The Cryosphere Discuss.,
doi:10.5194/tc-2016-79-AC2, 2016

Interactive comment on "Fram Strait sea ice export variability and September Arctic sea ice extent over the last 80 years" *by* L. H. Smedsrud et al.

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Received and published: 15 August 2016

Thank you for your very thorough and helpful review. The suggestions for changes have improved the manuscript, and on most issues raised a change has been implemented. Our response below substantiates our view on the issues raised, and explains our view for the few concerns raised that we do not think impact the conclusions presented in this paper. We include the original comments in **bold** font below.

The two major issues raised by the reviewer are (1) why we did not use the ice concentrations from the Walsh et al (2015) data set in the estimates of ice export prior to 2004, and (2) that the discussion is rather speculative. In response, we have looked more into the pre 2004 ice concentration data (see aFigure 1 below). Preliminary analysis of this

data shows that while there is increased variability, there is little long-term change that would systematically change the results we present in the paper and our discussion of trends. We also improve our description of the links between ice export and September ice extent. Our results are consistent with coupled GFDL climate model simulations, and also the recently published Williams et al (2016) paper that finds Fram Strait export to be the most important predictor for September SIE for 1993 – 2014, confirming our results are less speculative than the reviewer assumed.

The manuscript discusses the Fram Strait sea ice area export over the last 80 years, i.e., from 1935 to 2014. Large variability but no longterm trend is found. However, during the last decade according to the presented time series, ice area export increase. The authors, based on comparisons between spring ice export anomalies and summer minima, conclude that the increased ice export is partially responsible for the accelerated decline in Arctic sea ice extent. The variability and long term trends of the Arctic sea ice export and its connection to changes of the sea ice area within the Arctic Basin is an interesting and important topic.

For the manuscript at hand I had many problems reviewing it because it (a) discusses and mixes very different datasets and methods, and (b) draws very bold and far reaching conclusions based on quite simplified assumptions not taking the complexity of the coupled ocean-sea ice-atmosphere system enough into account:

- the authors construct a Fram Strait sea ice area flux proxy time series based on the across Strait air pressure gradient between Greenland and Svalbard. A regression between a high resolution SAR based ice area flux time series for 2004-2014 and the pressure gradient is performed. The regression coefficients (including a seasonal cycle adjustment) are used to reconstruct the sea ice area flux based on pressure observations alone. No sea ice observations are used before 2004 but only the air pressure. This fact was not initially clear to me

as a reader from the methods section and I only understood it from the side note on page 9. Before the authors mention a new longterm sea ice extent time series (Walsh et al., 2015) but in the end they do not use it. This means that the time series before 2014 does not include any variability due to the changing sea ice area within Fram Strait. While Fram Strait is one of the areas in the Arctic with the smaller sea ice decrease during the satellite era it still shows a significant decrease. The time series presented here does not account for any such changes before 2004. These issues or other limits of the proxy time series are not discussed in the manuscript. On the contrary the authors never call it a proxy time series. These facts should be clearly mentioned already at the beginning of the document.

We have tried to explain our methods as clear as possible, and start section 2 describing the ice drift observations from 2004-2014. We then continue with the Sea Level Pressure observations from 1935 – 2014. We describe how we blended these in section 2.3. While this is not a standard method we have clearly stated what we did. The term "proxy" is usually used for paleo observations like different organisms found in sediment cores that in some way reflect for example surface temperature. The physical relationship between SLP and ice drift is strong and qualitatively very different to this use of the word. We thus used the term "mSLP based" to describe our ice export estimates prior to 2004. This term was used in section 3.1 for example.

Fram Strait ice concentration change has been, and remains, small. Smedsrud (2011) found a small decrease in sea ice concentration across 79N for the period 1979 to 2009 of -1.3% per decade, and we have looked more into this in the new version using the Walsh et al (2015) data. For example, Figure 1 shows the mean (15W – 5 E) ice concentration along 79N. While there is considerable year-to-year variability, there is little long-term change.

- the Walsh et al. sea ice extent time series covering the complete 1935-2014 period is used for comparisons between ice export and ice extent in the

manuscript. For a revised version of the manuscript this dataset should be combined with the air pressure data to add some ice extent variability to the ice export time series, which should make it more realistic. It is unclear to me why this was not done. The Walsh et al. ice extent dataset is prominently introduced as a new and improved time series.

