The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-247-AC2, 2019

The Cryosphere Discussions

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

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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 terminology

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.

Reviewer 2 (WA)

p2l7 "(liquid) freshwater fluxes" and p2l23 "freshwater" Please give a clear definition of what you mean by freshwater. Is this H2O? At this point there are too many different (sometimes meaningful) definitions in the literature that you cannot assume the readers know exactly which one you are using here.

We agree with the reviewer's sentiment here. Freshwater flux is now defined in the introduction.

P3 L 20 We define a flux of freshwater to mean the rate of addition of pure water to (or its removal from) the ocean surface, by exchanges with the atmosphere (evaporation [E] and precipi- tation [P]) and by input from the land (runoff [R]). The total ocean surface freshwater flux F is then F = P − E + R .

p4l12-14 This equation appears to only hold for 1 constant salinity at the inflow and another constant salinity at the outflow from the box. For the Arctic Ocean, that is clearly not given. To me it is not clear from this manuscript or from Bacon et al 2015 whether Sbar is an area mean or a transport weighted mean salinity over the boundary. I would appreciate it if the authors could clarify this here.

Our original intention was to avoid over-complication, but again, we agree with the reviewer's sentiment, so as part of the new text in response to the preceding comment, we have revised section 2.4 accordingly :

P14 L 8 where the integral is taken around the ocean boundary, from seabed to surface, and including sea ice; the overbar indicates area mean and prime indicates deviation from the mean (and following text).

p5l2-3 and l6-7 “accurate estimates of freshwater flux require the definition of an appropriate reference salinity (Sbar)” and “the boundary-mean salinity is the only appropriate reference salinity” I do not think that either of these sentences is correct. But rather than arguing over whether they are correct, I would suggest to leave them out as they are in fact not crucial to anything that follows later in the manuscript.

We thought about this, and yes, we agree. Material discussing appropriate "reference salinities" has been removed. The text now appears as below.

P4 L14 The second way to estimate F is what Aagaard and Carmack (1989) call the “indirect” approach, which we call the “budget” approach. The budget approach recognises that ocean salinity is sensitive to dilution (or concentration) by addition (or removal) of freshwater. Therefore with knowledge of fields of velocity and salinity around the boundary of a closed volume (to ensure conservation of mass), the surface freshwater flux within the volume may be calculated; see (Serreze et al., 2006; Dickson et al., 2007; Bacon et al., 2015).

p5l29 Nd isotopes and REEs have also been used as conservative tracers of different rivers in the Arctic Ocean, e.g. doi:10.1016/j.gca.2016.12.028

An extremely interesting and useful pointer to material of which we were unaware; thank you. The discussion (Section 4) of the manuscript has been expanded to include the use of exotic tracers:

P26 L10 Nevertheless, other, more exotic species, may prove useful. For instance, Laukert et al. (2017) show that the distri bution of neodymium isotopes in Fram Strait that bears a considerable resemblance to our “Pacific” water distribution (our Fig. 9, their Fig. 3), and with a similar interpretion to ours (Section 4.2 above) as to the provenance of the water mass. Furthermore, Wefing et al. (2019) analyse isotopes of iodine and uranium, sourced from UK and French nuclear reprocessing plants, which trace Arctic Ocean circulation pathways and residence times, showing that some fraction of the near-surface freshened oceanic waters in the west of Fram Strait, which appear

to be of Pacific origin from the N:P analysis, may actually have originated from the Norwegian Coastal Current

p6l14-17 Again, while this is an appropriate step to take at this point (and better results might not be obtained from data at this point), it should still be pointed out that this is not perfect and there are in fact possibly large systematic errors arising from the sampling locations and/or spatially (potentially) insufficient sampling. It would be nice to mention these points with at least a few sentences.

We have deleted this text as part of our overhaul.

p7l8 The correct statement would be that this “conserves volume and salt transports”, not that it “conserves volume and salinity transports”!

No, this is wrong. The measured property that is transported is called salinity. "Salt" in this context is a colloquialism, however commonly it may be used. If you want to check on the history and background, then Bacon et al. (JAOT 2007, 10.1175/JTECH2081.1) give a reasonable overview in the first two sections, and that paper is usefully supplemented by McDougall (Ocean Sci. 2012, 10.5194/os-8-1123-2012) on ’absolute salinity’.

p7l14 Please say what you mean by the plus/minus here. E.g. it could be standard deviation or standard error.

