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Editorial Support

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**Author Response to Editor 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 agree that the main focus of the paper is the export values themselves, and that they alone could be basis for the manuscript. However, we also think that when we also include a discussion of the effects of ice export anomalies on September Sea Ice Extent a broader set of readers of the Cryosphere will be interested.

In the interest of time we return this as quickly as possible, and have followed your suggested changes based on the previous review. A more general improvement in our discussion relating to the “missing mechanisms and processes” has not been performed, but some new text has been added mentioning these in the conclusion. In our view we have based our existing discussion well on previously published results, and have stated the assumptions and limitations clearly.

Our more detailed response is given **using bold text** below. The changes in the manuscript are shown using “Track Changes”.

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

**Dear Lars and co-authors.**

Thank you for your response to the review and especially for paying attention to the concern regarding the role of varying ice area in interannual variability in Fram Strait export.

The reviewer did express concern about your results being overstated in the discussion and there not being a clear delineation between the direct findings and hypothesized about how changes in export drive internal Arctic ice pack extent changes in summer. I also find this, and agree there are missing mechanism and processes in your discussion. I agree with the reviewers that it would be best to focus on the data set presented (your proxy time series) and clearly delineate hypothesized implications of your findings. Below are some specific comments where I find you can draw this distinction.

**Answer: We do follow your argument, and have performed the suggested changes. Our previous text was supported by earlier simulations, largely consistent with the simulations of Zhang (2015), and the new results from Williams et al (2016). If a more substantial alteration of our discussion is required we need more time, but can do this in January 2017.**

In the abstract your newly added text discusses the relationship between export and opening in the central Arctic, and hence summer ice extent. It would be important here to identify that you only find 18% correlation between export and end of summer ice extent.

**Answer: This has now been added to the abstract.**

Section 4.6 is where your discussion delves into territory outside of the scope of your time series analysis. The paragraph at line 30, page 12 is in particular overstating in how you relate your findings to climate model simulations. These models do not show the recent decline in ice extent, and if they do it may not be for the same mechanisms as we are observing now. There is a wide range of literature on this that I am sure you and your co-authors are aware of. No evidence of a trend in export in models between 1979-2013 should not be compared to your result that there is no trend over a longer time period. And no trend does not imply that the phenomena are purely driven by natural variability. It is possible for interannual and decadal scale variability to increase, due to external forcing, while maintaining no trend. In my opinion this section should be reduced to meet the reviewer's concerns.

**Answer: This section was initially included after a request from an earlier reviewer that would like such a discussion and comparison to future global coupled model simulations. We have now reduced the text as requested, and removed the text about the trends and natural variability. We are indeed aware of the wide range of literature, and agree that it is best to remain focused on the export here. We are also quite fine with deleting this last paragraph of 4.6 entirely. This would leave out much of the discussion about the difference between global warming in contrast to "natural variability". What remains clear is that natural variability is significant, and this is what is also stated in the conclusion.**

You have presented a very simplistic model for how increased export relates to summer ice extent reductions. In general I am a fan of simple mechanistic models that can be tested, provided the limitations of the model are clearly articulated. I would caution you to make sure your text is clear that you are describing the mechanism disregarding some potentially important factors (changing stability in the ocean/atmosphere for example), in the interest of presenting the existence of the link and recognize that quantifying the physical mechanisms involved in the link is not possible.

**Answer: We understand your concern. Adding the GFDL model simulations was indeed an attempt at quantifying this link further, and it did confirm this link, at least in part. We have added some text in the conclusion that emphasize clearly other processes leading to September SIE variability, and that this is actually 82% of the variance. We have also modified the text about the increased correlations for the last decades. Correlations do of course not imply any causal relationships, but our confidence is carried by the physical mechanism itself. It is further supported by earlier simulations (Smedsrud et al 2008, Langehaug et al. 2013, and Zhang 2015), and the rather complicated drift model of Williams et al (2016).**

Specific Edits Required

p1,l34: remove duplicate 'the last'

**Answer: Done.**

p2,p13-22: I disagree about not referencing recent papers you were unaware of when writing your manuscript, if they are relevant to your results. Kloster and Sandven (2015) is obviously a relevant study and does enter into the conversation about the large differences in estimates of trends in Fram Strait export. On this topic, did you consider the differences in location of flux gates in explaining differences between studies?

**Answer: The new Kloster and Sandven (2015) report is the last version, but we have been citing the previous version all the time. Kjell Kloster is maintaining this tracking manually and has normally written a report presenting new values each year. As a co-author he has kept us updated on the comparison of the two flux gates since 2012, and the differences are smaller than 10% (Kloster and Sandven 2015). It is across 79N that we have the longest time series, so this is what we have used here to get to the long-term variability. Because we have been aware of this work all along, and the new report is cited instead of the older version, we have not applied any changes here.**

p4,l26: I realize that you can not identify if there have been changes in seasonality of the fram strait ice drift and export prior to 1979. However if there were changes this will affect your bias correction. It is fair to point out the limitations of your assumptions. p5,l21 might be a relevant place to point this out, or where you introduce the method.

**Answer: Yes – this is a fair point to mention, so we have added a note where suggested (p5, line 21).**

p5,l26: How do you blend the two products? The use of this word makes me think you may be doing more than a simple substitution. Please be more precise in your description here.

**Answer: We have not done anything fancy here, we do indeed only add the two time series together. Before 2004 the values are based on the mSLP values only, and afterwards on the SAR. We have changed “blend” to “merge” now, including for the section title. The word “blend” is now only used for the Walsh et al (2015) data that do apply a more sophisticated blending of products.**

I am happy that you have discussed the relative impact on results for both the full proxy time series and the blended time series. I do note that you have chosen to present in figures the time series that demonstrates the largest change in the last decade.

**Answer: Thank you, we agree that this was an important test. We have not used the SAR based values because they demonstrate the largest trend, we used them because they should be the best possible estimate of the ice export since 2004. When calculating trends towards 2014 it would in our view be less correct to use the mSLP based values, but before 2004 they are the best possible values.**

p6,l11: "true monthly mSLP". I am not sure what you mean by true here. I would chose a descriptor that clarifies this is the observed pressure. (truth is not a given for weather station data too, any measurement has an error associated with it!)

**Answer: Yes, by “true” here we meant a proper monthly mean, i.e. a large number of observations as compared to the Walsh et al. (2015) data that are only one mid-month extent value. Changed now, using station-based observations instead.**