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

Anonymous Referee #3

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The manuscript "Fram Strait spring ice export and September Arctic sea ice" by Halvorsen et al. presents a new Fram Strait sea ice area export time series for 1979-2013. Contrary to previous estimates they find a strong ice area export increase (+7%/decade) during that time. They further discuss the connection between Fram Strait ice area export in spring and the sea ice extent minimum the following September.

This is a very interesting topic and merits exploration. The question of the balance between sea ice export and sea ice growth in the Arctic Basin and the connection to the recent shrinkage in sea ice area and volume is an important research topic. The quantification of relative importance of thermodynamic versus dynamic processes for the sea ice decline is still an open question.

General Comments

The authors construct a new Fram Strait (FS) sea ice export time series by merging SAR satellite data for 2004-2013 with the across FS sea level pressure (mSLP) gradient from meteorological stations. The mSLP data is regressed on the SAR data for the overlapping period 04-13, an empiric correction for the seasonal cycle is applied, and then the two datasets are concatenated. Unfortunately the mSLP ice export reconstruction for the full period 79-13 is not shown.

The regression method actually is crucial for the reconstruction of the complete ice export time series from mSLP. As it is described in the manuscript it poses many questions: Were the time series de-trended before the correlation analysis? the presented 2004-2013 SAR ice drift time series exposes a strong positive trend. Can the same trend be seen in the mSLP time series? If not the mSLP based ice drift reconstruction could artificially amplify trends, which would influence the validity of the merged time series before 2004. A linear regression does not help for that. A time series with trend and month to month variability can very well have a correlation of 0.76 (as you find here) or higher with a time series with the same month to month variability but no trend. The drift speed trend in the SAR data could not be related to SLP and wind. That, for example, was found to be the case in the Arctic basin where the ice drift speed increased but the wind not (e.g. Kwok et al. 2013, Spreen et al. 2011). I have many doubts with the quality of the presented time series here. There is no doubt that SLP is the main driver for sea ice drift speed in Fram Strait. There are, however, many other factors (ocean currents, local e.g. thermal driven winds, ice thickness and compactness) which are not constant (as your constant factor of 0.065 would imply) but vary with time and more importantly exhibit trends like, e.g., the ice thickness. It is alway tricky to reconstruct time series based on correlations alone and merge different datasets (here the very indirect mSLP based ice drift and the very direct SAR based ice drift). This all maybe would not matter too much if your results would agree with previous studies. But they don't. You very briefly mention that in a half sentence in the introduction by mentioning one study by Kwok et al., 2013 who find contrary to you no trend and then directly cite two papers of your group (Smedsrud et al. and Widell et al.) who find a trend based on SLP. The Kwok papers (and there are many) are based on direct observations of ice drift speed using low resolution satellite data. I cannot judge which one is correct but at least the differences should be discussed. And if you cannot very convincingly argue why your time series is of higher quality you should very clearly state that this also shades some doubt on the interpretation of your results. You at some point mention that the low resolution satellite data from Kwok has problems for high drift speeds. But shouldn't that mainly cause a bias and not a change in trends? How do you quantify that your pre-2004 mSLP based time series does not have similar problems? If I for example compare your Figure 3 of Fram Strait ice export with Figure 7 from Kwok et al., 2013 I have problems to find the correspondence and the two time series differ most during the recent years where you merged your SAR based ice drift. How to interpret the difference? Your SAR based ice drift supposedly is more accurate but does that also hold for the mSLP based drift? What does that mean for the derived trends? Could it be that your correlation based approach artificially introduced a trend and the homogeneous, on one dataset based Kwok time series and trend is more correct? Does the mSLP based ice export time series for the complete time period 80-13 show the same trend and variabilty as the merged one presented here? The used statistical methods to construct the time series have to be explained in greater depth and possibly improved, the differences to other time series have to be discussed in more detail that the reader can have a more comprehensive view on the uncertainties of the results and draw their own conclusions. These unknown uncertainties of the ice export time series presented here shades some series doubts on the validity of the conclusions drawn in this study.

In their interpretation of the relationship of spring ice export and summer sea ice area the authors completely neglect any other area changes caused by ice dynamics like convergence. they only discuss thermodynamic effects. Many studies already have confirmed the importance of sea ice dynamics within the Arctic Basin for the summer

sea ice area (see below). These are completely neglected here. I am not saying that the ice export has no contribution and mainly agree with the authors. But the way the results are presented and leaving out other explanations the reader gets the impression the results are over-emphasised here. The authors assume that all ice area exported from spring and onwards is not refreezing and further reduced by further positive feedbacks (e.g. ice-albedo). Anomalies in spring ice export thereby directly influence summer sea ice area (again, tehy completely neglect ice dynamics in the Arctic Basin). The authors quantify this by calculation based on some assumptions for the radiative forcing. They easily could have checked if their hypothesis is right if they would have compared it with anomalies in sea ice area widely available from passive microwave sea ice concentration observations.

