**Ocean Science
Discussions**

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

Anonymous Referee #1

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General: The article "Export of Arctic freshwater components through the Fram Strait 1998-2010" by B. Rabe et al analyzed the Fram Strait freshwater transport, the origins of the freshwater and the variability of the freshwater components. Five single years of observations within a period of 13 years are analyzed. The article provides interesting results and is generally well written. At some places somewhat more discussion and description would be helpful to improve the readability of this article.

The discussion of uncertainties/errors should be extended. Although different error sources are mentioned and partly named in numbers, it remains unclear to me how large the combined error of all error sources is. A specific section dealing with uncertainties might be useful instead of spreading the error discussion at a number of different places throughout the manuscript.

Somewhat more description of the methods should be given. At least a short description and not only a citation should be given, so that a reader who is general interested in the results and the topic does get a basic understanding of what is done without reading all the referred articles.

The authors should try to avoid abbreviations for terms that are only used a very few times in the article. Furthermore, also terms like ADCP or CTD should be written out at least one time and all abbreviations should appear in the list of abbreviations (I like this list but it should be complete).

This study discusses results from 5 individual years in a period of roughly a decade. From an observational point of view, this might be a quite long period, however, the amount of data is very small to use statistical methods for analyses and particularly draw conclusions from these results regarding changes and trends. I feel that the authors should be somewhat more careful using the word trend and should rather talk of variations and variability instead of changes and trends. Although there are a lot of changes ongoing in the Arctic, not all differences that appear on decadal time scales are trends. There is a huge amount of articles showing that the Arctic is subject to large decadal scale and also multi-decadal climate variations. Thus, we can not conclude from 5 years of observations, spread over 13 years, much about changes or trends. Of course it is valid to speculate that certain observed variations fit well to expected changes in a changing Arctic climate but that this might fit together is not a prove for an actual change.

I could not yet find the manuscript by Dodd et al. (submitted to J Geophys Res Oceans), which is referred to many times in this article. Also a number of conclusions of this paper are based on figures and results by Dodd et al (submitted). These references are quite useless at this stage and for the unlikely case that the article by Dodd et al. should not be published or only after substantial changes, this would also mean that this article would require larger changes. In addition, it makes it difficult at this stage to decide how much new information this article provides. However, since most of the

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authors from Dodd et al. (submitted) are part of this article I am confident that there is sufficient additional results. However, I feel- although there are some lines on page 5 discussing the differences to Rabe et al. 2009 and Dodd et al. 2012/submitted – the differences and advantages of the different methods used in this study should be made more clear.

Specific comments: 1. Page 2, line 7: It might be a matter of taste but I would avoid citation of other studies in the abstract. My opinion is that the abstract should be understandable without reading additional literature. Thus, you should at least mention which components Dodd et al. used / analyzed.

2. Page 4, line 4: ".. in particular, the dominant pattern of large scale atmospheric variability" Do you mean that this dominant pattern is still unknown or that its influence on the Arctic Ocean is unknown. The dominant pattern is obviously the AO/NAO in the Arctic (at least it explains by far the largest variance of the Arctic atmospheric circulation): Its influence is discussed e.g. in Dickson et al. 2000. However, there are other large-scale atmospheric circulation patterns which are very important. E.g., the so called dipole anomaly as important mode for the Arctic is discussed in Wu et al. 2006 and Wang et al. 2009. There are also a number of model studies analyzing the effect of large scale atmospheric circulation on Arctic freshwater budget and export, which are worth to mention at this point (e.g. Jahn et al., 2009 and citations therein.)

3. Page 4, section 1.2: The model study by Jahn et al. (2010, J of Geophysical Research) using tracer to determine the origin of liquid freshwater exports out of the Arctic is cited in the section before but should be discussed here as well. Page 6, line 3: Please spell CTD out (CTD, conductivity, temperature, depth) and add it to the abbreviations list

4. Page 6, line 9: Please spell ADCP out and add it to the list. Also add vmADCP and lADCP to the list.

5. Page 7, line 1: Again please spell out and add to the list: Aandera RCM (Recording

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Current Meter) and FSI (Falmouth Scientific Inc.?). Any literature describing these instruments/methods?

6. Page 7, line 5: a short explanation what an "end member balance" is and how it is used here would be helpful

7. Page 7, line 10-12: Why is d18O set to a constant value? Why is the salinity of SIM set to 4 instead of 3? Are there any new observations since Dodd et al. 2009/ Rabe et al. 2009 showing that 4 is more realistic than 3?