Yes - we used the new Walsh et al (2015) data set primarily to evaluate effects of sea ice export, because we wanted to investigate September SIE variability in relation to ice export. 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. For 2004 – 2014 we use ice concentration for the same days as the SAR imagery. While using the mid-month extent for 1938 – 2004 is worth looking into, it would create a "shift" pre and post 2004, and the reviewer also states that it would be better to use the "mSLP based" ice export series for the entire 1938 – 2014 period, so these two suggestions cannot both be implemented. We instead analyze the long-term changes in ice concentration and extent from 1938 – 2004/2014, and discuss the consequences of this change in the new version as shown in the figure.

- the 2004 to 2014 part of the time series is based on ice area flux estimates based on manually derived sea ice drift from high resolution SAR imagery. This should give very good estimates of the ice area export. I still would have appreciated some discussion of potential uncertainties due to the manual extraction by a human analyst or how they were mitigated. For example, were the number and the spatial distribution of the manually derived ice drift vectors constant for the complete time series? It is my understanding that this time series was build up over many years. Can we assume that the quality is constant over time? The stated uncertainty of +-3 km for an individual ice drift vector is actual much higher than what I would have expected. The grid cell size of the SAR data is about 100m. Adding some uncertainty caused by geolocation variability and identifying the exact same point in two images I still would have expected an uncertainty on the

order of 500m or better like for example reported for the Radarsat RGPS data.

The SAR time series has images every three days for the 2004 – 2014 period, and have been manually derived by the same person, Kjell Kloster, for all that time. The details are described in a report; Kloster and Sandven (2014). Although it is manually derived, having the same person doing it should lead to a constant quality over time. An independent test of a SAR image pair by the University of Tasmania (Heil, personal comm. 2012) showed that a computer image tracker could re-produce about 60% of the velocity vectors, but gave basically the same vectors for those that were picked up. We have added a better description in the paper now, and give more details below.

- SAR ice displacement is based on comparing two 3-day interval images, each with 300 -500m pixels. These are resampled from images with 50 -100m pixels in order to reduce the SAR specle noise, thus greatly improving feature recognition and to ensure that the same feature is found on each image and correctly tracked over the interval. Gridding to 2km accuracy is done using the known satellite orbit and instrument parameters in addition to one reference point, and has varied for the different SAR data suppliers used between 2004 and 2014. Drifting platforms with GPS were sporadically present in the Strait and used to check the SAR drift accuracy, indicating errors of about 3%. Similar errors were found for the comparison with several drifting IAPB buoys.

The ice concentration is measured by passive microwave (SSMI, AMSR) that has an estimated uncertainty of about +-3%, resulting in a flux uncertainty of about +-5% in the 3-day fluxes. Neglecting any biases, adding 10 values to get the monthly flux would decrease the uncertainty by a factor of three. An uncertainty estimate of the monthly fluxes of about +- 5% is therefore also conservative. For the seasonal 6-monthly means mostly discussed here, the estimated uncertainty further reduces as the individual months are truly independent. A total seasonal area flux of about 500.000 km^2 should thus have an estimated uncertainty smaller than +-11.2000 km^2.

- the authors then merge the air pressure proxy time series with the SAR based

time series. The complete air pressure based ice export time series is not shown. In my opinion that should not be done. The two time series have very different error bars and characteristics. The air pressure gradient is the only information we have got to estimate the ice export before 1979 when the satellite data start. This is argument enough to use the air pressure as a proxy to derive and discuss the ice export variability.

But again, it then also should be clearly stated what kind of time series is discussed in the manuscript. There is quite some focus on the 2004-2014 SAR dataset but the authors state themselves that this time period is too short to discuss significant trends. On page 7 the trends for the 1935-2014 air pressure time series alone are given and it is argued that these statistics are very similar to the merged time series. I would argue the other way around: use a consistent time series, i.e., the air pressure proxy ice export, for the complete period. This will avoid any biases, changes in statistics etc. due to the merging process in 2004.