How we define the +/- has now been included :

p7 L21 ($\pm$ standard deviation)

p7l15 “1.0 +- 0.2” not “1 +- 0.2”

p7l18 “Sv” not “sV”

Both errors have now been corrected as indicated.

p7l24 Please state where your information on sea ice export is from, e.g. satellite observations of sea ice drift and sea ice volume?

The terms reported here are outputs from the TB12 model. How the sea ice flux was initialised in the model is detailed in TB12 (see para. 39), but in brief, they used remote-sensed area flux (due to Ron Kwok) in combination with thickness flux (due to Edmond Hansen).

We edit our text to:

P7 L29 The net surface freshwater flux (both liquid and solid) calculated by TB12 is 187 $\pm$ 44 mSv, manifest as 147 $\pm$ 42 mSv in the liquid ocean plus 40 $\pm$ 14 mSv in sea ice.

p8l5 “nutrient and delta18O data were optimally interpolated” Comment on whether the spatial distribution of the data was sufficient or whether there could be interpolation issues.

Reviewer 1 asked a similar question and we refer to our reply above.

p8l9 “grid cells as hydrographic stations” I see where this is coming from, but it is still a strange way to formulate it.

The statement has now been clarified:

P8 L16 Our domain comprises a total of 147 hydrographic stations, which includes data from 16 general circulation model grid cells in the Barents Sea Opening that are used as hydrographic stations, covering a total oceanic distance of 1803 km, with a total (vertical) section area of 1050 km2.

p8l15-27 For me this was totally incomprehensible upon first reading. The terms “3EM”, “4EM” and “4EM+” are not self explanatory. I would strongly advise to make a diagram or a table. A suggestion would be a table like this (columns could not be formatted in plain text, so the individual lines of the table are grouped together): Model name Constraints End members that are solved for Comments new line 3EM Volume con-

servation, salinity data, delta18O data Seawater fraction, meteoric water fraction, ice melt water fraction Seawater is water with S=35 irrespective of whether it enters from the Atlantic or the Pacific new line 4EM Volume conservation, salinity data, delta18O data, P* data Atlantic water fraction, Pacific water fraction, meteoric water fraction, ice melt water fraction Atlantic water and Pacific water are defined to have identical S and delta18O end member characteristics, but different P* new line 4EM+ Volume conservation, salinity data, delta18O data, P* data Atlantic water fraction, Pacific water fraction, meteoric water fraction, ice melt water fraction Atlantic water and Pacific water are defined to have different P* and similar, but not identical S and delta18O end member characteristics

This is a sensible suggestion, so we have added a new Table 1, appropriately referenced in the manuscript text, as a compact display of the three model schema.

I would also already add a sentence like the following one here, because (anyways for me) it was not clear why you do these two versions with 4EM and 4EM+: “4EM+ is degenerate (meaning that numerical values are strongly affected by small perturbations) because the distinct source salinity and delta18O values of Pacific water are on a mixing line between the meteoric and Atlantic Water end member quantities.

We include the 4EM+ schema as despite it being degenerate, this represents what is becoming common practice in geochemical tracer studies as noted in the text :

P9 L28 Thirdly the 4EM scheme is applied again, but now adopting distinct end-member properties for both ocean-source salinity and δ 18 O (4EM+), replicating previous practice (Dodd et al., 2012; Jones et al., 2008; Sutherland et al., 2009). The properties of the three schemes are summarised in Table 1.

Comments about the degeneracy of the 4EM+ scheme are made in the discussion (see point below).

p9l14 1 sentence here why you use Pest: in order to judge the method, not for use in the method itself

Pest (now P_oce) is used to calculate end-member values of P*. The text has now been updated to make this clearer:

P10 L17 where P_oce is the estimated concentrations of phosphate from the relevant ocean (either Atlantic and Pacific) waters and the subscripts slope and int indicate the slope and intercept of the relationships.

p10l5 Again, I think this would be much clearer if you could refer to the table as I suggested above.

A reference to the table is now included.

p11l14 refer to Table 2 in this sentence

A reference to the table is now included.

p12l16 Again, area mean or transport weighted mean?

