The Cryosphere Discuss.,
doi:10.5194/tc-2016-79-RC1, 2016


The Cryosphere
Discussions

# *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.

**Anonymous Referee #1**

Received and published: 3 July 2016

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 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.

- 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.

- 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 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 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.

- 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.

- 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.

- 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.

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 Welsh 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.

minor comments:

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



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

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.

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

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

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

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

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.

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

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.

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?

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