The study explores ice area and volume fluxes into, around, and out of the Canadian Arctic Archipelago (CAA) using a combination of remote sensing datasets. Ice motion data are derived from SAR imagery for deriving area fluxes and thickness estimates from CryoSat-2 around the primary gates and an ML random forest model for the inner gates. The paper follows on from a study by the same author in 2022 deriving the new ice area flux estimates from Sentinel-1 and RADARSAT RCM (Howell et al., 2022), a paper in 2022 looking at MYI conditions in the CAA (Howell et al., 2023a), then a paper in 2023 looking at similar ice area/volume fluxes of the CAA and Nares Strait by integrating the new CryoSat-2 thickness data with the previous area flux estimates (Howell et al., 2023b).
Overall, the analysis in the paper was well presented and relatively easy to follow and the science aligns well the scope of The Cryosphere. My comments are provided below, I hope these make sense and help improve the manuscript.
Alek

Howell et al.
We thank Dr. Petty for the review comments and have tried to implement all of the suggestions.

General points:

1. The first thing I think worth flagging is how closely aligned this paper is with the 2023 JGR study (Howell et al., 2023) that derived similar estimates of ice area and volume fluxes using the same method and over the same time-period but also comparing to Nares Strait. The methods sections of both papers are virtually the same, which I think is fine, but I was surprised there wasn't a clearer note to this effect in this paper. The addition in this study seems to be the CAA inner gate area/volume flux estimates that were not included in the 2023 study and the generally increased focus on the CAA results. Still, it took me a bit of a while to realize that and I think this paper should tie together much more with that study and make clearer what the relative goals of each study are and what exactly is new in this study. Similarly, the MYI replenishment section didn't refer to the author's recent paper on CAA MYI replenishment which seemed a little odd too. Put another way, the author and author team have been looking closely at area/volume/MYI fluxes around the CAA for a while now, so what was the gap that this study needed to respond to and how did it build on the preceeding efforts?

Howell et al.
The previous paper (Howell et al., 2023b) only focused on Arctic Ocean export (i.e. inflow into the Canadian Arctic domain). This paper is focused on inflow and outflow for the CAA together with internal flux estimates. As for MYI replenishment, it was only looked at in this context in the 2015 paper (Howell et al., 2015). The recent MYI paper used Ice Charts to estimate dynamics, here we can use SAR to get more robust estimates. Altogether, we understand the Reviewer's point and have revised accordingly as follows:

Specifically, previous studies using SAR imagery have only been able to quantify ice flux between Arctic Ocean and CAA (Kwok, 2006; Howell et al., 2013; Howell and Brady, 2019) ignoring ice flux between the CAA and Baffin Bay. This omission also constrained MYI replenishment estimates within the CAA (Howell et al., 2015). Limited SAR image availability has also prevented ice flux estimates from the QEI to the Parry Channel which is a key part of the Northwest Passage. Using SAR imagery from Sentinel-1 and RCM Howell et al., (2023b) was able to provide year-round flux estimates between the Arctic Ocean and CAA but did not consider the ice flux between the CAA and Baffin Bay.

2. I'm not a fan of the uncertainty bounds approach, especially as they aren't really used (hard to plot an uncertainty bounds in a time-series plot!). I really think you should just pick your best guess uncertainty estimate and justify it as best you can – I don't particularly believe the bounds truly represent realistic bounds anyway. It would be good to then use those values on the time-series plots you show to get a sense of how important the uncertainties are for assessing seasonal/annual variability. However, I do have some additional concerns about the uncertainty quantification:

Howell et al.
Agreed. We selected the upper bound uncertainty and applied it to all Figures.

1. What about the errors in ice concentration (L158)? I would guess they are not negligible considering how small some of the channels are, but maybe this was addressed in one of the recent papers.

Howell et al.
Ice concentration estimates are from the Canadian Ice Service (CIS) ice charts which do not suffer from the problem of small channels and surface melt ponds like passive microwave (e.g. Agnew and Howell, 2003). Further, since 1995 they are derived entirely from high spatial resolution synthetic aperture radar (SAR) imagery. They are often used as validation (i.e. truth) so one could argue the errors in ice concentration are negligible compared to other uncertainty components of the flux calculations.

