

## ***Interactive comment on “Export of Arctic freshwater components through the Fram Strait 1998–2010” by B. Rabe et al.***

**L. de Steur (Referee)**

[laura.de.steur@nioz.nl](mailto:laura.de.steur@nioz.nl)

Received and published: 12 October 2012

General:

This paper presents and discusses the fluxes of the three main freshwater (FW) components observed in Fram Strait along the hydrographic section and mooring array at 78°50'N / 79°N based on late summer data for 5 different years between 1998 and 2010. This is a very relevant topic regarding the ongoing large changes, such as increased FW storage and changes in FW pathways, which have been observed in the Arctic Ocean during the last decade or so. The paper is a little hard to read and will benefit from adding more details of how the transports are calculated (e.g. how sensitive they are to including either/and/or vmADCP or IADCP or mooring data since not the same type or amount of data is used each year) and in general on the results. The

C1141

discussion of the results is rather meager compared to the final discussion on impacts of Arctic circulation. I find some figures and accompanying text sometimes a little hard to follow, usually because the text is rather short. Also, the inclusion of more details on the errors related with the final flux estimates is recommendable. The paper finally discusses the different possibilities of changes in upstream conditions with respect to the observed variations in the FW flux components, which is a real good attempt to interpret the variability. I realize that is the best one can do at this stage with this data but it should at least be mentioned or addressed in the discussion, how representative or not they are for a whole year since these are based only on hydrographic data which are taken once a year (September). At least with respect to variations in fluxes (which are known, kind of, based on the mooring data). In addition, at this stage the paper unfortunately suffers from the fact that the main results of the analysis and estimate of the different FW components (that is, Pacific Water, sea-ice melt, and meteoric water) has been written up in a paper which is still under review elsewhere and hence actually not referable to. Perhaps this is different by now (?), or will be hopefully very soon. Otherwise, the paper should at this stage refer to it as ms in preparation or pers. comm. In general this submitted manuscript would become more readable if no direct references to figures of “Dodd et al. (2012)” are given but instead if some of those details could be presented here (table or figure wise).

\*Please note\*: On the day I submit this review I see the note that Dodd et al (2012) is accepted, that is great, congratulations. Still I think it would really help if some more details are provided or visualized in this paper for clarity.

Specific + technical Comments:

Page 2750 Line 7: Do not refer to Dodd et al (2012) in the abstract Line 9: Please give the range for the LFW estimates (around the mean of 92 mSv) or the error. Line 16: “An increase in PW transports from 2005 to 2010...” There is no trend (increase) of PW: in 2005 it was low, 2008 it was high, in 2009 it was low again, and in 2010 it was high again.. Last sentence of the abstract is too general, this is indeed raised in one

C1142

sentence at the end of the ms but it is not further explored in the paper and does not really belong in the abstract to my opinion.

Page 2751 Line 12: add “subpolar” to gyre circulation Line 21: Add some references here at the end of the sentence?

Page 2752 Line 18: Rewrite “distinguishing the composition” into eg. “to distinguish components..” or “to quantify/determine the composition”

Page 2752 Line 12: FIFB vs FSIM throughout ms is confusing. And, could be different, at least in the vertical (so, do not integrate)?.....

Page 2754 Line 16: 79°N should be 78°50N for most of the years Line 18: Barotropic tide model vs mooring obs. on shelf: I believe that on the shelf the amplitude difference between the tide model and velocity measurements from shallow moorings is substantial too. Please check and include a comment or show.

With respect to the “Data and Methods section” on velocity measurements: Line 25: Are the mooring velocities detided as well? Table 3: Caption: What is the “eastern part” of which are the “eastern” moorings? Table 3: Are in 2009 both vmADCP and ADCP (which is latter? IADCP?) used? Why? And if so, what is the sensitivity of the results to that if only one of those is used, like in 2010? Or if IADCP data are not used in 2009 then remove here... Table 3: Two different ways of spelling are used for Beszczynska-Moeller/Möller (throughout the ms)

Page 2755 Line 1: It should be Aanderaa, and please spell out RCM and FSI Perhaps add a table with values of End-Members here too. Line 12: Why was the change made from 3 to 4 (for SIM?) Line 15: Why is only a subset used? And which subset then? Can a figure be included here? (not referenced to a Fig in Dodd et al.) Line 17: Please provide some more details of the tracer measurements here too, eg standard or typical sampling depths.

