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The Cryosphere Discussions

Interactive comment on "Variability and trends in Laptev Sea ice outflow between 1992–2011" *by* T. Krumpen et al.

Anonymous Referee #2

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The manuscript "Variability and trends in Laptev Sea ice outflow between 1992–2011" by T. Krumpen et al. discusses the variability and trends of sea ice area export out of the Laptev Sea during 1992 and 2011. A positive trend in ice area export is found and its possible connection to wind forcing is discussed. The pathways and source regions of the exported sea ice are shown and discussed. In contrast to other studies no clear linkage to large-scale atmospheric circulation is found. Some co-variability of the winter ice area export to the sea ice area in the Laptev Sea during the following summer is shown. However, this connection is not always apparent.

The manuscript discusses a variety of different aspects of the Laptev Sea sea ice outflow. Not only the outflow time series, derived from satellite observations, is presented but also connections to the large scale and synoptic atmospheric forcing are discussed.

The results of this study are discussed in context with results from previous studies for other time periods. Some findings of previous studies (e.g. connections to AO and DA) cannot be reproduced. Hinting either to a change of these connections during recent years or a lack of our current understanding of those connections.

The topic and results of the manuscript are important and add new knowledge to our understanding of the Laptev Sea sea ice dynamics in respect to the atmospheric forcing. In general I find the results and methods presented sound. However, sometimes the description of the methodology lacks some more in depth descriptions. Especially the satellite sea ice drift validation (which is not the main topic of this paper) is not very convincing at the moment as it is presented. Some results should also be further discussed as detailed in my comments below. These issues have to be addressed before publication.

Below is a list of more detailed comments. After these are resolved I can recommend the manuscript for publication in "The Cryosphere".

Specific comments:

2893, L15: "Smedsrud et al. (2011)" instead of "Smedsrud and Skogseth (2011)". This reference is wrong several times in the text. Please check. It might also be worth mentioning that, to my knowledge, this is the only study, which found a strong ice export increase in Fram Strait.

2894, L3: what is the time span for the 1.57 m mean? 1930 until when?

2894, L10: hav -> have. The connection and importance to the sentence before is not clear to me. Maybe it can be shortened and combined with the sentence before.

2894, L16: word like "using" missing

2895, L15: Which "boundaries"

2895, L17: maybe add an "e.g." before the two references as there are many more

studies using the mentioned technique especially older ones from e.g. Emery et al.

2896, L7: European

2896, L11: “over 3-days overlapping time periods” This part of the sentence is not clear to me. Maybe spend a few more words on that. Also the next sentence is hard to follow.

2896, L15ff: It is surprising to find this validation in the “Data and methodology” section. I would rename it (e.g. “Dataset description and validation”) and also add a new subsection 2.2. To me a clearer and easier to follow structure would for example be: 2.1 Sea ice drift dataset 2.2 Sea ice drift validation 2.3 Sea ice concentration 2.4 Sea ice area flux estimates at the Laptev Sea boundaries

2896, L18-20: The description of the ASAR drift dataset is very brief and no further reference is given. Which months are used? How many scenes per month? How was the comparison done (down-scaling of high resolution to low resolution dataset etc.).

2896, L21ff: What is the beam width of the ADCP? In any case it will be very small compared to the satellite ice drift resolution. How is the comparison done? I guess interpolation of the satellite data to the ADCP location. In the end you are comparing the mean of about 10 independent (10 x 3 days per month) satellite sea ice drift estimates with a special scale of > 50km with probably hundreds (what is the sampling frequency of the ADCP?) almost point-wise ADCP ice drift estimates. Even for perfectly accurate satellite and ADCP drift measurements one can expect quite some deviation between the two datasets under these conditions (one would need the spatial and time variance of the ice drift at this location to calculate numbers). Therefor I am not sure that the difference of the correlation coefficient between the ADCP and ASAR comparison is significant. Your conclusion that the polynyas are responsible for the difference sounds reasonable. However, I don’t think you can show that using these two very different validation datasets. An in depth validation with error estimate of the ice drift dataset is probably beyond the scope of the paper. However, you should briefly discuss some of the issues involved doing such a validation. In any case you should give a few

more numbers for the comparison not only the correlation coefficient. What is the SE mentioned in the graph? What is the mean difference, i.e., bias between the datasets?

2897, L3: “EASE” not defined

2897, L5-6: I don’t understand the second half of the sentence.