2. Seems quite odd to assume the ice motion errors are uncorrelated if derived based on the same image pair? But then you later add the errors to generate the monthly uncertainty estimates so you assume they are correlated? Are these assumptions justified better in previous papers?

Howell et al.
These are widely used assumptions based on previous papers (i.e. Kwok and Rothrock, 1999; Kwok, 2006; Agnew et al., 2008; Kwok et al., 2010) that we acknowledge. It is indeed an image pair, but each image is different so the uncertainty estimates between spatially adjacent pairs are assumed to be uncorrelated. We are not adding errors but estimating the error per month based on the number of observations (i.e. image pairs) which is approximately 1 per day or ~30 per month, and the daily ice motion uncertainty estimates are assumed to be correlated.

3. The use of the linear trend to fill in the summer months for the inner gates seems very crude, especially as this seemed to be one of the big differences with the 2023 JGR study. At the very least I think the paper would benefit from showing what the raw and interpolated thickness values actually look like at each gate and how justifiable they are. Any way you could tie it together with the all-season CS-2 data outside the CAA?

Howell et al.
Indeed it is crude but there is an overall lack of sea ice thickness measurements in the CAA except for a few winter survey's (Melling, 2002; Haas and Howell, 2015), landfast ice locations (Howell et al., 2016). In the summer months there is pretty much nothing. Therefore, we would like to keep the volume flux estimate in the paper because it would be considered a baseline estimate until it can be refined. Linear interpolated thickness uncertainty is unquantifiable in a meaningful way therefore, we provide a Figure (Figure 3) to indicate and state that the volume flux estimates are likely overestimated over the annual cycle and have revised the text as follows:

Since sea ice thickness values from the proxy ice thickness dataset were not available from May to October, accordingly we used the linear trend of April to November to approximate those values. This follows the climatological record of landfast ice thickness loss through melt in summer that was reported for Eureka by Dumas et al. (2006) in their Figure 9. The reduction in ice thickness at Eureka follows an approximate linear trend between June and September. Figure 3 here shows a time series of plot of CryoSat-2 sea ice thickness at the QEI gates (in black) followed by the time series proxy ice sea ice thickness at Byam Martin Channel (in red), and the linear interpolated ice thickness at Byam Martin Channel (dashed red). Note the thickness decrease with latitude is similar to what has been reported in previous studies (Melling, 2002; Haas and Howell, 2015). With the exception of 2016-2017, the linear thickness approximation likely overestimates thickness for 1-3 months per year (Figure 3) and as a result the inner gate annual volume flux estimates are likely to be overestimated.

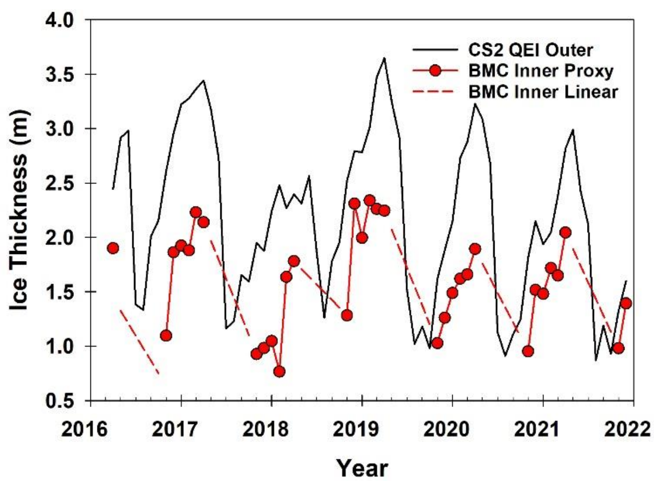


Figure 3. Time series of CryoSat-2 s ice thickness for the Queen Elizabeth Islands (QEI; black) outer gates, proxy ice thickness for the Byam Martin Channel (BMC, red) inner gate, and linearly interpolated ice thickness for BMC inner gate (dashed red) from 2016-2022.