8. Page 7, line 20: FEMSECT. Please spell it out: Finite Element Method Section model ? Somewhat more description of FEMSECT and possible advantages of FEMSECT compared to other box-models/ interpolation techniques would be helpful.

9. Page 8, line 1: "..over the duration of the survey". You should somewhere, e.g. in table 2, mention when the campaigns did take place (not only the year of the campaign). Did they all take place at the same time of the year? What are the uncertainties in the inverse analysis due to possible different time periods of the campaigns?

10. Page 8, lines2/3: "…these gaps occurred around the time when each mooring was recovered and again deployed". How long are these gaps? How large is the error/ uncertainty due to these gaps/the linear interpolation?

11. Page 8, line6: ".. which represents a negligible error." A negligible error compared to what? To the 123mSv you get as average or to other error sources in the methods/ observations? If I look to your errors in table 4, 5 mSv do not seem to be negligible.

12. Page 8, lines 10-20 and table4: This entire error estimation section is somewhat unclear to me: The entire error is the error from the inverse method (which includes errors due to gaps in time, upper layer problem and errors in the observations) plus the error in LFW from the end-member method. In table 4 you state that you discuss the error form the inverse method in section2.4, which you partly do but you do not mention any numbers. Thus, it is very difficult to understand where your error numbers in table

4 come from. Obviously you did not use just a simple linear approach for this error in table 4. Please clarify how you calculate your inverse method error for the different components. I also would like to see the entire error/ uncertainty for every year and every component and also for the average in table 4.

13. Fig.2: Is the scale in a) and c) the same as in b,d,e? Please add the numbers to all color bars or show only one single color bar for all figures.

14. Page 9, line 12/Fig.2: I have difficulties to extract the magnitudes of the barotropic and baroclinic flow from fig. 2. It might be worth to explicitly show both components.

15. Page 9, 15-17: "Overall we expect effects from both atmospheric patterns and bottom density to influence the barotropic flow" This is quite a very general conclusion. Please be more specific. Which atmospheric pattern is leading to high or low barotropic flows? A composite analysis between the barotropic flow through Fram Strait and SLP would be useful.

16. Page 12 lines 2-10: It is very likely that a positive correlation really exists but 5 values are by far too few to discuss differences in the correlations between 0.8 and "almost" 1. Thus, instead of ".. but almost 1 for PFW" you should write "..and almost 1 for PFW".

17. Page 12, lines 16-18: Again, having only 5 values,, I would be somewhat more speculative in the use of the words. You should write "The relatively constant LFW.." instead of "The almost constant . . ." (there is one strong outlier and the other 4 values are still varying quite a bit between 92 and 113 mSv) and "..component transports partly compensate" instead of "component transports compensate". Following lines: Could you specify what processes this could be?

18. Page 12, 19-24: I do not think that the discussion of the ratios is very helpful; both values are varying quite strongly within the 5 years and if you e.g. would compare 2009 and 2010 you will find a decrease in the ratio, 5 values are just not sufficient to make

this kind of trend-discussion.

19. Page 13, line 17: Alkire et al. (2007)

20. Page 15, line 4: replace "an ice ocean ..model, NAOSIM" with "the ice ocean ... model, NAOSIM"

21. Page 15, lines 16/17: Again, I am not so convinced by this ratio conclusion: According to table 4 we see a strong reduction in FIFB in 2009 but not in 2010, the ratio changed or varied but still the compensation rate is quite similar particularly if we take the error into account. Since FIFB and MW vary independently in the Canada Basin should we not expect a larger change in the ratio if most of the LFW water would have come from there? 22. Page 15, lines 25ff: Here, you compare, the variability in MW with the trend in river runoff, you should compare it with the variability in the river runoff. However, your conclusion will probably be the same, that river runoff variability is small compared to the variability in MW. 23. Page 17, lines 1-3: Please, shortly describe how the AO influences upper ocean stratification and how LFW pathways changes.

24. Page 17, lines 6-12: I do not see the direct connection of this section with the AO-discussion before and thereafter and it is a repetition of what is stated in 3.2.4 on page 12. Either delete these lines or make the connection to the AO-discussion clearer.

25. Section 4.4: I would consider either starting with the second part of this section or renaming this section and include "Denmark Strait" in the title. As a reader I am expecting this section mainly dealing with the transport through the Canadian Archipelago and the impact of the Canadian Archipelago transport on the circulation south of it but half of this section deals with FW-properties in Denmark Strait and Fram Strait. Model studies suggested a tendency towards an anti-correlation of FW-exports through Fram Strait and the Canadian Archipelago. Can you see a similar relation from your observations?

26. Page 19, lines 8-13: Here, I would like to see the total error added to all compo-

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