Response letter to Anonymous Referee #2

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.

We thank the reviewer for these thoughtful comments which helped to improve the paper. We have revised the paper substantially to address the comments from both reviews. The main changes are listed in the following:

- We have carefully revised the computation of the ice volume flux and checked the results.

- We have updated the NSIDC ice drift data set, since it is now available until February 2017, which allowed us to update the NSIDC ice volume export time series until the 2015/2016 season. 2016/2017 has not been computed, since March and April are still missing in the NSIDC data.

- The Methods section has been revised for a more detailed description of the ice volume export calculations, especially of the uncertainty estimates.

- We have added a figure showing the ice concentration along the Fram Strait Gate, corresponding to Figures 2 and 3 for sea ice thickness and sea ice drift.

- We have revised section 4.4, discussing the impact of openings in the MYI zone, which might lead to a positive bias in MYI growth rates due to erroneously classified MYI.

In the following, please find our responses separately for each of your comment:

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.

We took into account the varying length of the grid cell. In Eq. 2 and 3, the grid cell length l=25 km is divided by cos(lambda), where lambda represents the longitude. Therefore, the varying grid cell length is l_uv = l/cos(lambda). We have clarified this in the text.

- 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).

We applied the uncertainty estimation according to the drift error function given in Sumata et al. (2015). For a better understanding we have included the applied equations in the revised version of this paper. However, this drift error function does not contain biases or systematic errors. These have been investigated separately in Sumata et al. (2015). We have added a paragraph to better explain the error estimates and potential biases.

- 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.

We agree that section 4.4 was lacking a discussion of potential errors due to openings and forming of new ice within the MYI zone that are not well captured by the ice type product. This indeed contributes to the residual term in Eq. 5. Therefore, we have added a paragraph for clarification.
However, we are convinced that thermodynamic ice growth still plays a role for MYI. It is true that 2 m thick snow covered sea ice does not show relevant thermodynamic growth anymore, but thickness of second-year or third-year sea ice can decrease to 1 m during summer melt. Then, during the freeze-up, ice grows again, even if slowly, until it reached ~2 m. Buoy measurements from the Arctic Basin do capture this behavior (Figure R1). This buoy data set covers two freezing seasons from August 2013 to August 2015 and shows how thickness of second/third year sea ice decreased during summer melt and increases again during winter. To conclude, both effects (bias due to openings in the MYI zone + thermodynamic ice growth) most likely play a role and are therefore represented in Eq.5. It is difficult to separate the two effects since quantification is rather difficult and not within the scope of this study.