4. It would be good to get a better sense of how important the results are to the thickness estimates, I'm guessing there is some skill in the seasonal thickness cycle

in the input datasets but not much de-seasonal skill beyond that considering the errors.

Howell et al.
Adding the ice thickness data for volume instead of area estimates provides the additional dimension and less uncertainty than the ballpark estimates of e.g. early Kwok paper who lacked thickness estimates. These ballpark estimates currently act as the baseline for SIV fluxes within the CAA.

It is true that the ice thickness estimates are more skillful at measuring the average seasonal cycle than measuring interannual variability. However, in both papers describing the ice thickness datasets used here, Landy et al. 2022) and Glissenaar et al. (2023), the authors provided anomaly correlation coefficients of sea ice thickness time series versus in situ draft/thickness estimates obtained from mooring-based ULS or landfast ice drill hole sites. For both products, the ACCs were lower than the correlations for seasonal time series, but all positive and between 0.11 and 0.51, meaning that oscillations between higher- and lower-than-normal thickness are always captured and for some locations the magnitude too with significant skill. Our SIV fluxes will integrate any skill capturing the IAV, which should be an improvement compared to using ballpark or climatological mean thickness data. This point has now been emphasized in the manuscript.

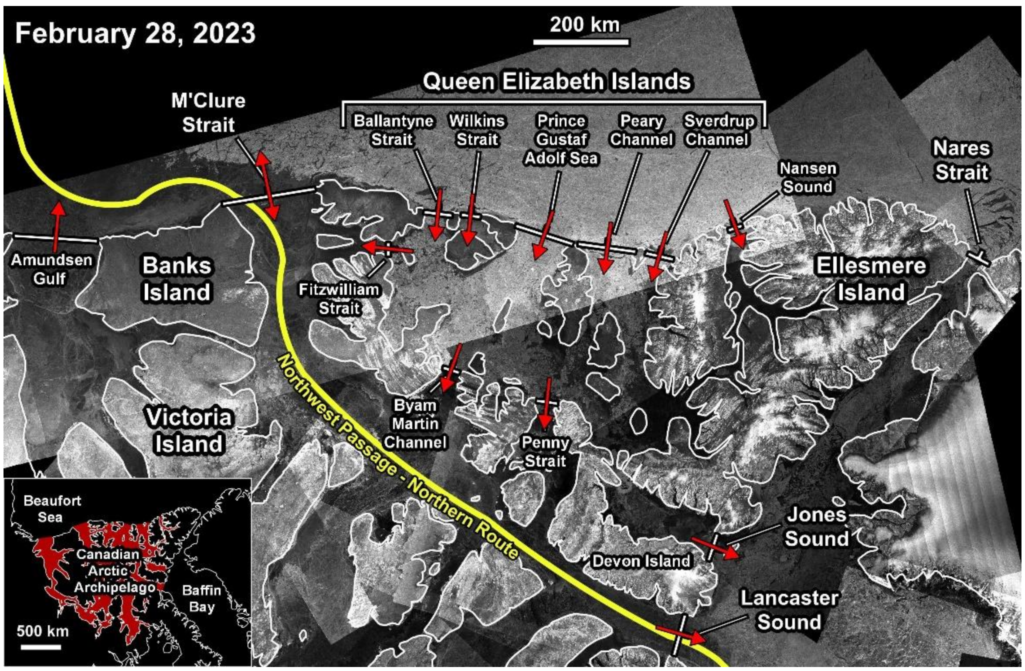
3. L266-266 on QEI area vs volume import/export I view as the most interesting idea from the paper but think it should be explored in much more detail to help justify this paper. How thick does the ice need to be north of QEI for the assumptions of net volume sink to be true? Do we think we're approaching an inflection point of this not being true anymore? I think it would be easy and quite illuminating to run a little sensitive test here changing the ice thickness values north of QEI and re-running the analysis. I don't know the cited Melling 2022 study that well but the claim that the thickness isn't changing north of QEI is a little surprising to me, but eventually I think we can agree it's quite likely to change after a big MYI flushing event. In general, I think the paper would be much improved if you could test some hypotheses out in this framework rather than just showing raw data and discussing ideas.