We agree that this is an important question, and it is exactly why we discussed this merging in three different paragraphs (Page 7, line 23 – 29, Page 8, line 13 – 33). We did however end up on the opposite conclusion that the best thing was to present the "best possible" merged time series. The trends would be very similar if we should follow this suggestion and plot that in Fig. 2 instead. We will re-consider this in the new version, but no significant changes should occur. Note that the above suggestion of using ice concentrations from Walsh et al (2015) for at least 1938 – 1979, would lead to another "shift". Using the pressure based ice export as suggested here actually thus suggest not to use the Walsh et al (2015) data.

Figure 2 shows the similarity of the seasonal cycle between the adapted air pressure and SAR ice export time series. This is nice and shows good agreement but also differences for some months. For the reader it would be important to also see the two time series together for the complete 2004-2014 period. If the complete discussion in the manuscript would be changed to the air pressure only

time series (see my last point) the SAR derived time series could be added to Fig. 4 for comparison.

We understand the importance of checking the agreement between the mSLP based and SAR based values. Fig. 4 in Smedsrud (2011) shows such a comparison for 2004 – 2010. The updated values are similar, and we found no particular reason to include them as a separate figure here. From visual inspection of Fig. 4 here it should be clear that there are no significant differences in the merged values on either side of 2004.

- The manuscript mentions that their ice export estimates for the last 30 years do not agree with estimates from passive microwave radiometers (e.g. Kwok et al., 2013). Actually, these satellite data based time series do not find a trend in ice export, which is opposite to the trend found here from the air pressure data. The authors attribute this difference to the low resolution of the satellite data and that it will not correctly track all ice in Fram Strait (p. 12). That is one possible explanation but the authors do not demonstrate this failure but hypothesis it. That is okay because the satellite data is not the topic of their study. But then the authors should be more critical also towards their own time series and list factors, which could explain the difference to the satellite data. For example: there is an increase in the across pressure gradient during the last 30 years. As this is the only data used in the proxy ice export time series presented here this directly results in a positive ice export trend.

However, there are other factors, which influence Fram Strait ice export and could or have changed during the last decades and therefore counteract the increased pressure gradient:

(i) the ice area in Fram Strait (FS) shows a negative trend reducing the ice area export, which is not accounted for here.

(ii) the surface winds in FS are not only determined by the pressure gradient but have a strong contribution from thermal wind (THW) forcing (van Angelen et al.,

2011). If the THW forcing would have been reduced during the last decades that would counteract the increased pressure gradient

(iii) the ice surface drag (surface roughness) could have changed, i.e., the atmosphere to ice energy transfer function can have changed. This could also be caused by a change of internal ice stress, i.e., how lose or compact the ice in FS is.

(iv) the ocean forcing can have changed

I don't know if these factors can explain the difference to the satellite ice export time series but they should be discussed. Also in the summary it should be mentioned that all conclusion drawn here are based on the air pressure time series presented but that for other available ice export estimates one would get to complete opposite conclusions.

The reviewer states an important point, and we have not tried to "minimize" the sea ice export variability not related to SLP. We agree that there are a number of physical parameters that could have changed over these 80 years, and we have added a better discussion of these points in the new version. All points i) – iv) are valid, and will be included. We quickly summarized the "non SLP related" variability to be 20% on page 6 (line 3 – 6), but we will extend this text.

However – we have little or no observations of these parameters for the time frame from 1935 – 1979. An exception is i) that may now be estimated using the Walsh et al (2015) data. For ii) we agree that the stronger thermal wind forcing during winter (van Angelen et al. 2011) is another explanation for the larger export during winter than estimated by the mSLP. We discussed this seasonal difference and attributed it to a stronger East Greenland Current (EGC, page 5 line 10 – 28). It is also consistent with a stronger thermal wind, and note that the simulations of van Angelen et al. (2011) did not include an ocean model, so the thermal wind could well explained the stronger current during winter.

For iii) the main influence is probably due to thickness change, and it is likely that before 2004 ice was thicker and moved less effectively for a given mSLP gradient as found by Kwok et al (2013). This would lead to smaller values of ice export prior to 2004, and would thus increase the trend onwards from 1979. Our results remain different from for example Fig. 7 of Kwok et al (2013) that finds a positive trend for summer ice area export (June – September), but not for the annual values.