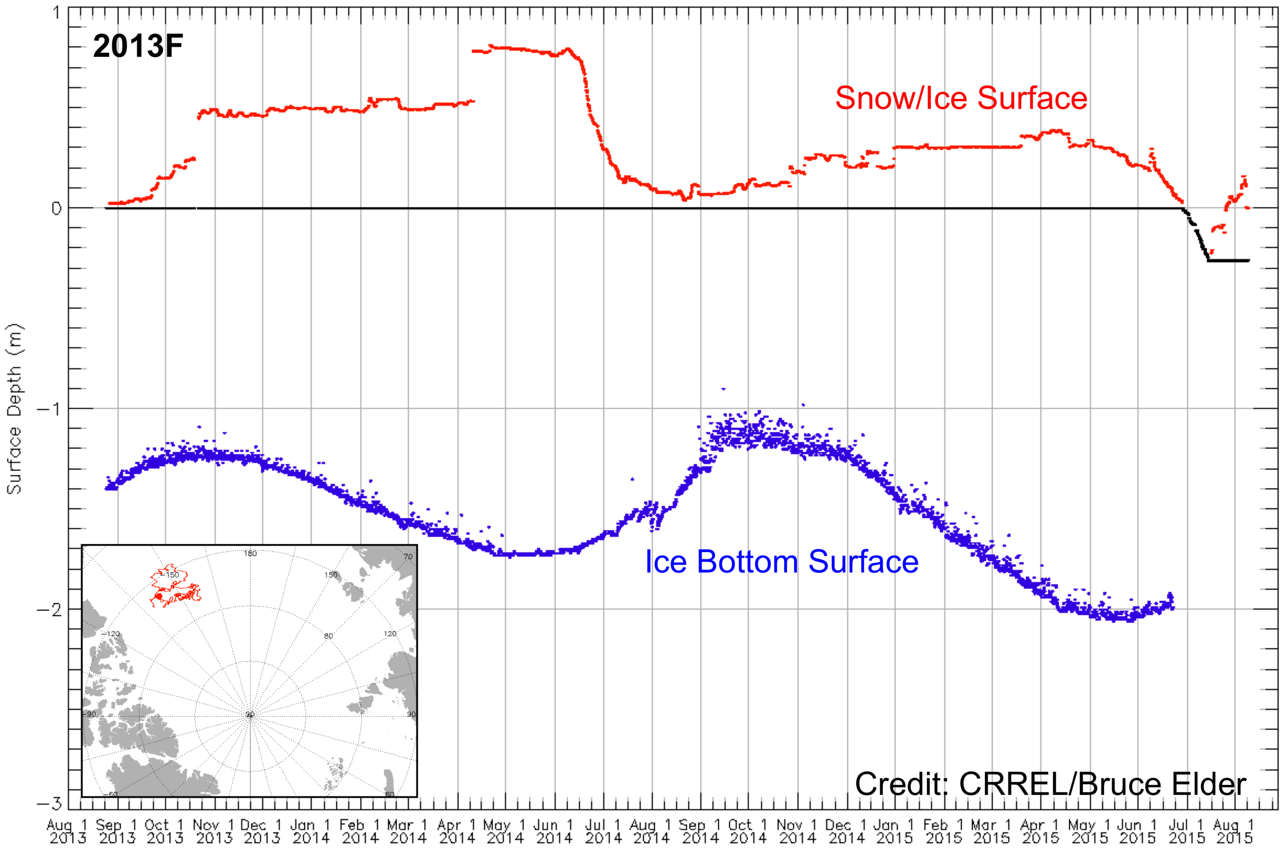

Figure R1: Snow/ice and bottom surface for ice mass balance buoy 2013F obtained from http://imb-crrel-dartmouth.org/imb.crrel/2013F.htm.

Detailed comments:

p1,l23: better split in two sentences. Sounds like the definition of MYI is connected to ice export.
Done. We also have restructured this part of the introduction.

p2,l1: add a sentence explaining how storms reduce sea ice.

We have added a sentence on this topic referencing a study by Parkinson et al., in which they have investigated the sea ice minimum in 2012.

p2,l4: "Multiple" you only name one.

Thank you for pointing that out. Corrected. We only refer to one study here.

p2,l13: what do you mean by "parametrization"? These studies were based on ULS ice thickness measurements.

„Parametrization" here means that thickness across Fram Strait is estimated using ULS ice thickness measurements from distinct locations in the Fram Strait.

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

Yes. Fixed.

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.

Thank you for pointing this out. The quantification of ice type errors is difficult because it might vary temporally and regionally, also depending on external factors. We have added paragraphs/ sentences in the relevant sections (2.3, 4.4) to discuss the uncertainties of the ice type products more carefully.

p5, l13: if you average them you wouldn't get the monthly displacement but the mean 48h displacement.

We agree that this formulation was misleading, and we have changed it.

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?

We calculate the export at the meridional (zonal) gate considering the line section crossing each grid cell. The length of the grid cell is thus a function of the longitude, which is considered in Eq. 2 as mentioned above. We have now changed the formulation and introduced l_uv = l/cos(lambda) to clarify this point.

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.

l/cos(lambda) does account for the changing length of the grid cell as a function of longitude. See above.

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.

We do apply the error function from their paper. They provide tables with error estimates of drift in x and y directions for different categories of ice concentration and ice drift speed for all three data sets. We have added a more detailed description of how we retrieve the errors.

p6, Figure 1: make figure larger (full page), arrows are hard to see.

Done.

p7, Figure 2: make larger

Done.

p8, Figure 3: make larger

Done.

p9, l8/9: Do you mean Table 3?

Yes, indeed. Thank you. Corrected.

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.

We have checked this, and included another reference by Steur et al..

p10, l5: SIC is not shown in Figs 2 & 3. What do you mean?

Thank you for pointing this out. This was indeed not entirely clear from the text. We have now included a figure also showing the ice concentration at the gate for the entire time series to clarify what is meant here.

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.?

The OSISAF ice type product only provides binary values for ice type, e.g. FYI or MYI. The percentage gives the fraction of grid cells that indicate MYI along the gate. We have added a paragraph in section 2.3 to shortly explain how the product is derived and what the uncertainties are.

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.

Thank you for pointing this out. We have added: „The maximum values have to be considered in relation to the 25 km grid resolution and are likely different on smaller scales".

p11, l10: see comment for Table 3. Explain better what "majority fraction MYI" means.

We added a paragraph in section 2.3 for better explanation.

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.

Figure 6a only shows the export along the gate and reveals a strong correlation between the products. We have included monthly export estimates using the different drift estimates. The correlation coefficients between the monthly estimates of the different products are > 0.9 and indicate similar variability. However, it seems that there are seasonal biases, especially considering NSIDC in 2011/2012.

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.

As mentioned above, this is mostly due to seasonal differences. Considering the monthly export rates, the correlation coefficients between the products are > 0.9.

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.