Howell et al.
We appreciate the Reviewers suggestion and agree import from the QEI has the most impact on the volume balance of the CAA. However, it is not really how thick the ice is but rather how much ice comes in. This is controlled by the timing and collapse of ice arches together with winds. The ice north of the QEI is generally thick experiencing only very slight (not significant decline) since? 2016 (see Figure 3 above). So again, ice duration and wind are the key drivers not thickness during this time period. We added this following to the discussion:

Overall, the CAA appears to be a strong ice area exporter and a lower ice volume exporter when appreciable ice from the Arctic Ocean is imported to the QEI. Ice thickness in the vicinity of the QEI has only experienced a slight (not significant) decrease over our study time period (Figure 3). Therefore, the CAA's net volume import in certain years not likely a result of how thick the ice is but rather how much thick ice is imported into the QEI which is a function of ice arch duration and atmospheric circulation patterns (i.e. wind).

4. Finally, on a similar theme, it's quite hard as a reader to intake all the different flux estimates and get a sense of what it all means. Most of the figures don't really much of a compelling story or scientific result. The discussion and conclusion sections do help but they are not very visual. A map schematic showing the area and volume flux estimates for your study period I think could help a lot?

Howell et al.
Interesting suggestion but with such inter-annual variability this is difficult to depict on a schematic. We did however place arrows on Figure 1 (below0 indicating the dominate direction of transport which should help readers interpretation throughout the paper.



Specific comments:
L33 – are goods actually transported through the NWP?!'

Howell et al.
Yes. Many communities in the Canadian Arctic receive their supplies via ship.

L60 – but then at L66 you say people have done this also using AMSR-E. So, what resolution do we truly need for this kind of analysis?

Howell et al.

89 GHz enhanced imagery at 2.2 km spatial resolution. But Kwok has done it with 6 km. The problem is resolving the summer sea ice conditions is challenging and lower resolution is bias to slower speeds.

L64 – what exactly do you mean by images not being consistent in space and time?

Howell et al.
Change to spatially and temporal uniform to construct ice flux estimates of over all the gates. For example, image availability from RADARSAT-1 and RADARSAT-2 was typically only available with high temporal resolution in certain regions of the CAA and mostly only during operational months of June to October. As a result, a complete picture of sea ice dynamics of the CAA over the entire annual cycle was not possible.

L68. – The relative benefits/merits of S-1/RCM vs R1/R2 for doing this area flux analysis is still a little unclear to me, was this explained better in the earlier papers?

Howell et al.
We addressed this in a previous comment (General Comment 1). Hopefully the benefits of S1/RCM vs R1/R2 are now clear.

Figure 1 – I think it would be good to highlight Baffin Bay/Beaufort Sea as they are mentioned in the text too, appreciate the figure may need to be zoomed out a little more.
Generally confused by the discussion of Parry Channel but Peary Channel being indicated on the map?!

Howell et al.
Parry Channel is correct. The Parry Channel was named after William Edward Parry who "almost" discovered the Northwest Passage. We went with an inset. See response to previous comment.

L96 - this discussion of the all-season CS-2 data is a little odd, you are just using the data from the 2022 paper right, so shouldn't that be cited first? The rest is background to that study.

Howell et al.
Agreed. Rephrased accordingly.