The main difference from Kwok et al (2013) is the 2004 – 2014 time period when we have higher export values. In this period we use the observed passive microwave sea ice concentration. This is in short why we wanted to present the “best possible” time series and not the “mSLP based” time series as suggested above.

- In section 4 from 4.2 onwards the sea ice area export time series and the Walsh et al. sea ice extent time series are used to draw quite far reaching conclusion about the influence of the sea ice export increase they find on the recent decrease in Arctic sea ice area. They make the in my view oversimplified assumption that every spring ice export anomaly directly relates to a loss in ice area for the summer sea ice extent. There are many other factors which will influence this relationship, e.g., if the ice gets compressed or more spread out in the Arctic Basin and many more feedbacks the authors are well aware of. One would need a coupled Arctic regional climate model to make more robust conclusions about such relationship. I actually like such simplified speculations in the way of: “If we would assume the ice export anomalies to directly relate to anomalies in Arctic summer ice area this would mean . . .” But here they are presented as hard results and in a very broad way. I recommend to remove most of the discussion related to this in section 4 and concentrate on the new 80 year ice export time series at hand. Some of these hypothetical consequences can then be briefly mentioned at the end of the discussion.

We understand the reviewers point. Specified simulations using a regional climate model could be performed for another way of estimating the effects of the sea ice ex-

port variability. Such model simulations are complicated, and have not been performed. Using a dynamical sea ice drift model Williams et al (2016) have actually performed experiments using coastal divergence and Fram Strait export, and find a similar level of influence on the September SIE. We are indeed aware of many other factors influencing September SIE variability, and only state that between 18% - 22% is caused by the export, apart from in the last 10 years. Our understanding is also based on the long control run from the coupled GFDL model. In a previous version of this paper (Halvorsen at al 2015, The Cryospere Discussions) these model results were included in more detail, and backed up our understanding. They were subsequently removed due to a previous reviewer's suggestion. We will add some of this text back and thus come out as less speculative in the next version.

The 80 year long air pressure based FS ice export time series by itself merits publication. Some information about the actual sea ice variability from the Walsh et al. dataset should be added. Errors and uncertainties have to be discussed more upfront and also in relation to other published but much shorter ice area export estimates. The mainly speculative discussion about consequences should be reduced and declared more clearly.

Thank you for your interest in the export itself. We agree, but also found that more people are interested in the export if the plausible consequences are also discussed. This is what we attempted to do here.

Minor comments:

p7, l18: for 2011-2013 the export exceeds 1mil sq km.

Corrected.

p8, 3.2: is there a reason for choosing the period 1979-2014 beside that it maximizes the trend found in an on longterm average trend less time series?

The period 1979 – 2014 is when the “high quality passive microwave” sea ice concentration data is available, so our confidence in September SIE is higher for this period than earlier.

p8, l19: in 2011 and 2012 the spring and winter exports are of similar magnitude but not in 2013 and 2014. Exports were on more similar magnitude during the 1940-50s. The reduction in seasonal cycle therefore is only temporarily.

Yes, correct. A change will be implemented. We were thinking of the smoothed values here.

p9, l3: I cannot see that Kwok, 2009 uses reanalysis data. They use satellite data.

Kwok (2009) used reanalysis data during the summer months, when the passive microwave data does not allow for "proper" feature tracking. Have been changed now.

p10, l13-14: In Fig 4 the 1995 export is larger than in 2012. That was also correctly stated before.

The difference comes from the use of calendar year. The Fig. 4 values use 1.September – 31. August.

p11: see also Kwok et al., 2013 for a detailed discussion of AO and ice circulation.

We will include both Hillmer and Jung (2000) for the NAO, and the newer Kwok (2013) for the AD/AO comparison. In addition, the work by Wu et al (2006), which come to similar conclusions to ours on the AD link, will be included into the discussion.

p11: the purpose and conclusions from 4.3 regarding this manuscript remain a bit unclear to me. Better motivate or remove.

A discussion of the large-scale atmospheric circulation was requested by previous reviewers, and relates to the comparison with the long-term variability simulations by Zhang (2015). A better motivation will be included.

p13, l8-9: this is a very strong assumption (no feedbacks considered) and makes

all conclusions based on this more hypothesis and speculations. Not a problem but should be clearly called that then and not presented the same way as the results based on the export time series. Could be more like an outlook section.

