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The Cryosphere
Discussions

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

Anonymous Referee #2

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The manuscript presents a new time series of Fram Strait sea ice volume export for the years 2010 to 2017. Fram Strait is the main gateway for sea ice leaving the Arctic and therefore estimates at that gate are a good indicator for sea ice mass change by ice export. The topic therefore is highly relevant for Arctic climate understanding. The authors describe a method solely based on satellite data, i.e., CryoSat-2 and microwave radiometer and scatterometer data. A similar method was applied before for different satellites but not for this combination and more recent years. While not discussed here the method potentially can serve as a tool to extend previous ice volume export time series. Results are discussed in connection with atmospheric forcing (NAO, AO) and the total Arctic mass balance. The topic is suitable for publication in The Cryosphere.

I, however, have some mayor concerns, which have to be addressed beforehand.

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Mayor criticism:

- Flux calculations (eq. 2 & 3) seem to contain an error (varying length of grid cell not taken into account), which can cause the volume flux to be biased low by up to 40%. This has to be corrected or justified why the flux calculation is correct as it is given. This error will change the magnitude but not the variability of all calculation. Thus most conclusions will still be valid.

- The Sea ice export estimates based on three different ice drift datasets do not agree within their uncertainty estimates. Which means that either the uncertainty estimates are wrong (to conservative) or some justification should be provided which dataset is more trustworthy. Otherwise the reader cannot use the information provided in a meaningful why. Unfortunately the difference is not just a bias but in some years exhibits different variability (Fig. 6b).

- Explanation of changes of MYI volume in the Arctic basin (4.4) does not sound physical to me. 4 out of 6 years show a gain of MYI ice volume through winter (100-300km3/month), even after taking the ice export into account. The authors attribute that to thermodynamical growth. This would mean that in most cases ice growth for MY dominates the MYI ice volume change over ice export. I find that highly unlikely. The thermodynamic growth of snow covered MYI ice of >1.5m should be close to zero. Ice export through Fram Strait should by far dominate the month to month changes. The authors need to analysis this in more detail or provide more evidence. Actually, I assume their finding are dominated by the uncertainty of their MYI classification. They only use a binary MYI/FYI mask. The increase of MYI ice volume they observe could be well not MYI but FYI that growth in the leads or is otherwise integrated within the MYI within a 25 km grid cell. In summary, I don't think their conclusion that MYI volume is increasing in most months during winter is correct. Sea ice export should dominate the MYI volume change and cause it to be negative almost always.

Detailed comments: p1,l23: better split in two sentences. Sounds like the definition of

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MYI is connected to ice export.

p2,l1: add a sentence explaining how storms reduce sea ice. p2,l4: "Multiple" you only name one. p2,l13: what do you mean by "parametrization"? These studies were based on ULS ice thickness measurements.

p3, Table 1: the table should also include the name used for the three products in the text, i.e., OSISAF, IFREMER, NSIDC

p4/5, 2.3: Please discuss potential errors of MYI classification. You make quite strong use of the MYI dataset throughout this paper. However, in the convergent zone of Fram Strait, where ice gets deformed and broken up in smaller floes, ice type identification gets less reliable (surface scattering can dominate the volume scattering used for MYI type identification). Uncertainty estimates should be mentioned here and also more critically discussed later in the paper (e.g. in 3.1) when the ice types are analysed.

p5, l13: if you average them you wouldn't get the monthly displacement but the mean 48h displacement. p5, l19: hm, the gates are not aligned with the grid. The gates then would be not smooth lines like in the figure but step-wise functions, right? I think that makes flux calculations unnecessary complicated (see below). p5, eq. 2: why is l kept constant at 25km? Depending on the direction of the meridional or zonal component l can increase to sqrt(2)*l = 1.4*l=35km at 45° or not? p5, eq. 3: again, l should not be constant. With 40% the changes in effective length of your grid cell, which you did not take into account, could well dominate your error. p6,l1 and ff: Do you apply the error functions from that paper to your datasets or do you use their values? Do these error values then vary in space and time? Shouldn't the error function depend on the drift dataset or dis Sumata evaluate all your three drift datasets? Please provide some more information and clearer description of method used here. p6, Figure 1: make figure larger (full page), arrows are hard to see.

p7, Figure 2: make larger

p8, Figure 3: make larger

p9, l8/9: Do you mean Table 3? p9, l18: this is definitely related with the EGC location, which floes along the shelf edge (haven't checked but which probably is at 6W at that latitude then). See e.g. papers by de Steur et al.

p10, l5: SIC is not shown in Figs 2 & 3. What do you mean? p10, Table 3: Explain what % MYI means (maybe better in the text). Is a grid cell MYI if there was any MYI detected within the month or does it have to be >50%? What are the uncertainties of this MYI % values? Also, how does the MYI product define the MYI ice type? Is >50% MYI fraction within a grid cell considered MYI or 100% etc.?

p11, l4: here you are reporting on per grid cell values. That should be mentioned. Because without the grid size of 25km these values are quite hard to set into relation for the common reader. p11, l10: see comment for Table 3. Explain better what "majority fraction MYI" means.

p11, l19. What are the correlation coefficients? Actually, the exports correspond less from what I would have expected from Fig. 6a. Please explain in more detail why, e.g., NSIDC in 11/12 goes down while the other two go up.

p12, l6: I am not sure I agree with that conclusion. If NSIDC goes down in 11/12 while the other two go up, the variability is quite different, or? Similar for 13/14 to 14/15, where OSISAF & NSIDC show strong increase and IFREMER is more neutral. p12, l7: It is understood that you do not make a ice drift dataset validation study. However, your export estimates do not agree within their uncertainty estimates. Which means either your uncertainty estimates are wrong or you need to justify why you trust one dataset more than another. To me also the inter-annual variability in Fig. 6b is quite different for the three products. Some explanation for that should be added. p12, l13: in

p13, Figure 7: shades of gray are hard to discern; caption: remove (b) at the end.

p14, l1: Not "on the other hand". This argument also supports moving the gate north.

p14, l19/20: this is supported by the overlap of STD of this study with previous studies for most months. p14, l22/23: are there estimates of ice thickness gradient between 80 and 82°N? What gradient does CS2 show? Can you estimate the thickness gradient to support this argument? p15, l15: Have also a look at Kwok et al. (2013), which analyses ice area export in connection to different atmospheric indices (AO, DA).

p17, l5: what happened to the 7th year 16/17? p17, l6: "Scattergram" p17 l10: See my mayor comment at the top. I don't think this is correct. dV_MYI/dt should almost never be positive. p17, l15. hm, that is maybe correct. The word "variations" is not very well defined, maybe better standard deviation? However, only 29% of the variance of dV_MYI/dt is explained by Q_MYI. p17, 26: yes, there are some similarities in their variability but actually their variability differs quite significantly and they do not agree within their uncertainty estimates. I find this conclusion too positive or at least need some explanation of the problems. Having results that do not agree within their uncertainty but not to mention that I do not find acceptable. Actually, I would prefer that you guide the reader which estimate they should use or you have to increase the uncertainty estimates.

p18, l7: "explained" to what degree? Give numbers. p18, l9: How is "variability" defined if quantitative numbers are given here? p18, l11-13: I do not agree with this point. See explanations above.

p19,l17: please provide information how and where to obtain this user guide.

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