Reviewer# 1
2nd Review of "Sea ice transport and replenishment across and within the Canadian Arctic Archipelago: 2016-2022" by Howell et al.

Thank you very much for the revised version. I think most of the points have been addressed. It is appreciated that you updated the outer gates timeseries for volume flux.

The only point remaining is the interpolation between April and November. You refer to Dumas et al. (2006), but I would disagree with "The reduction in ice thickness at Eureka follows an approximate linear trend between June and September." Where is this stated in the paper or shown in the figure? Actually, it seems that observations only cover the winter season until late spring. Moreover, these are historical data from < 1990. Actually, the observations show even an increase until mid-May. Then they apply a thermodynamic model, which indicates linearity only over may be max. 2 months.

So, I find the method of liner interpolation still not very convincing. I believe applying a simple thermodynamic model would already shed more light on the process between April and November. I understand if you thought this is not within the scope of this paper. But then, if you want to keep the linear interpolation, I think there should be more discussion on the biases due to the linear interpolation. For example, how much (quantify) do you expect over/underestimation of ice thickness and volume fluxes in certain months?

I will indicate minor revisions, but I think this last point should be addressed before publication.

Howell et al.
Fair point. However, running a thermodynamic model in this region is not straightforward given the mix of ice types with the CAA (i.e. 50% MYI on average) and indeed not within the scope of this study. The Dumas study was meant to show the linear sea ice (landfast) decline with the CAA but admittedly is not representative within these regions. In these regions, the ice is not stationary and is continuously flowing out of the QEI being subsequently replaced by thicker ice from the north (i.e. less seasonal ablation) over the duration of the melt season. As a result, this would be (is) very challenging to model because it would need to be a coupled model (dynamics and thermodynamics). We also think quantifying a monthly uncertainty is not possible. To that end, we have provided additional text on the potential problem with this method and we feel Figure 3 nicely shows potential under/over estimation in this region.

Revised text in the Methods section:

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. 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). Looking at Figure 3, it is apparent that the linear thickness approximation could overestimate thickness for 1-3 months during the melt season and underestimate for the remainder. This variability is influenced by MYI flowing through these gates that is replaced by thicker MYI from the north (i.e. less seasonal ablation) over the duration of the melt season. As a result, the estimated value of uncertainty for the proxy sea ice thickness dataset that ranges from 30 to 50 cm could fluctuate even more during the summer the months thus impacting volume flux estimates in the region. We acknowledge that quantifying this exact amount for the summer months is challenging and therefore there could be more variability in our annual volume flux values from the QEI to the Parry Channel.

Results Section:
It should be noted that there is more uncertainty in the inner gate volume flux estimates compared to the outer gates because we use linearly interpolated ice thickness values during the summer months and as a result, the uncertainty range shown in Figure 10b could be more variable.

Reviewer #2
Thanks to the authors for addressing the concerns from my previous review. Happy to see this published but with a few minor/technical points below I think could be addressed first (I don't need to see this again!).

I'm pleased you changed the uncertainty approach but it seems the text doesn't exactly match how this is reported in the tables now (e.g. you say it's calculated from a. range in sigma u)? It looks like you followed the recommendation to just report a single value for each gate that represents the monthly mean uncertainty for each gate, but you use the upper bounds of the input error terms, right?

Table 1 and 2 captions also need fixing. I also think the values are also stated to too high a precision (1 s.d. probably sufficient) considering these now represent average uncertainties for the gates.

Howell et al.
We cleaned the text mix-up associated with the uncertainty bounds. The precision conforms to a level previous studies report and is fine.

Reviewer #2
The new reference to the Dumas (2006) paper doesn't make sense to me. What exactly are you trying to say here? I looked at that paper and figure which didn't help!

Howell et al.
Agreed. Removed.

Reviewer #2
I still think the description of the CryoSat-2 data is confusing. I think just state you use the all-season dataset from the Landy et al., (2022) paper. Then you can provide a line or two describing the origins of this datasets.

Howell et al.
Agreed. We just made it simple as follows:

Year-round sea ice thickness estimates for the outer flux gates were acquired from the CryoSat-2 radar altimeter from October 2016 to September 2021 (Landy et al., 2022). All CryoSat-2 ice thickness data are available from https://data.bas.ac.uk/full-record.php?id=GB/NERC/BAS/PDC/01613.

Reviewer #2
A few types with the new additions that need to be fixed.

Howell et al.
Cleaned them up.