The Cryosphere Discuss.,
doi:10.5194/tc-2016-79-AC1, 2016
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The Cryosphere Discussions
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EGU

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: 8 August 2016

Thank you for your review, although it was rather short and not so positive.

Needless to say we were rather disappointed with your suggestion to reject the paper, and our response below substantiates our view on the issues raised. We include the original comments in bold font below. We appreciate the reviewer pointing out the Hilmer and Jung (2000) paper which we had overlooked.

Our results are consistent with those presented by Hilmer and Jung (2000), yet our study improves upon their results by 1) evaluating the long-term trend over a longer time-period (1935-2014) than they considered (1958-1997); 2) using station observa-

tions instead of NCEP reanalysis data, which have known problems; 3) analysis of the more Arctic relevant AO instead of the NAO; and 4) we find that the AD is a better index for explaining Fram Strait ice area Export than AO and NAO. Note that Hilmer and Jung (2000) only presented winter data (DJFM), but we present data for all months through the year, and we find substantial changes between winter and spring (Figure 4) over time as well. In their discussion Hilmer and Jung also state that "it cannot be decided wether NAO and Arctic Sea ice export are significantly related in a long-term context", and continued; "This question might be addressed by analyzing historical SLP data ". This is indeed what we have done in our paper here.

This paper attempts to extend the time series of Fram Strait (FS) ice export back to 1935. My primary concern with this paper is the accumulated errors in their regression of ice velocities going back to 1935. Given these uncertainties, I don't think they can make any definitive conclusions based on the extrapolated time series. Details on this concern and other comments are provided below. I suggest rejection of this paper.

The reviewer seems to have not quite understood the methodology. Our ice export time series is not based on extrapolation, nor on simulations. Rather, it is based on observations of surface pressure. A large number of papers find that ice drift is proportional to the geostrophic wind (this is basic physics), and there is no reason to expect the uncertainties in the calculated monthly means should accumulate. On the contrary will the uncertainties in the monthly means (based on 30 daily observations of pressure, and 10 values of the SAR derived ice drift) be further reduced when they are averaged into the seasonal means as described below.

1) Standard error about the regression line for equation 1. The authors state a standard error of the regression line of 3.4 cm/s. Ice velocities are typically 12 cm/s. The error adds up to an ice export uncertainty of +/- 250000 km^2, which is also the variance about the mean of 883000 km^2. Given this uncertainty, it is hard to trust any conclusions drawn on their extrapolated time series, which is

foundation of this paper.

The standard error is a statistical estimate of uncertainty, and describes the scatter around the regression line. The scatter is caused by the other factors influencing sea ice drift other than the geostrophic wind, and will be close to normally distributed around the regression line. The method is the same as used in Smedsrud et al (2011), but with 5 years of extended data. The uncertainty will further decrease when averaging into seasons is performed, because some months have slightly higher speed than the regression predicts, and some will have lower. So it is not correct to "add up" the uncertainty as suggested by the reviewer here.

Such a level of uncertainty is common in geophysics. When regression is used for non-physical relationships one should be very careful, but here the regression confirms first-order physics that the ice speed is proportional to the geostrophic wind. A correlation of 0.73 is very high. The correlation further increased to 0.88 after the correction on the East Greenland Current was done, reflecting that the SLP gradient is the most useful estimate of Fram Strait Export before detailed satellite imagery became available. We have explained clearly how the observations have been analyzed, and no formal errors in our data analysis have been suggested. The uncertainty and method is similar to previous published estimates, and seasonal mean values that are mostly used in our paper will have a lower uncertainty than the standard error derived from monthly mean values.

2) Fram Strait SLP Gradient I think the linear interpolation to estimate pressure to 78N after 1958 is probably OK since the stations are close, but prior to 1958, the southern station may be too far? The authors need to substantiate the use of the 3 weather stations on Greenland to interpolate SLP at 78N and estimate the across strait pressure gradient. One way to do this is to compare the estimated SLP at 78N based on the regression from Nord to Danmarkshavn, and Nord to Tasillaq during a period when they have data from all 3 stations.

Equation 1 should also be evaluated based on the 2 estimates of dp/dx across the strait to see how much difference the use of the different stations make.

We did perform correlations between the stations as described on page 4, line 11-18. There is relative lower but significant correlation (r=0.77 instead of r =0.93) between Nord and Tasiilaq, and there are no other alternative observations available prior to 1958, so this is the best data we have. The SLP pattern tends to follow the Greenland coast quite well (Fig. 4c in Hilmer Jung (2000) for example), and what matters here is the East-west SLP gradient, which should be robust.

3) The authors should cite Hilmer and Jung, 2000 "Evidence for a recent change in the link between the North Atlantic Oscillation and Arctic sea ice export", in any discussion of Fram Strait ice flux. I think this is the definitive paper on the topic. Given that HJ cover the period going back to 1958, and many of the authors own papers discuss the period after this to the present. This paper would really have to substantiate their estimates for export prior to 1958 to make an acceptable contribution to the literature.

Thank you for pointing out this paper, we will cite it in an updated version. We find that some of our conclusions are consistent with their results. Prior to 1978 Hilmer and Jung (2000) used simulations with quite a coarse resolution numerical model driven by another set of simulations (NCEP reanalysis) that are now known to have several issues in the Arctic. We therefore have more confidence in our own results for the early time period, as they are directly based on observations. Note that both the "missing" link between NAO and winter export, and a (not discussed) long term trend 1958 – 1997 is qualitatively consistent with our results. We used the AO index in our discussion (page 11) instead of the NAO, as it is a better index for the Arctic large scale atmospheric circulation and is also highly correlated with the NAO.

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