Page 2756 Inverse model and velocity results: this needs to be worked up, it raises a

C1143

lot of questions (with me) which may affect the final error estimates. Line 1: Why not even use a mean from the moorings for a day, ie. that day the tracers or CTD profile was taken? The vmADCP or laDCP data are snapshots too, as well as the tracer samples. Please check and provide details on how sensitive the flux results are for the choice of the period (1 month, cruise period or 1 week or a day) over which the moored velocity measurements were averaged. This should also be incorporated in the final combined flux error. In relation to this, the panels in Fig. 2 show the velocity field as far west as there are vm/IADCP measurements, but how far west do the moorings extend? And, hence it is a bit unclear what the difference is (or would be) between the flow fields when mooring data are used and when not. Line 2: Please provide more details of these data gaps for each year, perhaps in a table or so. Line 4: typo: ... did not fill the all of the FEMSECT grid... Line 4: Provide more details of this grid/cells of FEMSECT: width/height of cells, depths Line 15-20: Much more should be said about the combined and final error, and why is the combined error for PW transports (not FPW?) double the error of the inverse estimate? Line 24: Please show in the Figure 2 the location of the moorings / instrument depths. And even where the CTD stations (hence vmADCP/IADCP data) were taken for that matter, those may not have been on the same location for each year? All this could be in another figure. Line 26: There are important and older works on the EGC that should be referred to, eg. Aagaard&Coachman (1986) and Foldvik et al (1988). Same holds for line 4 on the next page.

Page 2757 Line 6: East-west movement of the EGPF can be seen too. This is quite variable, add reference. Line 20: the results on wind driven mesoscale variability hold mostly for the eastern Fram Strait (results from Jonsson et al).

Page 2758 Line 3: Ref to Dodd's figure 9: It would help to have a section with fresh-water inventories here too, just for readability. Line 6: Transport densities? I found it not so clear what it is. Could another terminology be used here? (sorry, no suggestions) Figure 2: Please mark the 10.6°W in the figures to indicate up to where fluxes are

C1144

calculated. Could the isopycnals be shown in white? They are hard to read. Can the panels of figures 2 and 3 in general be shown larger, hence better readable? Can you say something about barotropic vs baroclinic flow on the shelf or in the EGC for the periods you discuss? Since you use both the moorings and the hydrography, clearly the baroclinic flow is very variable related with the difference in isopycnals from year to year, but what about the barotropic component? Specifically since the measurements are taken in a region where recirculation is very large (which is mostly barotropic). Line 9: "...shallow LWF layers..." How shallow? Line 12: why go as far east as 4°E? The EGC is really confined to 1°W, one sees the transports hardly change between 1°W and 4°E. Figure 3 could become more clear if zoomed into the EGC. Line 15: I see R09 here for the first time, I presume it refers to Rabe et al, 2009? If introduced properly use it like that everywhere. Line 17: The different approach used here compared to R09 led to increased estimates here, by how much? Line 19: Please provide/repeat the numbers of earlier published estimates. Line 21: Is FIFB only just negative FSIM? Isn't there a difference in the vertical, eg. small positive SIM near the surface, and negative SIM below that (which is IFB). Can you provide a bit more detail on this? There can still be a SIM flux and a IFB flux separately - so not all integrated in the vertical (hence one number for FSIM)? And if one only considers FIFB = - FSIM then perhaps use just one term (so either SIM or IFB) throughout the paper. Or at least if a figure shows FSIM, also use FSIM in text (and not FIFB), and vice versa.

Page 2759 Line 1: east of 6.5°W Line 4-8: Again, Figure 3 shows FSIM, text talks about FIFB. Line 10-12: I do not understand what is discussed here, please clarify. Line 19-27: I find the term 'section inventory' confusing since in Dodd et al (2012) 'inventories' are the distribution of FW components whereas here it is about volume transports / net fluxes. Perhaps call it 'section flux' or so. For some reason I find Figure 5 and associated text real hard to understand, even after reading it several times. What is the difference between LWF component transports and section inventories? Explain or introduce the figure better and what the purpose is, discuss the results or main point more clearly or cut out stuff that is not relevant. Eg. is Fig. 5b integrated over the whole

C1145

section instead of just between 10.6°W and 4°E? And down to where (not 300m?) are results from Fig. 5a integrated to? Dates on top of Fig. 5b must be clearer. I find it also difficult to interpret the correlations based on just 5 years of data.

