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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 #2

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The paper uses 2004-2013 SAR based direct observations of sea ice drift across 79 N in Fram Strait to construct timeseries (1978-2004?) of icedrift, based on the relation between the observed velocities and the cross strait surface pressure difference. In the linear relation between the drift velocity and the geostrophic wind, a constant accounts for the remaining driving forces. The 1978-2004 mSLP derived drift is then merged with the 2004-2013 directly observed drift (?). NSIDC sea ice concentration data is then used along with the drift data to calculate the monthly area export. The spring area export is then put in the context of the sea ice extent following summer. Finally, a climate model is used to simulate nothing less than 3600 years of the coupled arctic climate system, including the Fram Strait area export.

The paper contains material and results which merit publication in journal like The

Cryosphere. I recognize much hard work, and do not want to discourage the authors. However, I'm afraid I have substantial concerns which I think should be addressed before publication.

The length of the paper is not justified by the results. As it is, it resembles a master thesis which have been slightly shortened and transformed into journal format. Explanations and references needed in a master thesis, where a student is required to demonstrate both insight and oversight, is not needed to the same extent in a journal paper. The paper covers too much, and has a structure which makes it difficult to read. As an example, it was unclear to me whether the 2004-2013 SAR derived part of the timeseries was added to the 1978-2004 mSLP derived data, or whether the mSLP derived results were used across the whole period. This is central information which should be precisely stated.

I reccomend a substantial shortening and restructuring of the paper. I suggest to remove the modelling part entirely, and focus on the data analysis. The modelling part does not add anything in this context. This is exemplified by the fact that its results are not even mentioned in the abstract. Moreover I reccommend to streamline the paper into a structure with introduction, data and methods, discussion an conclusion. In its present state, the paper contains discussions, but these are spread out across the paper. Often the discussion is presented before a result is delivered. The information then serves as an introduction. This is very confusing. Most of these discussing/introducing paragraphs should therefore be significantly shortened (or even deleted), and moved into a new discussion section, or to the introduction. There is a great potential for making this paper more precise.

I would also urge the author to rethink their use of linear correlations. Some passages contain a veritable hail storm of correlations. Everything is well correlated with everything, even when the correlation is below 0.5. Are all those correlations needed, i.e. are they useful?

Finally I would like to urge the authors to work on the language. A native english speaker or an expert is likely to improve the quality significantly.

Some specific comments:

-The title is imprecise. Merely adding "extent" at the end would help. What about something simple like "The relation between Arctic sea ice export and extent"? This comment also exemplifies a recurring problem throughout the paper; there are frequent referrals to "the sea ice cover". But what in the sea ice cover? Extent, area thicknes,...?

pp 4206 l. 1-2: Already the first two sentences are examples of a recurring phenomenon in the paper: before presenting the results, a brief discussion or introduction is delivered. It is sometimes difficult to separate between the results of this paper, and results derived earlier by others. These two sentences do not belong here in the Abstract.

-pp 4206 l. 21-23: It surprising to see, in a paper addressing sea ice export, that the anomalous export events in the late 80's and early 90s, largely believed to have triggered or at least amplified the recent thinning, is not even mentioned (e.g. Deser et al. 2000, Rigor et al. 2000,...). It is also surprising to see that the overall thinning of the sea ice is not mentioned as the major explanation of reduced extent.

-Is the 2004-3013 SAR derived part of the time series added to the 1978-2004 mSLP derived part? If so, does not this result in an inconsistency, introducing a changepoint in the timeseries? Would it not be better to use the mSLP derived data across the whole period? This needs to be discussed. If the latter is already the case, there is no need to elaborate on this. But nevertheless it must be made clear how the full time series is constructed.

-pp. 4207 l. 10: Here and elsewhere Kwok is cited. Wherever referred to, the Kwok results appear to differ. Why isnt the difference between the Kwok results and those derived here discussed? This is a crucial point. It hints to the uncertainty in the drift data sets, which are known to display large differences between them. What are the causes for the observed differences? It is imperative that this is discussed.

-pp. 4207 l.19: Kwok, in a relatively recent paper (2007-2011, somewhere in this time period), discussed how anomolues export of ice in the non-freezing months is a potential driver of the basinwide thinning. This merits at least a citation.

-pp 4208 l. 9: Accuracy better than 10%. Does not this statment require a citation or explanation? There are similar statements later.

-pp. 4209 l. 18-24: This is an example of introductory material that belongs in the Introduction, not in Results. It blurs your presentation when you mix introduction and discussion with the presentation of your results.

-pp. 4211 l. 16 and pp. 4212 l. 13 + other places: The EGC only covers a part of your section (something like 1-2 W to 6-7 W, please check with literature). There are two other dynamical regimes along your section; the AW recirculation zone to the east of the EGC, and the shelf to the west. It could be that the EGC dominates the variability, but this needs to be discussed. During summer, is not the NEW polynya to the west and the open ocean to the east likely to amplify the effect of the EGC on the seasonality of the drift? Merely due to their existence, confining all the exported ice over the EGC? This is not even mentioned, is that because the authors have ruled out the role of these local physical processes?

-pp. 4213 l. 10-11: The drift in Fram Strait is compared to drift speeds in the Arctic Ocean. However, the Fram Strait drift is locally driven. Does such a comparison then make sense? Should you not at least mention this difference?

-pp. 4213 l.26 and onwards: Your results are comparable to others before you switch to SAR results. Does this mean that your timeseries switch properties in 2004? Is it meaningful to derive trends from such a merged time series? What if the mSLP derived drift was used for the whole time period to derive trends?

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Discussion Paper

-Roughly in the same paragraphs: The authors compare their trend to trends derived from others. Would it not be useful to see whether the export time series reproduces the known large export events, such as those during the late 80s/early 90s, mid 90s, 2005-2006, etc?

-pp. 4215 l. 14: What does this mean? Did Kwok and Cunningham look at the the contribution from ice export to to the loss in MY ice? If so, how can this be compared to your results, you do not identify MY ice? Please clarify. If this comparison makes sense, which I'm sure it does, I think you should comment on why there is a difference between the Kwok and Cunningham results and yours. If you have no explanation, state that, or discuss possibilities. But please note that in my opinon, such a discussion belongs in a dedicated Discussion section.

-pp. 4215 l. 28-29: I think you should be careful to deliver such general statements. This is mostly true for the early freeze up season, when the new ice is thin enough to deform. Maybe you could be more precise.

-pp. 4218 l. 22-25: You have already stated that the correlations is well below 1, and that for some years there is no relation between spring export and September SIE. Then it is not really very useful to list single years when the relation appears to be visible.

-pp. 4218 l. 26-27: If you state something like this, you need to explain why this should be the case.

-pp. 4219 l. 3: "Largely overlooked" - it is far from overlooked, you have cited several studies addressing this link. You have obtained different numbers than others.

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