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Copernicus Publications

Editorial Support

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Longyearbyen, Svalbard, December 6. 2016

**Author Response to Editor and one Reviewer for MS No.: tc-2016-79:** “Fram Strait sea ice export variability and September Arctic sea ice extent over the last 80 years”

Dear Editor.

Thank you for your helpful comments. We are glad to state that we have fully implemented the suggested new data analysis and changes. In short we produced a new figure showing the comparison between the SAR based and mSLP based values for 2004 – 2014 as requested (new Figure 3). We have also included the effect of the varying ice concentration from Walsh et al. (2015) in an existing that now is Figure 5. We do agree that this improved the clearness and basis for our conclusion in a significant way.

We have also more precisely described the methods so it should be very clear that we have indeed used varying sea ice concentration for the 3-day time periods when SAR images are available, so the “bias” issue raised by the reviewer does not apply. We have also modified the abstract and discussion to reflect the suggested changes.

We have received a number of requests for the ice area export time series, and have added the data to PAGEA now. We will publish this with the new doi from the Cryopshere paper if this new version of the paper can be accepted now: <https://doi.pangaea.de/10.1594/PANGAEA.868944>

Our more detailed response is given **using bold text** below. The new improved text in the manuscript is shown **using red bold font**.

Lars H. Smedsrud, Mari H. Halvorsen, Julienne C. Stroeve, Rong Zhang and Kjell Kloster.

**Review of “Fram Strait sea ice export variability and September Arctic sea ice extent over the last 80 years” by Lars H. Smedsrud, Mari H. Halvorsen, Julienne C. Stroeve, Rong Zhang, and Kjell Kloster**

This is my second review of the manuscript. I therefore will concentrate on, if my initially major concerns were addressed appropriately. I thank the authors for providing a revised version of the manuscript. The text was changed at many occasions and many specific comments were addressed adequately. The analysis, however, was not changed or extended significantly.

My major concerns for the first version of the manuscript were

1) Before 2004 the authors only use the sea level pressure difference across Fram Strait (mSLP) as a proxy for the sea ice area export and it was suggested to combine the mSLP derived ice drift with ice area data from Walsh et al., which is used for other purposes in the analysis.

- The authors did not follow the suggestion to call the mSLP time series a proxy time series (neither ice drift nor ice area are observed directly here) but now clearly state that it is only mSLP based and clarified that several times in the manuscript, which is fine. They also clarified the description in the data section (which now raised new questions, see below).

**Answer: We are glad the reviewer finds our solution here satisfying.**

They, however, did not combine their mSLP time series with the ice area time series. This would add variability in sea ice area to the so called “sea ice area export time series”, which actually before 2004 only is a (indirect) ice drift speed time series. Ice area information is not included. How variable the ice area in Fram Strait can be is, e.g., shown in Kwok, 2009 (including a negative trend counteracting the positive trend found here).

The author’s argument for not using the ice area data is: “It is not straight-forward to combine it with the SLP observations to make a new and more ‘realistic’ ice export because it only provides a mid-month ice concentration field, and many of the winter months have values based on spatial and temporal interpolation.” I cannot follow that argument. In sections 2.1 and 2.2 it is described that (a) even for the SAR drift data monthly ice concentrations are used and (b) the mSLP time series is a monthly dataset. The Walsh et al. mid-month is the best information we have and is used elsewhere in the manuscript. Actually, Fram Strait is one of the better observed areas in the Arctic for the pre-satellite area because of ship logs.

**Answer: We have now implemented the effect of changing sea ice concentration over time. The effect is added as an additional time series for the winter and spring seasonal ice export in Figure 5.**

**As described further below we are using 3-day mean values for both sea ice concentration and ice drift for the 2004 – 2014 time series. The later averaging into monthly means lowers the uncertainty of these time series, while this would be fundamentally different for the Walsh (2015) data that are only available as mid-month values. We therefore used seasonal mean anomalies for sea ice concentration, and estimated the corresponding effect on the ice export. This shows that there is an effect in the 1950’s, but that the differences are small after 1979. Because the mSLP values are based daily observations we still hold the original time series as “the best”, but we are glad that the time-varying effect of sea ice concentration could now be included as well.**

2) Uncertainty of SAR derived ice drift time series.

