The Cryosphere Discuss., 6, C2777–C2781, 2013
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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.

**T. Krumpen et al.**

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Dear Julienne,

we very much appreciate the comments you made on our manuscript. Following your suggestions, some parts in the manuscript were modified and reformulated. In addition we once more carefully checked for grammar/spelling. Please find our answers and revised sections below. Again, many thanks for comments. Best regards Thomas Krumpen

I thank the reviewers for attention to the reviewers comments. However, I do feel several more corrections are needed prior to publication. First off, the manuscript requires careful editing for proper grammar/spelling. For other comments, please see below.



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Additional comments:

1) abstract: should be average "total" winter (line 4) Answer: Thanks, was changed.

2) The trend in EB flux is nearly the same magnitude as the average EB flux, which implies that the average EB flux was close to 0 early in the time-series. If so, why is that, and what caused it to change? Answer: Correct, average EB flux is only around 0.61 x 10ˆ5 $km^2$. This is because there are years being dominated by ice import through the EB, rather than ice export (see line 300 - 306). Monthly variations in ice area flux through the EB and NB are mostly related to changes in the SLP fields (see line 382 - 390 and others). However, there is no evidence for an increase in geostrophic wind velocities that could explain trends in EB. Therefore we believe it to be rather related to a change in the ice cover (e.g. ice thickness). See also slightly changed line 534 - 547.

3) line 21-24 - should add that reduced ice concentration and thinner ice leads to increased transport. You should verify that winter ice is thinner and has reduced ice concentration - this would be easy to do with the satellite data, and there is some satellite data (ERS 1/2, ICESat, CryoSat so that you could look at some thickness changes). Answer: Thanks, added (line 534 to 547). The verification of a thinning winter ice cover is however a bit beyond the scope of this study and would require an entire new chapter discussing associated errors etc. Nevertheless, we now provide an updated and more comprehensive list of publications that cover the thinning of winter ice as the consequence of a rapid reduction in multi-year ice coverage etc. (e.g. Haas2008, Kwok2009b, Comiso2012).

4) intro: update accelerating rate of decline with the Stroeve et al. 2011 paper (line 38). Can additionally update climate model references to include those related to CMIP5, such as Stroeve et al., 2012, Overland et al., 2012. Answer: Okay, updated: Massonnet et al. 2012, Stroeve et al. 2012 and 2011.

5) line 56: trends were not found first time by Smedsrud, but there were found to increase. Rewrite. Answer: Thanks, rewritten



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6) line 60-64: ice volume loss will accelerate further - do you mean to say Fram Strait export? Answer: Sorry, yes. I meant the Fram Strait volume flux. Changed.

7) intro: if Zakharov estimated a similar value in the 1960s, then one could argue not much as changed? Answer: Yes, true. In the introduction of the manuscript we are citing Zakharov since he was the first author ever providing an estimate of Laptev Sea ice exchange with the surrounding seas. Unfortunately the publication is in Russian only and not available in the internet. Note that the citation provided in the manuscript was taken from a publication of Alexandrov 2000. Hence, a direct comparison with our estimates is difficult, since a closer look at the used methodology and accuracy of the provided estimates is not possible. For some reasons, Alexandrov himself did not compare his results to Zakharov's either. That's why we decided to neglect the work Zakarov in the discussion of our area flux estimates completely (chapter 4). We still believe that it is worth mentioning the early work of Zakharov in the introduction. However, we also agree that the similarity of results should be mentioned somewhere, although an interpretation remains difficult owing to a missing translation. Please see revised paragraph line 462 to 469.

8) section 2.3 Can you add a couple of sentences on the accuracy of the 85 GHz sea ice algorithm? Answer: Okay, spatial resolution added

9) results: the NSIDC drift data doesn't directly give flux. Did you use that drift data together with the 85 GHz sea ice concentrations? If so, be sure to state that. Same with the Smedsrud data. Answer: Yes, we were using same sea ice concentration data as for IFREMER flux estimates. This is stated now. With respect to the Smedsrud data there is a misunderstanding: The approach used by Smedrud is different to ours (see you comment 11). We make this more clear now.

10) given the uncertainties, can you really say the NSIDC ice flux differs from the IFREMER ice flux? Answer: We could show through a comparison of NSIDC and IFREMER data with SAR and ADCP drift information that NSIDC estimates underestimate ice drift



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velocity (see answer to comment '2898, L8-9', '2902, L16' and '2903, L2' made by reviewer 2). This becomes apparent also in Fig. 3 and is in agreement with findings of e.g. Schwegemann 2011 in the Antarctic. However, in the manuscript we use the NSIDC data only to conclude about relative consistency of the IFREMER dataset (see l 280 – 291).

11) I don't see how your approach is the same as Smedsrud (Similar to our approach, the authors derived ice area transport rates from radar satellites and SLP differences). You didn't use radar data nor SLP differences. Answer: Yes, that's wrong. Actually, actually I meant 'different' not 'similar'. Just out of curiosity I was applying our approach to the Fram Strait and got quite different results to what was obtained by Smedsrud. This is partially related to the low spatial and temporal resolution of the drift product in the Fram Strait. On the other hand, the accuracy of manually derived SAR ice drift estimates is questionable (in particular during summer months when ice flows change shape easily). I replaced 'similar' with 'different.'

12) linkages with large-scale circulation should really consider the positions of the pressure anomalies. Several papers now have shown how the AO-sea ice correlation has changed, and that it's not the index that is so important, but rather the position of the SLP anomalies (case in point the 2009/2010 extreme negative AO). Some mention of this is needed. Answer: Good point. This was included. Check line 621 – 625.

13) line 638: did you mean to say positive AO phase? No, that's correct. See also finding of Proshutinsky 1997 or some of the polynya studies made by Willmes in that area: During low index phases, ice leaving the Laptev Sea is directly incorporated into the TD drift. This phase is characterized by an above the average northward ice advection.

14) in section 4.2, seems you should mention the general thinning of the Arctic ice cover. Several publications exist on the topic, as well as change in ice age from Maslanik et al. The thinning of the ice, from changes in circulation and general





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warming, have resulted in an ice pack that drifts faster. The thinning of the ice is also reflected in Figure 12 since the polynya/ice drift relationship starts to diverge in recent years. Basically the rules are changing for a thinning ice cover. Metrics that were useful for predicting the summer ice extent (such as more cyclones = more summer sea ice, less cyclones = less summer sea ice, negative AO = more summer ice) are changing. Answer: Yes, correct. So far we limit discussion to the physical connection between late winter/early spring ice area flux and ice extent in summer. However, we agree that the thinning of the Arctic ice cover will further accelerate ice drift that will in turn amplify the preconditioning effect of offshore transport on summer sea ice anomalies. This is now stated at the end of section 4.2 and added to the conclusion. A more comprehensive list of studies on thinning Arctic sea ice and the enhanced ice drift is now given in line 534 - 547 (see also our answer to comment 3).

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C2777/2013/tcd-6-C2777-2013-supplement.pdf

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