This has now been clarified in the text:

P14 L5 Seawater salinity for 3EM and 4EM models is fixed at the boundary area mean salinity for the TB12 model (34.662).

p14l1 m/s (see my comment below on Figure 6)

The units of Sv are correct - in all cases we take a volume flux from the TB12 model, which is calculated as a velocity (v) * grid cell area (horizontal distance ds x vertical distance dz) and scale it by the estimated water type fraction.

p14l11 It might be helpful to remind the reader that your +- values stem from the Monte Carlo simulations.

A reminder has been now included in the text :

P16 L2 For the 3EM model schemes, the net seawater volume flux is effectively zero

(0.002 $\pm$ 0.006 Sv, Table 4, Monte Carlo uncertainty quantification).

p15l2 Also, here 1 sentence would be in order repeating what the difference between 4EM and 4EM+ is and why you do both calculations.

We have revised the text as suggested :

P16 L11 The 4EM scheme extends the 3EM scheme through use of inorganic nutrient (nitrate and phosphate) data, aiming to discriminate between Atlantic and Pacific seawater origin. The 4EM scheme retains single end-points for salinity and δ18O, as in 3EM. In the 4EM+ scheme, distinct salinity and δ18O end-member properties are attributed to Atlantic and Pacific seawaters, replicating previous practice (Dodd et al., 2012; Jones et al., 2008; Sutherland et al., 2009).

p15l22-23 and l25-26 Please don't just show both sets of numbers, but also comment on which one you think makes more sense.

This text is no longer in the manuscript.

p16l16 Add a sentence such as: "Both of these numbers should be approximately 0 and therefore, we consider this a model/methodological/data(?) mistake for the following reasons. . ." and p16l21 You are only looking at data from 1 summer month. Discuss whether all of this should be balanced in the quasi-synoptic view of the data you use.

This text is no longer in the manuscript.

p17l16-18 Neither of these views seems plausible for the West Spitsbergen Current. Should the Atlantic water salinity not rather match the WSC closely?

We have added a new and detailed discussion of the WSC attribution to the manuscript, in Section 4.1 on ice-modified waters, starting as below.

P20 L1 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 δ18O 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 ng choice of journaland 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 δ18O values in the ranges 35.0 – 35.2 and 0.2 – 0.4 ‰ respectively (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 δ18O (mean 0.2 ‰ endpoints.

p18l14-15 Should this not be considered everywhere?

This text is no longer in the manuscript.

p19l2 “solid (sea ice) fraction” instead of “solid, sea ice, fraction”

The discussion has been substantially updated and these lines are no longer present.

p19l27 “(at least when considering full depth assessments)” It is not clear why that caveat is necessary and why the sentence is not correct without the added information in brackets.

This text is no longer in the manuscript.

p20l6 How can a river be a sink? Processes on the continental shelf near the river

could be sink processes.

This text is no longer in the manuscript. The discussion of nutrients in Section 4.2 is substantially re-cast.

p20l20 no “:”

This text is no longer in the manuscript.

p20l25 Explain how I would see that from Figure 3 and what degenerate means in that context.

We discuss degeneracy now at the end of Section 4.2 (P24 L10), in the new Section 4.3 (P25 L1-13), and Section 4.4 (P26 L4-7).

p21l1 “boundary mean salinity” Again, where do I “see” that in Figure 3?

This text is no longer in the manuscript.

p21l12-24 This text and the associated Figures 11-13 should in my opinion be removed from the manuscript as it is unclear what you mean by “oceanic origin freshwater”. Additionally, there is no insightful information contained in them.

We retain Figure 11, since it is a useful visualisation, but otherwise, yes, you are right, so we have removed Figures 12 & 13. The "oceanic freshwater" comment was removed as part of our clean-up of terminology mentioned in our introductory comments to these replies.

p22l17 “Carmack et al. (2016, Appendix)”

This text is no longer in the manuscript.

p23l6 “salt conservation”!

This text is no longer in the manuscript.

Tab1 Why is there a larger line break after the first line of delta18O and salinity?

Table format has been corrected.

Tab1 2nd line under ice melt: What is "surf"?

The table has been updated and the caption expanded to clarify this point:

Table 2 caption End-member values for salinity and δ18O (‰ from the literature. Note Bauch et al. (1995) calculate ice melt δ18O by multiplying measured surface seawater δ18O (surf) by a "fractionation factor" of 1.0021 .

