

Interactive comment on “Satellite-derived sea-ice export and its impact on Arctic ice mass balance” **by Robert Ricker et al.**

Anonymous Referee #1

Received and published: 26 March 2018

This study combines sea ice thickness retrievals from CryoSat-2 with different ice drift products to estimate the volume of ice export through the Fram Strait over the winters of 2010-2017. The authors find that ice drift variability dominates the variability of ice volume export over annual and inter-annual timescales, but the seasonal cycle is also impacted by the thickness of exported ice. The export of sea ice through the Fram Strait accounts for 54% of the variability of multiyear ice (MYI) volume over a given winter season.

The manuscript is clearly written and the figures are well-constructed and informative. Unfortunately, I still struggled with this review as I'm left wondering what the key purpose of the paper is. The manuscript includes a wealth of information but doesn't read like a complete method or scientific study. This is highlighted by the concluding bullets

C1

ranging from comparison of drift products to importance of ice export, presented as a list rather than a logical connected paragraph.

For a methods-based paper I would expect a more thorough description of the product development. This includes expansion on the error analysis explaining why the specific approach was chosen, how sea ice drift uncertainty is estimated using empirical error functions (brief summary of Sumata (2015) method), which high resolution SAR data is used a reference, and why such a reference is needed. The usefulness of the paper in a scientific sense is currently limited over such a short time frame, and it lacks novelty considering the number of existing sea ice export studies for the Fram Strait. The obvious way to develop the paper scientifically would be to investigate long-term trends in ice volume export, but as the authors state this would require a consistent methodology to compute ice volume flux through Fram Strait from multiple products.

I encourage the authors to think about their intended purpose for the paper then either a.) sufficiently describe the development of their new Arctic sea ice volume export product or b.) expand their scientific analysis utilising the product. Despite these reservations I would like to repeat that this was a well-written paper and the content will be of interest to the sea ice community, so I have included some detailed and technical comments below.

Detailed comments

P1L18-P2L7: The reasoning is not clear here with regards to concentrating on MYI and winter. For example, the authors should explicitly state that winter does not play such an important role for FYI mass balance, and why. They also mention summer ice concentration when the focus of the manuscript is on the winter period. If the authors want to justify their concentration on a given ice type and season then I suggest they first discuss winter ice mass balance variations (MYI and FYI) and then summer (MYI and FYI), then reach a logical conclusion.

P2L16: State ICESat periods

C2

P2L20: "...we use the CS-2 ice thickness dataset..." -> "...we use *our* CS-2 ice thickness dataset..." There are numerous datasets, so the authors should be specific about which is used.

P2L21: Be more explicit about which part of the study is novel (i.e. the "first" estimates of what). It is not the sea ice export estimates themselves, but the timeframe for which they're provided.

P2L21: Define "winter"

P4L21-23: Explain how unconstrained polynomials are dealt with at lower latitudes

P4L26: And also snow depth, correct?

P5L11-12: NSIDC products are also provided monthly

P6 Figure 1: The FYI and MYI masks are quite hard to distinguish with the current colour separation. I'd like to be able to see them clearly for each year.

P9 Figure 4: It is not clear why the frequency scale ranges from 0-25 for the right hand box. It's also hard to see the variation in the lines over one another. Sub-plots could work better here. It may be for thickness and drift the time-series isn't necessary, as the relevant data is already displayed in Figures 2 and 3.

P9L7: "2012/2013" is repeated. I believe second date should be 2014/2015.

P12L5-6: I suggest moving the statement that the choice of drift product has no major impact on the variability analysis to the start of the start of the section, as until then I wasn't sure of the point of the section.

P14L1: Should this read "Similarly" rather than "On the other hand" as it's previously explained that uncertainty of ice drift also increases at lower latitudes.

P14L3: What is the "compromise" here? Uncertainty reduction vs. discarding higher ice velocities? It's not clear.

C3

P15L33: "...**seasonal** or **winter** MYI area loss can be explained almost entirely by ice export."

Technical comments

P1 L1: "*Sea* ice volume export..."

P2L18: "sea-ice" -> "sea ice" for consistency

P2L22: "access" -> "assess"

P3L11: "...(Continuous MCC)..." -> "...(Continuous Maximum cross-correlation (MCC))..."

P3L9: "Table 1" -> "Table 3"

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-6>, 2018.

C4