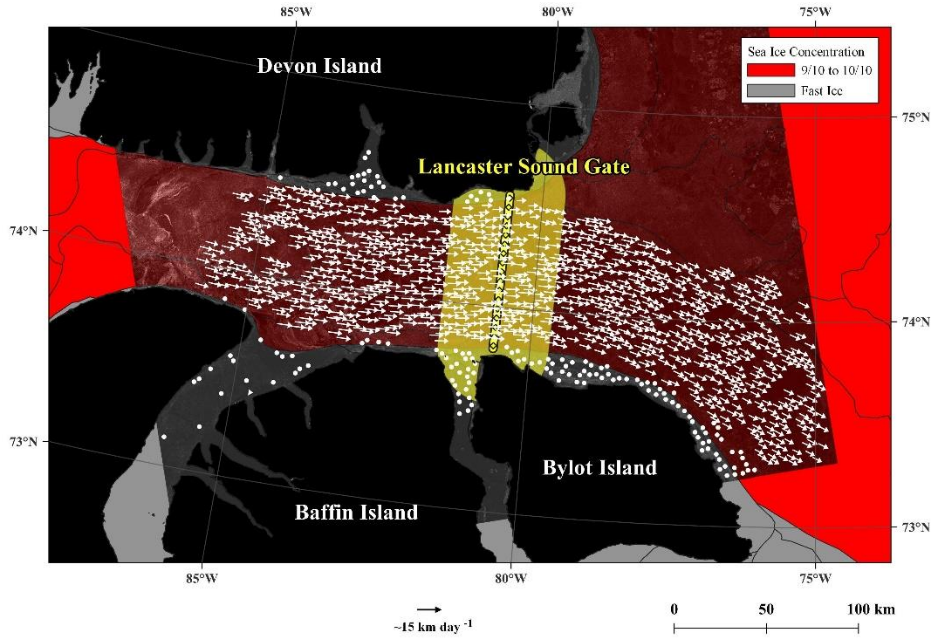
L106 – what are the open seas of the CAA?
Howell et al.
Rephrased to marginal ice regions.

The CAA thicknesses from the Isolde 2023 paper basically show a seasonal cycle of ~60 cm to 160 cm, errors in the proxy data of 30 cm?

Howell et al.
That is a bulk value. Errors range from 30 to 50 cm.

L28 - Not quite sure what you mean by buffer region, this seems maybe a bit colloquial. Can you be more explicit?

Howell et al.
We have added a Figure to illustrate the “buffer region” as follows:



L143 – what are the units of that? Km/day??
Howell et al.
Displacement error should just be km.

L160 – this is confusing as you don’t reference the proxy data which I believe is what you are using for the inner gates here. I think in general it would help to show what these data look like for a given gate as a case study – show the area flux, the thickness then the fluxes for a given season with those applied uncertainties. Would help us visualize the variability in the source terms and how it relates to the variability in the flux terms.

Howell et al.
Agreed. We added a Figure to help visualize the uncertainty. See previous response.

Figure 2 - How well correlated are the area and volume fluxes? Could also show the thickness/are variability too. As stated in the general points, also unsure why the uncertainties are not shown. I think just take your best guess uncertainty and include that in the figures, would help to visualize how they compare to the variability of the signal.

Howell et al.
Agreed. Added uncertainty to all Figures.

L260 – Shackleton would be turning in his grave over that comment! But seriously, I would guess there is a good chance there could be thicker ice in the Weddell Sea..?

Howell et al.
Perhaps not as north of the CAA and Greenland is likely thicker. Nevertheless, we added "Arctic."

L318 onwards - I feel like the MYI replenishment ideas could benefit from knowing how much MYI is lost too? Struggling a bit too put the replenishment numbers in context.

Howell et al.
Agreed but it is very tricky to estimate how much MYI actually melts. Especially quantifying how much MYI is advected into the CAA and melts has too much uncertainty associated with it. As it stands, replenishment is a function of advection from the Arctic Ocean and local FYI aging.

L359 – again I think here is where you could benefit from a better understanding of how sensitive this result is to the underlying thicknesses.

Howell et al.
As previously discussed, it is not so much thickness by how much is allowed to pass through the northern CAA (QEI region) which is controlled by ice arch duration and wind.

Figure 3 and 4 – it would be quite easy and I think much better to read if you combined these, put the volume flux alongside the area flux bars with a twinned y-axis on the other side. Ideally it would be good to see the area and thickness numbers too.

Howell et al.
We prefer to keep them separated.

In several figures you should add the exponent multiple to the label and make the figures consistent in this regard.

Howell et al.
We do not label for volume because of smaller values. We assume that is point raised by the Reviewer.