963). Where nutrient availability does not limit phytoplankton growth, this indicates that the ratio of the uptake of nutrients (the ratio of nitrate to phosphate, in this case) by phytoplankton, known as the "Redfield ratio", is fixed. In the Arctic context, this implies that deviations from typical Redfield ratios of seawater concentrations of these inorganic nutrients may serve as tracers of the geographic origin of seawaters, which would be useful to understand seawater pathways through the Arctic Ocean.

Figure 2 – (caption) I think the gateways are shown anticlockwise from Davis i.e., Davis, Fram, BSO, Bering? I think you should write on all of these Figures (2 and 5-10) which opening is which, for clarity and ease of interpretation.

All the relevant figures have been updated to include labels on the gateways.

P11L4-6 and Figure 3a/b – the fits to the green points (Fram) are poor, or rather the data are clearly not linear, which you attribute to the presence of Greenland Ice Sheet meltwater. Is there a way to exclude the Greenland water masses from your data in order to improve the fits? The linear regression to the green points in fig 3A especially is clearly not suitable and shouldn't be used for further analysis.

The linear fit lines originally plotted on the inset to Figure 3a were intended to give an indication of how closely these data conform to the mixing lines of the plotted end-points, and to demonstrate that the data used are representative, meaning that the calculated end point fell within both the range of the literature end-point values and within the end-point distribution used for the Monte-Carlo simulations. The deviations exhibited by the Fram section are explained in the text (as noted). The linear regression values are not used, outside this, in any analysis. To avoid confusion, the regression lines have now been removed from the figure. Figure 3 caption now reads:

P41 Panel a: Salinity – $\delta$18O relationship for all samples used in this manuscript; mean literature end-points ($\pm$ standard deviation) are marked. Red crosses indicate the mean values of literature end-points and black dashed lines the mixing lines between them. Panel b: Nutrient data for all samples used in this manuscript compared to the published N:P relationships of Jones et al. (2008), Dodd et al. (2012), Sutherland et al. (2009). The dashed red line indicates a best fit to the Bering Strait nutrient data presented here. Symbols denoting the data from each section are common to both panels. Note Dodd et al. (2012) uses the same Pacific relationship as Jones et al. (2008).

P13L25 and Figure 6 – is flux positive in or out/positive or negative? Say explicitly early on for ease of interpretation.

The statements have now been clarified as suggested. Figures 5 and 6, Tables 4–13, Positive values indicate flux into the Arctic.

P14L1 – "positive fluxes indicating an export of high-salinity waters", is this equivalent to an import of freshwater? Seems more intuitive to talk about freshwater fluxes as that's the main focus of the paper.

While what you say is true, we frame this point in terms of the physical process (export of high-salinity waters) and not in terms of the impact on the freshwater flux calculation, because we are describing fluxes of source fractions. For this reason, we prefer to leave the text unchanged.

P14L11-17 and Tables 3-8 – I find reading data off tables pretty unhelpful in general, but especially when we are trying to compare data between different tables as here. I think you could easily summarise the budgets presented in tables 3-8 in one figure with multiple panels, or in a couple of separate figures. Personally I would use bar charts with error bars, and you could also include the Fram Strait break down as 'sub-bars' of the Fram bar. Would highly recommend this as it would make interpretation/comparison between the model runs much easier

We have a lot of sympathy with the sentiment because there is no doubt that tabulation is cumbersome. However, bar charts are much worse because the range of volume flux values is so large – three orders of magnitude – so that small but important freshwater fluxes (of some tens of mSv) become invisible. Therefore we leave the tables as they are, but this comment did prompt us to expand a little on the explanation of the approach and method in Section 2.3, to illustrate the origins of this range in various physical processes apparent in (for example) Figure 3, panels (a) and (b).

P14L27 – the ice-modified water in the WSC is from recirculation, right? State this here

There are several $\delta$18O samples in this area that lead to the apparent presence of ice modified water (brine). Closer examination of our results and also of source water properties (Frew reference below) led us firstly to make a new run of the 3EM model, and secondly to revise our reasoning and text. In the new Section 4.1, we write as follows.