We are here trying to isolate one feedback – the strong ice-albedo feedback. While this is a simple way to do it, we at least clearly state this assumption here, and given that no dedicated simulations are available it is the best thing we can do in our minds.

p13, l26-31: again speculative; the correlation of -0.43 is modest as you correctly say.

Yes – correct. We do state that this is a "possible explanation", and that there could be others. This is the discussion section, and we will add more clarification here and speculations should be OK as long as they are clearly stated.

p14, 4.6: here you estimate the influence of one feedback. But there are many others. See e.g. the influence of ice convergence along the CAA contributing to the 2012 minimum. As a fully coupled system I am not sure one can simply separate feedbacks and sum them up again in the end. All feedbacks will interact with each other, there are many non-linear responses. A coupled GCM would be a better approach to evaluate this.

We do agree that dedicated regional simulations could potentially be valuable. What we have at hand are the long runs of the GFDL coupled GCM, which largely confirms that the AD is linked to the export, and further that the export is linked to the September SIE. The other factors that are correctly mentioned here were shortly mentioned on Page 15 (line 21-23).

p15, 4.7: here you look at a GCM but only in relation to AD. Does the GFDL model show high correlations between spring export and summer ice area minima?

In the 3600 year control simulation of GFDL coupled GCM, the spring Fram Strait ice area export also has an anti-correlation with the September Arctic SIE (r =-0.34). This is similar to the anti-correlation between the observed de-trended spring Fram Strait ice area export and September Arctic SIE (r = -0.43).

This modeling result is also consistent with our conclusion that “the recent increased spring export can directly explain around 10% of the observed September SIE loss”. However, a detailed analysis of the GFDL simulations was requested removed by another previous reviewer. We will add some of this text back as it probably has backed up our understanding of these processes, but been removed, so that it seemed more speculative than it really was.

New citations:

van Angelen, J. H., M. R. van den Broeke, and R. Kwok (2011), The Greenland Sea Jet: A mechanism for windâĂŘdriven sea ice export through Fram Strait, Geophys. Res. Lett., 38, L12805, doi:10.1029/2011GL047837.

Walsh, J. E., Fetterer, F., Scott Stewart, J. and Chapman, W. L. (2016), A database for depicting Arctic sea ice variations back to 1850. Geogr Rev. doi:10.1111/j.1931-0846.2016.12195.x

Williams, J. Tremblay, B. and Newton, R. Dynamic preconditioning of the September sea-ice extent minimum, J. (2016), Journal of Climate, 29(16), 5879–5891. DOI: 10.1175/JCLI-D-15-0515.1

Wu, B., Wang, J., Walsh, J. E. (2006). Dipole anomaly in the winter Arctic atmosphere and its association with sea ice motion. Journal of Climate, 19(2), 210-225.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-79, 2016.

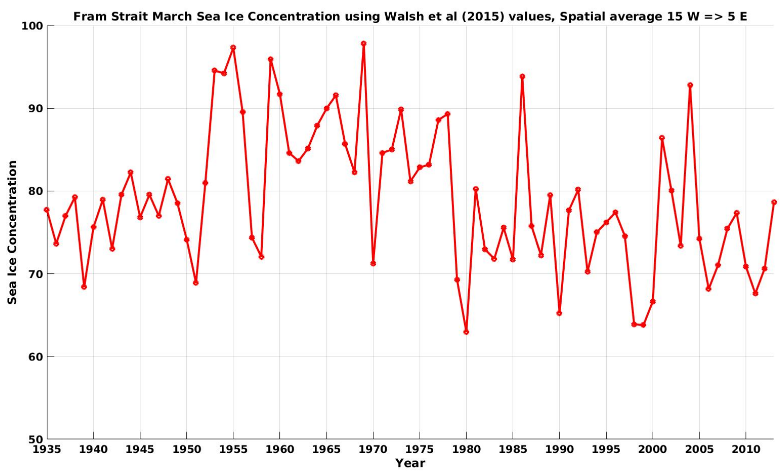

Fig. 1. Mid March sea Ice Concentration in the Fram Strait spatially averaged over 15 W to 5 E using data from Walsh et al (2015).