Find more comments below.

At the present state I cannot recommend the manuscript for publication in the Cryosphere. There are many issues which should be addressed beforehand.

As main points I recommend - a better description and uncertainty estimate for the mSLP based sea ice area export time series. The presentation of the complete mSLP ice area export time series for 79-13 and discussion of possible biases introduced by the merging of two very different dataset. This is of especial importance as there are different time series with contrasting trends in the literature. - better quantification of the ice area export versus ice area reduction in the Arctic Basin by using available sea ice concentration information

Specific Comments

p4205 title: maybe you should clarify in the title that you are talking about ice area here (not volume or thickness)

p4206 abstract l3: unclear, remove l17: not the best citations for that statement (MYI & modelling). Have a look at e.g. the IPCC report.

p4207 l6: also Spreen et al., 2011 and Kwok et al., 2013 l14: and also not for the period 1990-2008 l22-24: don't understand the sentence, reformulate.

p4208 l21 ff: In Tsukernik, M., C. Deser, M. Alexander and R. Tomas (2010), Atmospheric forcing of Fram Strait sea ice export: a closer look, Climate Dyn., 35, 1349-1360, doi:10.1007/s00382-009-0647-z it is argued that daily and higher frequency atmospheric data is much more suitable to model the sea ice drift in Fram Strait. They find large differences between monthly and 6-hourly forcing. That is understandable as sea ice drift varies on synoptic scales. For example one or two cyclones with associated high ice drift speeds can make a significant difference in ice export. These would be missed in your monthly data. What is your argument to only use monthly data? When using monthly data should you not estimate the associated error?

p4209 l1-2: But in Fram Strait there is not only geostrophic wind but also a significant contribution of thermal driven wind (Greenland to ocean temperature contrast): van Angelen, J.H., M.R. van den Broeke and R. Kwok (2011), The Greenland Sea Jet: A mechanism for wind-driven sea ice export through Fram Strait, Geophys. Res. Lett., 38, L12805, doi:10.1029/2011GL047837 How do you account for that? And if you are not accounting for it how reliable is your pre 2004 time series?

p4209 l22: I cannot see that Widell et al. show that wind is the dominant force. Where and how is it shown?

p4210 l1: I would more say 8°W also looking at Kwok & Rothrock, 1999, Fig. 2 l9: over which period? How many data points? l20: Hm, r=0.76 I would not call good keeping in mind that you want to merge two very different datasets and interpret the trend of the resulting time series. To use a 99% confidence interval is not unusual and would be more appropriate for such an application in my opinion. See also my comment above in the summary regarding the regression.

l22-25: don't understand the meaning of these sentences. What did I learn? l26: Is that correlation used for anything? Otherwise remove.

p4211 l1: are the absolute numbers of your ice speeds really similar to Kwok, 2009? To me it looks like your estimates are biased high compared to Kwok. Please discuss. l6-7: what is this sentence supposed to tell me. I can only speculate: NCEP data is not good, ice drift speed increased, both? l13-14: the same internal ice stress effect you are correcting here for your seasonal cycle could also cause an artificial trend for your time series as ice thickness and thereby stress supposedly has changed during time (see e.g. Hansen et al., 2013). l17-18: this conclusion about the EGC is very far-fetched based on the data you present. I suggest to remove it.

p4212 l1-11 and paragraph before: besides ice stress and ocean currents also mention not resolved local winds (see above). Discuss that your empiric correction strictly only is valid for the period where you have overlapping data and not necessarily before. You are assuming stationarity here, which is a strong assumption keeping the, e.g., strong changes in ice thickness in mind. l18-19: this statement is not true. Kwok, 2009 is not using any SLP data but direct satellite observations. Therefore also your conclusion that your dataset is more accurate is not valid. l23-27: okay, this is a strong limitation. Why are you not using sea ice concentration to estimate the ice area for the export? Variability in your mSLP is solely representative for wind variability not ice export. If there is no ice to blow around there will be no export (or the oposite). I recommend to add sea ice area to your ice export estimate. Especially during summer it can be quite variable. Sea ice area in Fram strait also shows a small negativ trend (e.g. Kwok 2009). Not taking that into account will again create an artificial trend in your sea ice area export time series.

p4213 l10-11: yes, but these positive ice speed trends are found in the Arctic Basin but explicitly not in Fram Strait. Please mention that. l16: what is the mSLP trend for the complete 1979-2013 period. Please add those numbers. l26-28+: That is a good possible explanation. But wouldn't that mainly explain why yours are higher (positive bias) but not the opposite trends between the studies? You have high quality data for 2004-2013 but your mSLP data before that has high uncertainties, likely higher uncertainties than the low resolution PMW data from Kwok et al., 2013. Shouldn't you mention that? And could not exactly this merging of two different dataset one of the mayor causes for the strong trends you find?