The estimated drift uncertainties do not contain potential biases. In Sumata et al. (2014, 2015), it is shown that these drift products are subject of systematic errors. The reason why we use OSISAF here as a reference is that it shows the best performance in the Fram Strait among the products used in this study. On the other hand, it is shown that the three products coincide quite well regarding the monthly variability (correlation between the products > 0.9). Therefore, the main results of our study are independent of the used drift product.

p12, l13: in

Corrected.

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

Fixed. We have chosen different line colors in the plot to increase readability.

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

Yes, this word choice might be confusing. Corrected by using “in addition” instead.

p14, l19/20: this is supported by the overlap of STD of this study with previous studies for most months.

Yes, exactly. Exceptions are March and April. But in addition, also the other factors (2-5) will probably play a role here.

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?

This definitely deserves more research. However, using CryoSat-2 thickness estimates for this purpose is not straight forward. South of 82°N, thickness estimates become more and more uncertain, due to the lower CryoSat-2 orbit coverage and higher ice drift.

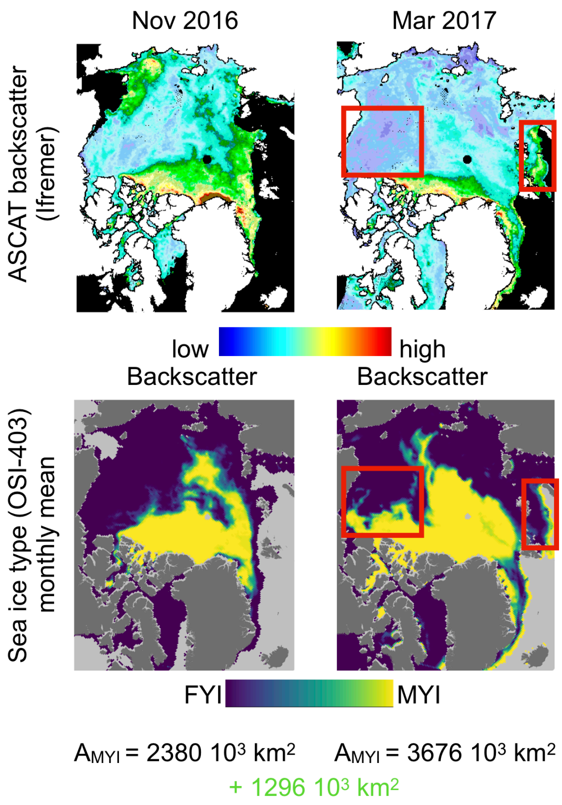

Figure R2: Monthly ASCAT backscatter maps from Ifremer and monthly averaged sea ice type from OSISAF for November 2016 and March 2017. MYI area shows a gain of more than 50% from November to March 2017, which seems unlikely.

p15, l15: Have also a look at Kwok et al. (2013), which analyses ice area export in connection to different atmospheric indices (AO, DA).

Thanks for pointing on this study. We have cited it with regard to the AO and its linkage to ice export.

p17, l5: what happened to the 7th year 16/17?

This winter season 2016/2017 has been excluded, because some obviously erroneous MYI classification has been observed, see Figure R2. We have been in contact with OSISAF therefore, and they are aware of this behavior. It might be linked to the unusual warm winter in 2016/2017 and needs further investigation. We have added an explanation in the text.

p17, l6: “Scattergram"

We have removed this sentence to reflect changes to the figure.

p17 l10: See my mayor comment at the top. I don’t think this is correct. dV_MYI/dt should almost never be positive.

As stated above, we think that thermodynamic growth can play a role also for MYI, certainly if it is undeformed second-year ice with a decreased thickness after the summer melt. But surely, the uncertainty in the ice type discrimination plays an important role, too. Therefore, we reformulated this paragraph.

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.

We have already calculated R^2. Therefore, it gives us the percentage variation in dV_MYI/dt explained by Q_Ex_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.

As mentioned above, the uncertainty estimates do not include a bias correction and the main difference between the products is due to systematic differences. However, the correlation between the monthly volume export derived with the 3 different drift products is > 0.9. We have included the correlation coefficients between the products in the paper now. In the beginning of section 3, we refer to Sumata et al. (2014), which shows that OSISAF ice drift reveals the best performance in the Fram Strait.

p18, l7: "explained" to what degree? Give numbers.

We now state the correlation coefficients.

p18, l9: How is "variability" defined if quantitative numbers are given here?

We acknowledge that using „variability" in this context is a bit confusing. It should be Arctic MYI volume change, since we consider dV/dt.

p18, l11-13: I do not agree with this point. See explanations above.

As mentioned above, we agree that a potential bias due to erroneous ice type classification could affect dV_MYI/dt. We have removed this conclusion from the list.

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

We have added a link that directly points on the pdf. Thank you for pointing on this.