2897, L13-14: Was the seasonal cycle removed before calculating trends and significance? Otherwise these statistics are not valid.

2897, L21: “NSIDC drift” not mentioned in data section and no reference given. Please add and shortly mention the differences to the IFREMER drift dataset.

2898, L7: What is the 14% supposed to mean? Please also give a number for the variability also in km2 for better comparison.

2898, L8-9: I guess these are now fluxes calculated with the IFREMER data only. Mention that. Please explain in a little bit more detail what you have learned from the NSIDC to IFREMER comparison. Why did you choose the IFREMER data for the rest of the study? What does the comparison mean for the error estimates of your results?

2898, L17: double 2007/08

2899, L14: Fig. 8 is mentioned before Fig. 7 in the text. Please swap.

2899, L20ff: Between which points or regions is the SLP gradient calculated?

2900, L8: when

2900, L11: “likely”? You should be able to explain that exactly as the blue line is the product of the red and black line. Is the normalized or % trend in drift speed larger than the area flux trend?

2900, L16: What is the approximate mean ocean circulation across the two transects? Would one expect the ocean to play a big role at these locations in any case?

2900, L17: It is not clear to me what I am supposed to learn from the comparison to

the SLP gradients. Please explain in more detail. Naturally SLP gradients and ice drift are correlated. However, as you mention the SLP gradient is not a good predictor for the ice drift in all cases. The internal ice state and ocean forcing can play a significant role. Is that your point?

2901, L27ff: I had problems to understand the meaning of this sentence. Suggestion: "Because the origin of most of the ice leaving the Laptev Sea is situated in the Central Laptev Sea in November and not in near-coastal, polynya areas . . ."

2901, L5: . . . end of winter. " However, mostly melts during summer again and the ice produced in Laptev polynyas does not contribute significantly to the ice in the Transpolar Drift or elsewhere in the Arctic Basin. If I understand correctly. I think this is an interesting finding and could be pointed out even stronger. Or is it likely that a lot of the ice from polynyas is transported out of the Laptev Sea after April?

2902, L16: How do these numbers compare to the difference to the NSIDC based area estimates, which can be used as another uncertainty estimate or give a hint to possible biases.

2902, L22: "4.83" What was your mean area flux again?

2902, L24: do you mean quantitative?

2901, L10: "accuracy . . . is high" give some numbers.

2903, L2: What SSMI ice drift dataset does Alexandrov et al use? If it would be the NSIDC one the positive difference would be consistent with your earlier findings. Anyhow, I would mention here again that your IFREMER area flux estimates are higher than the NSIDC mainly SSMI based one. Low resolution SSMI/QS ice drift datasets tend to have a negative bias, i.e., are to slow. Thus I assume your higher IFREMER estimate is more realistic. Again, it would be interesting to know if you find any bias in your comparison to the SAR ice drift data.

2905, L13-15: So your observations for 92-10 are in contrast with observations for the

70s and 90s? Clarify this please. I think for the Fram Strait area flux correlation with AO also dropped during recent years (sorry, don't have a reference at hand right now).

2906, L19: ". . . enhance the northward ice transport" Shouldn't this depend if the cyclone enters the Laptev Sea on the eastern or western side (compare also Fig.7). And you are only talking about summer situations here (warm air) I guess, which are not part of your study, or?

2907, L20-22: You were right. You can update this now.

2908, PSSM: Wouldn't it be more consistent to also describe the PSSSM data in the "Data Section"?

2908, L17-18: ". . . by means of . . ." I don't understand this part of the sentence. Are the AMSR-E and SSM/I data inter-calibrated?

2908, L26: Before you call a R =-0.62 weak (p 2905).

2910, L14: I would add "For comparison this is . . ." Otherwise one might imply you are talking about a connection to the FS export (I assume you are giving these numbers only to illustrate the magnitude of the flux).

2910, L25: "less pronounced" As a fraction of the mean flux (0.61) the EB trend is even more pronounced. I would reformulate this sentence.

2917: "positions"? I only can find one black star.

2918: Meaning of lines not described.

2920: Does the FS flux drop dramatically in 09/10? If not remove the black line or mention the time series end in the caption.

2923: The location of the Laptev Sea is hard to identify. Maybe add the NB and EB flux transects for clarity.

2927: blue line

2928: what does the gray, shaded area mean?

Interactive comment on The Cryosphere Discuss., 6, 2891, 2012.