p4214 l4-20: the complete paragraph completely neglects any sea ice dynamics and deformation. It assumes ice of constant area which simply is exported out of the Arctic Basin. As the authors now that is not true. There are constantly leads and polynyas within the Arctic Basin all year around. The Arctic ice volume is always a balance between ice export and production. I agree that this mechanism can get out of balance by increased export but I think that this paragraph is so much oversimplified that it completely should be removed.

p4215 l4: what does thin mean in this context. Under cold conditions sea ice easily can grow up to 1m within 3 months. l6: According to NSIDC the SIE maximum occurs between Feb 24 and Apr 2, on average on Mar 12. I would remove "early" in front of March here. l6-11: Again you completely neglect any changes in ice area due to sea ice deformation, i.e., convection. This is not valid. Maybe have a look at Kwok, R. and G. Cunningham (2012), Deformation of the Arctic Ocean ice cover after the 2007 record minimum in summer ice extent, Cold Reg. Sci. Technol., 76–77, 17—23, doi:10.1016/j.coldregions.2011.04.003 or the very recent Kwok, R. (2015), Sea ice convergence along the Arctic coasts of Greenland and the Canadian Arctic Archipelago: Variability and extremes (1992–2014), Geophys. Res. Lett., 42, doi:10.1002/2015GL065462. These studies highlight how import ice dynamics for the summer sea ice minimum area are. Please discuss these shortcomings of your interpretation. l22-23: on the contrary, these quite low correlations show that the mechanism presented here can only very partly explain the summer ice extent. That is not surprising as there are many more factors controlling it, which should be mentioned here as already said. These whole section needs reformulation to more honestly discuss what the results presented here can explain and what not. The one sentence about wind convergence is not enough.

p4217 l7 and ff: this is a thought experiment and likely not completely valid in the form you are doing it. As you already mentioned in March, April,and May open water in the Arctic Basin in large parts is still refreezing and not available for solar radiation input. Instead of using your very indirect "ice export changes translates directly to more open water area relationship" I recommend to have a look at one of the many sea ice concentration datasets (e.g. from NSIDC) and calculate the increase in open water area between 79-81 and 11-13. However, I doubt you will find the same values. In that case please discuss what that means for your assumptions. l13-21: all very speculative. Please first confirm with sea ice concentration data.

p4219 l2-3: that is not true. Have a look at your own reference list. You are only the only onces who so far have found an increased export. l9-10: instead or in addition to using the fully coupled climate model I recommend again to have a look at some more data, e.g., the already mentioned sea ice concentration datasets. l17-18: if the sea ice export in the GCM is double of your (already high) observed export how can the GCM be a good tool to explore the export to minimum relationship? If the export in the GCM is unrealistic this means that also the ice production in the Arctic Basin is unrealistic to balance the high export. The conclusions from such model regarding export vs. minimum are at least questionable.

p4221 l6-7: In your model the AD not surprisingly like in reality is correlated to FS ice export. Ice export is slightly (-0.34) anti-correlated to minimum SIE as well is the AD (-0.41). How does that explain that the AD to SIE minimum anti-correlation is caused by the export? Only because the correlation numbers are similar? I see no casual argumentation here just correlations.

p4222 l4-10: Okay, you don't find an ice export trend in a CMIP3 model and therefore conclude that your found increase in export is not caused by anthropogenic forcing. I disagree with this conclusion. There might be many reasons why the GFDL model and your export estimates do not agree. CMIP3 models aree not particular good in reproducing ice dynamics (e.g. Rampal, P., J. Weiss, C. Dubois and J.-M. Campin (2011), IPCC climate models do not capture Arctic sea ice drift acceleration: Consequences in terms of projected sea ice thinning and decline, J. Geophys. Res., 116, C00D07, doi:10.1029/2011JC007110). Or it could be that your estimate of a strong trend is not correct as already discussed.

p4223 l5-6 and ff: mention that this 10% of explained variability is based on correlation analysis alone. If two processes are physically and linearly linked they will show correlation. The opposite is not always true. A correlation (in your case with 0.3 weak) can just be arbitrary. However, you are also proposing a mechanism (removal of (a) ice area and (b) more open water with higher solar absorption). In such a case correlations can help to support the validity of a proposed mechanism (not the other way around). I recommend rewording this section.

p4223 l1-4 see my comment above for p4222 l7-8: Somehow I missed where you have quantified this statement of increase from 10% to 30% influence. With the GCM?

references Kloster & Sandven, 2014: how to obtain this report? It seems to contain significant information for this paper. If it is not publicly available it should be removed from the citation list.

Fig. 1 gives the wrong impression that most ice is exported in the middle of Fram Strait east of 0°E. This is wrong and misleading. Basically no ice is exported so far away from the shelf, especially during summer. There simply is no ice. This figure should be removed or completely changed. The speed might be high but the ice concentration is low in these regions.

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