Page 2760 Line 9: 'respective' relation? Line 11-14: Not so surprising. Also refer to Jahn et al (2012) where the dependency of (seasonal) FW fluxes on velocity vs salinity anomaly is presented (also for the observations) Line 25: So there was also positive SIM in 2009 and 2012, if integrated from top to bottom this is not visible. Is it worthwhile mentioning this earlier? Hence, can one not distinguish a SIM flux and a IFB flux?

Page 2761 Line 5: Please make a note (here or elsewhere) that this is based on September fluxes and not year round. There is a substantial seasonal cycle in fluxes too, see De Steur et al, 2009. This could also be incorporated in discussing the uncertainty of the results. Otherwise a rough estimate could be made by looking into the seasonal variation in the upper layer flow (from the moorings). Line 17: year of Alkire et al is missing. Line 20: rewrite sentence: ..variability of the PW advection... What kind of variability, the advection or the appearance of PW in the TPD? What is, or was, the earlier estimated timescale of PW reaching Fram Strait. Line 24: I see no trend or increase in the transport of PW from 2005 to 2010: 2005=low, 2008=higher, 2009=low, 2010= higher (but surely still not as high as in 1998)

Page 2762 Line 10: Import? or release from Beaufort gyre? Line 15: 'relatively' high MW transports after 2005. Line 15: ..additional MW... How much, relative to what?

Page 2763 Line 5: Enhanced SIM in model but I find no reference to actual (observed) sea ice melt in the Arctic. Line 10-12: Why could the SIM not just be transported with TPD to Fram Strait (eg. Bauch et al (2011)), why suggest it would be released from the Beaufort Gyre? Has there been a decrease in SIM in the BG since? Line 25: increase in river discharge from 90s to '07: how much? Line 27: perhaps use mSv here instead of Sv.

Page 2765 Figure 6 needs to be explained better, are moorings used here for all years

C1146

to get FEMSECT transports? What are the differences for data used for constructing this time series? Line 10-11: Is FPW or PW meant here (or both)?

Page 2766 CAA and Denmark Strait should be separated in 2 different paragraphs  
Line 17-20: Observations (hydrography) from Davis Strait did not show (and moorings do not show this either) a evidence of a decrease in southward transport (please communicate or refer to C. Lee) but only model results had shown that (P. Holliday). Same should be changed in the summary.

Page 2768 The last point of the summary should not be included here as it is not a result of this work and more importantly, it is not been shown that a reduction in FW has occurred. Here is referred to other work (pers. comm), of which the observational portion is not been evidenced in Davis Strait and it is merely a repetition of a sentence in the discussion. Also, what about sea ice melt? Too which extent could that been contributing to a FW accumulation, or change in pathways of riverine FW (eg. Morison et al, 2012). Line 15: An important question to finalize this ms but it does not belong in the abstract.

Table 1: FSIM, FIFB, FPW freshwater 'part' of ...: Could be SIM/IFB/PW 'contribution' or 'FW equivalent'

Table 2: OD = R/V Oden is mentioned in the caption, but R/V Oden is not mentioned in the table.

Table 3: Perhaps mention which moorings are used for the velocity observations. (names or between which longitudes) in the caption. Are both vmADCP and IADCP used in 2009?

Table 4: Caption: "Observed volume transports" should this not be "Derived volume transports (from FEMSECT and observations)". Last sentence in caption is too long here, should be in text.

Figure 1: Could we not see one figure with sampling locations here too? (just a sug-

C1147

gestion)

Figure 2: See earlier comments: isopycnals not visible, where are the moorings/instruments/CTD casts?

Figure 3 + 5: Can these figures become bigger, thus better readable? Font size is awful small.

Figure 4: Can an error bar be given for the LFW transport (which are now shown by asterisks)?

Figure 5: I find it hard to understand what is in this figure, see earlier comment.

Figure 6: Caption: "Observed volume transports", are these again derived from FEMSECT (hence model +observations). It should be explained what type of data is used when for this time series.

---

Interactive comment on Ocean Sci. Discuss., 9, 2749, 2012.

C1148