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Comment

Interactive comment on “Fram Strait spring ice export and September Arctic sea ice” by M. H. Halvorsen et al.

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We are glad you found the paper interesting and would like to thank you for your constructive criticism. We hope that you will find our response satisfying which is written in bold italic font directly below your original comments in the text below.

General comments: This paper examines the southward ice area export in the Fram Strait over the period 1979–2013. The ice area export from 1979 to 2003 is based on a relationship between observed mean SLP and ice export observations, while the ice export from 2004 to 2013 is based on sea ice drift speed derived from SAR images. The variability of ice area export in spring is then used to explain changes in September Arctic sea ice extent. A coupled GFDL climate model is also used to in-

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vestigate the relationship between ice area export in Fram Strait and September ice extent in the Arctic. It is valuable to use SAR data to determine the important Fram St. export. It is also interesting and useful to use observations to study the link between summer ice extent and ice area export in an earlier season. However, I am not fully convinced by the results and have questions that, I feel, need to be addressed. I recommend that this manuscript be accepted after significant revisions. The following comments/suggestions are provided for the authors to consider while revising the manuscript.

We hope that the answers we provide below are adequate, and we will improve the re-submitted manuscript according to your specific suggestions after the open review process is over.

Specific comments:

(1) The paper shows a positive trend for annual ice area export over 1979–2013. However, Figure 3 appears to show a negative trend (at least a neutral trend) over the period 1979–2003. The figure appears to further show that the positive trend over 1979–2013 is due to a strong increase from 2004 to 2013 when the analysis is shifted to use SAR derived ice speed. Thus the positive trend over the whole period may be caused by a shift in data or analysis, not necessarily real.

You are right that the 1979-2003 trends are not shown in Figure 3. It is also correct that the 1979 – 2003 trends are lower than those for 1979 – 2013. The earlier trends are, however, mentioned in the text (see page 13, lines 15-25). The trends are lower (3.5

Figure 8 shows that the ice export is coherent with the Arctic Dipole index (from NCEP/NCAR reanalysis) over the entire period, not just for the separate periods (1979-2003, 2004-2013). They also have very similar normalized trends over the entire period. This supports the consistence of the ice export data over the entire 1979-2013 period.

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(2) Results from the coupled climate model is also used to explain how spring ice area export may impact September ice extent. Needless to say, models have uncertainties. However, the model results are vastly different from observations. The modeled annual mean export is almost twice the observations, and the modeled standard deviations are 3 times bigger than observations. This makes me to wonder if it is justified to use the model results. I feel that a careful use of observations would explain things just as well.

Yes, models have uncertainties, and the simulated climatology and standard deviation of Fram Strait ice export are indeed different from the observations. This does not imply that the simulations do not have value, but that one needs to take care when applying them. In the manuscript we did not use any information related to amplitudes, but used the correlations between variables which are quite similar to observations. Observations also have uncertainties, and the results from the coupled climate model are used to support the mechanism/phase relationship between spring Fram Strait ice area export and September sea ice extent, not for amplitudes related to Fram Strait ice export. The model further have great value in that it can distinguish the mechanism behind the observed Fram Strait ice export trend and support the conclusion that the observed increase is not anthropogenically forced, but due to natural variability. A third advantage of the model is that it simulates such a long time period (3600 years) and therefore provides insight into the general mechanism, not just what occurred over the last 30 years.

The minor comments will all be addressed when we re-submit the manuscript.

Best regards,

Mari H. Halvorsen, Lars H. Smedsrød and Rong Zhang.

Interactive comment on The Cryosphere Discuss., 9, 4205, 2015.

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