P20 L 1 Section 4.1 The models generate apparent brine imports in the WSC and the Barents Sea Opening, both of magnitude $\sim$ 45 mSv, a total of $\sim$ 90 mSv with a large relative uncertainty of $\sim$ 50 mSv. If correct, this is a substantial component of the Arctic Ocean freshwater budget. These (apparent) fluxes are too small to be visible on Fig. 5, but for scale, note that each new (oceanic water) inflow is $\sim$ 3 Sv, 1 % of which is 30 mSv. These brine fluxes are consequences of weakly positive $\delta$18O anomalies centred around $\sim$ 300 m depth in both locations, each about 200 m thick and each spanning $\sim$ 200 km. The presence of these features in both Fram Strait and the Barents Sea Opening suggests that they are source water (Atlantic seawater) properties and not the result of modifications by local processes. Frew et al. (2000) examine the oxygen isotope composition of northern North Atlantic water masses from measurements made in 1991. Considering the waters of interest here – the upper $\sim$ 500 m in the eastern North Atlantic (their stations 10, 24, 26, 72) – we find (broadly) salinities and $\delta$18O values in the ranges 35.0 – 35.2 and 0.2 – 0.4 ‰(their Fig. 2). This combination and range describes the part of the dense cloud of points heading a short distance "north-eastwards" in phase space away from the seawater endpoint (Fig. 3 panel a inset). A consistent interpretation of the apparent WSC and Barents Sea Opening brine imports, therefore, is that they are actually manifestations not of local processes but rather of source water variability, in the light of our salinity (34.662) and $\delta$18O (mean 0.2 ‰ endpoints.

P15L26 – “large uncertainty”, the uncertainty is actually smaller than for the 3EM/4EM models, the relative uncertainty is larger though if that’s what you mean.

The statement has been revised - what we meant was:

P17 L12 smaller with a large relative uncertainty

Section 4.1 – Is the apparent consistency between the 3EM and 4EM models a surprise given that the difference between them is just the use of the geochemical data to partition the seawater into Atlantic/Pacific fractions? In Figures 11, 12, and 13 I can



Printer-friendly version

Discussion paper

see no difference between the 3EM/4EM fluxes. In other words, are the 3EM/4EM fluxes consistent just by construction? If so you should say this, as it is misleading to say they are “consistent” when they are simply the same by construction. Perhaps my misunderstanding.

The difference between 3EM and 4EM models is the use of inorganic nutrients to attempt to discriminate between seawater of Atlantic and Pacific origins, which we now state as such (P16 L19).

P22L8-12 – while you cannot ascertain exactly the source of this water transformation, could you speculate at all? At least on the classes of processes that might cause this?

Torres-Valdes et al. (2016) tested one hypothesis regarding the possible role of dissolved organic nutrients, only to eliminate that option. Their final section comprises a logical conspectus of research avenues to pursue to resolve the problem. We do not wish to repeat that text, but we have written a new Section 4.2 on "Pacific" water. The new text begins by devoping a hypothesis around denitrification (a new paper Matthew Alkire was very useful) as below, and continues, using the suggestion by WA (see below) to look at recently-published material on more exotic tracer species, which supports our development of the hypothesis into dense water formation in the Barents Sea. Section 4.2 begins as below; refer to the manuscript for the full text.

P 23 L1 A credible hypothesis to explain all these observations – the doubling of Pacific export over import, the transformation of Atlantic water, and the deep presence of Pacific water – concerns denitrification, the process that occurs in ocean sediments and removes nitrate from the ecosystem by discharging N2 . Chang and Devol (2009) estimate a net pan-Arctic denitrification rate of $\sim$ 13 Tg-N yr$-1$ , with much of that expected to occur in the shallow waters of the Barents and Chukchi Seas (6 and 3 Tg-N yr$-1$ respectively). They further note the likelihood that the process is a consequence of sea ice retreat enabling increased primary production through increased shelf-break upwelling, which delivers nutrient-rich waters to upper-ocean waters with greater light availability; the resulting increase in export production then fuels higher rates of sedimentary denitrification.

Section 4.3 – I was wondering if you could also provide a paragraph with some perspectives on 1) future research using these methods/datasets, and 2) implications for Arctic Ocean climate monitoring in terms of observation systems and optimal approaches at analysis/modelling.

We have re-written Section 4.4 Perspectives, which now concludes as follows.

P26 L23 We envisage that sustained measurement of suitable tracers around the Arctic boundary has the potential to further our quantification and understanding of key processes, variability and timescales and to help mitigate the scarcity of observations in the Arctic Ocean interior. More (and more reliable) tracers are needed, more observations of more "traditional" tracers are needed through the water column (from surface to sea bed), more of those observations are needed in seasons outside summer-autumn, and we need better understanding of Arctic Ocean biogeochemical processes.

---

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-247, 2019.