Tab3 Similar to p7l24, where is the information about -0.040Sv solid ice melt from?

The caption has now been updated to make this clear :

Table 4 caption Values of solid freshwater flux from Tsubouchi et al. (2012).

Fig2 I think the other piece of interpolated data that your study is based on is cross-sectional velocity. I would recommend to add this as a top (4th) panel to Fig2. In that case the reader does not need to refer back to TB12 to get that information

We haven't done this because it would be the same as the volume transport plot (panel d), apart from scale.

Fig2 caption l1 "P*" should be with a superscripted "*"

Now corrected.

Fig2 caption l4 Repeat what the main Arctic water masses are so that the reader does not need to refer back to TB12. and Fig2 caption Add: "Note the broken scaling of the y-axis."

Definitions of the Arctic water masses from TB12 have now been included in the figure caption as has the comment about the y-axis scale.

Fig 2 Caption Sections of δ18O (panel a), salinity (panel b), P* (panel c) and volume flux from Tsubouchi et al. (2012)(panel d) after optimal interpolation onto the Tsubouchi et al. (2012) CTD station positions, clockwise around the four gateways from Davis to

Bering Straits. Solid black lines indicate the potential density (σ) surfaces separating the main Arctic water masses grouped as follows, surface water (σ0< 26.0), subsurface water (26.0 < σ0 < 27.1), upper Atlantic water (27.1 < σ0 < 27.5), Atlantic water (σ0=27.5 to σ0.5=30.28), intermediate water (σ0.5=30.28 to σ1=32.75), and deep water (σ1 > 32.75); definitions from Tsubouchi et al. (2012). Note the broken scaling of the y-axis.

Fig3 Your 3EM model solves the classical end member decomposition in the triangle that is drawn in panel a. Your 4EM models essentially are the same, only that they solve the end member decomposition in the tetrahedron that would result if you were to extend panel a in the vertical with the vertical axis being P*. Since you can't add a 3 dimensional figure to the paper, I would recommend to at least add plane views of this tetrahedron with the data and dashed lines plotted into the panels just as you are doing in panel a right now. Common axes can be aligned with each other. My suggestion: 4 panel figure. top left panel as your panel a. top right panel x-axis P* y-axis delta18O, bottom left panel x-axis salinity y-axis P*, bottom right panel your current panel b. Also please substitute the current legend in panel b by a legend for the dashed lines and comment in the figure caption that all symbols and lines are the same in all panels. The 18 in the ylabel of panel a should be superscripted not subscripted. and Fig3 caption l4 "Dashed thick"

We think it would over-complicate to attempt to graphically reproduce in 2 dimensions a 3-dimensional phase space; we have made the other corrections have now been made as indicated.

Fig 3 caption Panel a: Salinity - δ 18 O 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).

Your units in Figs 6/8/10 and 11a/b are wrong. They should be "Sv/m/km" or more conventionally "m/s". Note that you only arrive at units of transport (Sv) after integrating the data in the figures in the horizontal and vertical dimensions. Same applies for Figs 12/13 where your units should be mËĘ2/s or Sv/km or similar.

We apologise sincerely for an error here (application of mistaken scaling). What we should have plotted was indeed volume transports (gridded v x area), but the units should have been mSv and the range more like $\pm$20 mSv. This has been corrected.

Fig7 What is plotted in panels 7a and 7b is different from what is plotted in panels 5a and 5b, yet the values in the Met. and Ice Melt columns of Tables 3 and 5 are identical. In my opinion, only either one of those can be correct.

This was a problem with contour levels in the figures. The tables are, in all cases, definitive. All figures have all been re-contoured (all corresponding panels use the same level boundaries for ease of comparison).

Fig11/12/13 can in my opinion be deleted from the manuscript. One of the reasons is that I do not understand what the black line in Fig12/13 is supposed to be.

As noted above, we largely agree.

The legend has now been updated as suggested.

p48l14-16 A correct reference to this data publisher contains the complete DOI and it is not a tech report. Compare how the citation is provided on the webpage of the data set. In addition, you have the wrong title which means that it took me some time to find the data set you are referring to! "Kattner, Gerhard (2011): Inorganic nutrients measured on water bottle samples during POLARSTERN cruise ARK-XXI/1. PANGAEA, https://doi.org/10.1594/PANGAEA.761684"

Thank you very much for the updated reference. This has now been added.

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