- More information was added to the manuscript and some estimates of expected uncertainty presented.

Thanks. (I had a problem to find the Kloster and Sandven (2014) reference, see below)

**Answer: Thanks for pointing out the unclear text. The last few details confirming that we do use 3-daily values of sea ice concentration is now added. The url for the available report has been added to the reference list.**

3) Two very different time series are merged, the mSLP proxy based and the higher resolution SAR based, which also includes ice area variability. The complete mSLP based time series should be presented in addition to the merged one including statistics to let the reader judge by themselves about the differences.

- The authors stick to presenting “the best possible merged time series” only. Instead they present in their reply (for reasons not completely clear to me) the Walsh et al. 1935-2013 sea ice concentration time series, which actually just proves my last point that there is quite some variability in sea ice area in Fram Strait and including it in the ice area flux time series should be beneficial.

For the mSLP to SAR time series they refer to Smedsrud et al. (2011). Fig. 4 there shows the difference between an NCEP SLP based time series and the SAR one but not the observed SLP based time series used here. I would have loved to have such a comparison plot also in this manuscript. Fig. 4 there actually shows, in my opinion, quite high deviations between the two datasets. In summer deviations can exceed 50% in certain years and in winter deviations still can be at the 25% level. Fig. 2 in this manuscript only shows the mean seasonal cycle including standard deviations, which does not allow any conclusions on the differences, i.e., error of the mSLP based time series if we assume the SAR based one to be the truth.

**Answer: Thank you for this constructive suggestion. We have now included a new Figure 3 showing both the mSLP based time series together with the “best possible” SAR based time series for 2004 – 2014. The differences are smaller than in Smedsrud et al (2011) because we corrected for the seasonal differences due to the ocean current, or perhaps the atmospheric jet, as discussed.**

4) Ice area fluxes presented here are significantly different from previous estimates based on satellite radiometer data. Conclusions drawn from these time series would be opposite than what is presented here. The quality of the time series presented here was not quantitatively assessed against previous estimates. Conclusions therefore carefully be drawn based on the presented time series and other possibilities and explanation be mentioned.

- The authors give an extensive answer on this concern, which is appreciated. However, not much of it made it to the manuscript. The van Angelen paper is now mentioned and one short paragraph (p5, 138-41) was added at the end of section 2. The additions actually do not discuss differences to previous area flux estimates and no critical discussion was added to the results or conclusion section. While the authors in the first paragraph of their reply “certainly agree that “no particular record is the truth” and that the errors “are not well defined””

The plots in Fig.1 of the reply are actually interesting. I agree that there is no significant trend in ice area for 1935-2013 but there seems to be a clear negative trend after 1955 and also for the 1979-2013 period discussed in more detail here. In the reply Fig. 2 the authors present the mSLP based time series together with some ice

concentration variability. The conclusions from this are not completely clear to me besides that the two time series do not co-vary. The only analysis that made it in the new manuscript as far as I can see is that the ice concentration variability only makes up 10% of the ice area flux variability, which is true and was shown before. However, that does not mean that changes in sea ice area cannot offset or reduce a 6% trend in ice area flux.

**Answer: This was a good point to rise, and the effect of seasonal variability in sea ice concentration for Fram Strait (79 N, 15W – 5E) have now been analyzed and added to Figure 5. The effect is less than 10%, as suggested. The adjusted values would reduce trends between the 1950's and the 1980's. However, trends after 1979 remain unaffected.**

5) Very far reaching conclusions about the fate of the Arctic ice cover are drawn based on a very uncertain time series and very strong simplifications of physical processes and their interaction in the coupled climate system.

- The authors shortened the discussion section, added more references but rebutted that the simplifications might lead to overstated consequences. I still think the conclusions are too general and not clearly enough marked as hypothetical because they are (a) based on a uncertain time series with contradicting results found by other studies and (b) left out many physical processes, e.g., ice deformation and many others. I would prefer an approach where first the quality of a new time series is assed rigorously and discussed critically before far reaching conclusions are drawn or the limitation are clearly mentioned in the discussion. Anyway, this is probably also a matter of personal taste and one can agree to disagree.

The text of the manuscript has improved. I also appreciate the shortening of discussion section 4.5 and 4.6 while I still don't agree on the, in my view, over-simplifications in assumptions applied here. However, most of my main concerns (see above) were not addressed. In addition, the improved new method section raised another question about the method and a potential bias. I do not have the feeling the authors included mayor revisions to address the two reviewers and editors concerns. It is the right and has my full understanding that authors rebut reviewers comments they don't agree on. But I hope I could clarify why I cannot agree on certain points. I therefore stand with my initial assessment that the manuscript needs mayor revisions before publication.

**Answer: Thanks for confirming that the manuscript has improved. We have now addressed the two major concerns as explained above. We agree that this made a major improvement to the manuscript in the way that these issues of doubt could now largely be removed.**

Additional major comments

p3, l15-16: If I understand the procedure in the last paragraph correctly you first calculate a monthly mean sea ice drift speed value for the complete Fram Strait, i.e., you have one ice speed value per month. In a second step you now multiply this one ice drift speed value with one monthly mean sea ice concentration value for the complete strait.

**Answer: We use 3-day means of ice concentration. So the problem suggested below does not apply.**

Your figure 3 shows that drift speeds are much higher in the east than in the west of Fram Strait. Ice concentrations are, however, are much lower in the east. You have to multiply your high drift speeds in the east with low ice concentrations and your low drift speeds in the west with high ice concentrations to get the correct

ice area flux. Your method will yield a positive ice area flux bias, which might explain some of your higher flux estimates compared to previous studies. If this bias is constant in time is not known and therefore can, potentially, also influence the area flux trend. This issue should be investigated.

In your replies to my comments you write on p5 that you use daily ice concentration data in contrast to what is written here. Such contradictions leave open questions to me about the method applied.

**Answer: We now specify in more detail that we do use 3-day ice concentration values in section 2.1 for the SAR based time series. The trends and export since 1979 also showed to not be sensitive to changes in sea ice concentration from Walsh et al. (2015) as shown in Figure 5.**

#### **Minor comments**

p2, l15-20: Another recent study does not find a significant Fram Strait Sea ice area flux trends for 1988-2012 (Positive trend in summer, negative in winter).

Bi, H., K. Sun, X. Zhou, H. Huang & X. Xu (2016): Arctic Sea Ice Area Export Through the Fram Strait Estimated From Satellite-Based Data: 1988–2012, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 9, 3144-3157, doi:10.1109/jstars.2016.2584539.

**Answer: This new paper compares existing NSIDC data with the Smedsrud et al (2011) values, and finds very similar conclusions for the atmospheric drivers of the ice export. They do not find a trend for the shorter 1988 – 2012, but uses different “winter” and “summer” means, and cannot explain the differences between SAR based and passive microwave based values. They do, however, confirm that the SAR based values should be better. We thus find no important reason to include a citation this late in the review process. We will make our data available to the authors, and perhaps cooperate in the future to explain the differences better.**

Fig. 3: caption says dataset until December 2013, text says until December 2014

**This has now been corrected.**

#### **References**

I could not verify any statements regarding the SAR sea ice drift dataset (e.g. sec 2.1) because I could not find how to access the main reference:

Kloster, K. and Sandven, S.: Ice Motion and Ice Area Flux in Fram Strait at 79N using SAR and passive microwave for Aug. 2004–Jul. 2014., Technical Report no. 322c. Nansen Environmental and Remote Sensing Center, Bergen, Norway, 2014.

If this technical report is not accessible to the general public it should be removed from the reference list.

**Answer: A 2011 version of the report has been available on the institute web pages, but we have now also made the updated 2015 version available at the url below. This was also added to the citations list:**

**[https://www.nersc.no/sites/www.nersc.no/files/NERSC-TecRep-322d\(2015\).pdf](https://www.nersc.no/sites/www.nersc.no/files/NERSC-TecRep-322d(2015).pdf)**