General

I have found it helpful to receive these two thorough and thoughtful of the original manuscript. I thank them.

My plans for the manuscript in response to these comments are outlined below.

Reviewer 2

Major

The comparison between the two periods (1970s and 2009/2010) seems to me a bit tricky … Recent multi-year ice thickness is represented by data from one-mooring site with short period (2009-2010). … I would suggest the author to provide more careful examinations to strengthen the conclusion of this study … Since the manuscript focuses on long-term changes of multi-year ice thickness, sea ice growth estimates by climatological forcing may be problematic … I suggest using specific forcing from 2009/2010.

I share the discomfort of both reviewers about the seasonal "correction" of ice draft.

See the comments below in relation to lines 286-292, and also above in relation to the first reviewer's second comment.

Minor

Line 47: It would help readers if the location and area of Sverdrup Basin is annotated in Figure 1.

The domain of Fig. 1 is the Sverdrup Basin. I will modify the caption to state this.

Line 65: Bathurst Island and Norwegian Bay could be also highlighted, otherwise readers have to take time to find them in the map.

I acknowledge that it may take a little time to find labelled features. However, the geography of islands and channels in the Canadian High Arctic is complicated. Given the number of geographic features referenced in the text, I believe highlighting would make things less clear, not more so.

Line 93: Queen Elizabeth Islands could be also annotated in Figure 1.

The domain of Fig. 1 covers the QEI. Therefore I will modify its caption to state this. See also my response to comment #1

Line 93-94: I agree that the meaning of "cut off the supply and the waters between the Queen Elizabeth Islands could be a very different place" is obscure.

I will revise the sentence to "The ice regime of the Sverdrup Basin would be very different if in-drift of Arctic Ocean ice were to cease".

Fig. 3: As recommended, I will switch to SI for the weights and measures on this illustration.

Line 143-145: I suggest to mark the position of the mooring in Figure 1 or another large-scale map. I would suggest to show this in a closer map showing bottom topographic features, e.g., maps covering the area shown in Figure 5.

The exact position of the mooring in Penny Strait is already marked on fig. 6; albeit, the symbol used is too small. I will also add the mooring's coordinates to the text in section 2. In reality, awareness that the mooring is on the western side of Penny Strait is sufficient in the context of this paper. As for seabed topography, it is largely irrelevant to this discussion because sea-ice drift atop highly stratified Arctic waters is little influenced by it. Moreover, an additional large format figure (in an already lengthy paper) would be required to display the complicated terrain of Penny Strait.

Figs. 5 & 6: I would suggest to show coastline on top of the image, if possible.

I understand the difficulty of distinguishing land from ice if not familiar with the geography. I will experiment with a coastline overlay to see if discrimination can be improved. I will also enlarge the red star marking the mooring on these figs.

Line 252: Why 0.75 m is used as a threshold to identify multi-year ice?

As evident in fig.9, a draft of 0.75 m marks the low-point in the histogram for all ice measured, both old and seasonal. This low point is the logical choice for this discriminant. I will make this clear in the text.

Line 257-261: Is it possible to annotate these features in Figure 8, which helps reader to understand the temporal changes of ice type.

Yes. I can readily delineate the various segments using background shading on the plot.

Line 286-292: Why has the author applied climatological data to derive the ice growth rate? I suppose that a calculation using the data from 2009-2010 could provide more accurate estimate that takes into account specific condition during the observation period.

I share the discomfort of both reviewers about the seasonal “correction” of ice draft. Unfortunately, this is a thorny problem that has no ideal solution. Indeed the authors of the landmark paper on the thinning of the Arctic perennial pack (Rothrock Yu & Maykut, GRL 26, 1999) made a similar remark and took an approach similar to my own, using a 40-year climatology spatially averaged across much of the Canada Basin. However in contrast to my correction, they lacked snow-depth data to account for the strong influence of snow on the thermodynamic growth and ablation of sea ice. They state:

> This [seasonal] cycle was derived from an ice-ocean model with a 12-category ice thickness distribution [Zhang et al., 1998]. The model was forced with 40 years of winds and temperatures from the NCEP reanalysis, and thickness was averaged over those 40 years, over the SCICEX data release area …, and over the thickness distribution (including open water)

Their adjustment of measurements to a reference date of September 15 considerably changed mean thickness values, by as much as 0.6 m.

Snow depth exerts a dominant control on the thickening of sea ice during winter (see Maykut and Untersteiner, JGR, 1971; Dumas Carmack Melling, Cold Reg Sci Tech, 2005). Unfortunately, it varies appreciably with location and neither of the available long-term sites is close to Penny Strait; Resolute Bay is 230 km to the SSE and Eureka 520 km to the NE. It is my view that an ensemble of values measured over a long period can provide a useful measure of natural variability that is not available using one winter’s data from a specific location. It goes without saying that a single winter’s snow-depth development on sea ice at Resolute Bay would be of little relevance to that on ice in Penny Strait. I am presuming that the likelihood is greater for the climatology values.

I will undertake new seasonal “correction” calculations that incorporate the range of inter-annual variation in surface air temperature and snow depth, and will devise means to depict this as uncertainty on the figures that show seasonally “corrected data. The figures presently depict only the result of calculations based on long-term mean monthly values.

Line 289: I suggest to mark Resolute Bay and Eureka on Figure 1.

Yes, I can do this. Neither station falls within the border of the map, but both are close to it. These stations were chosen as the closest with the necessary long records of snow depth on sea ice.

Line 296-300: Is the uncertainty here significantly small compared to the difference of thickness from 1970s discussed later?

This comment joins that referenced to lines 286-292 in urging me to include an estimate of uncertainty in the seasonal “corrections”. I can answer this question when I have done this.

Line 314-316: How is the numbers shown here sensitive to the assumption (half populated by multi-year ice)? Is the ‘half populated by multi-year ice’ the ice situation in the comparison period (1970s)?

Certainly there is a sensitivity since the MYI here is appreciably thicker than the 2.3 m attained by first-year ice. However, it is necessary to choose some selection criterion. One is therefore faced with the usual challenge when parsing data, which is a loss of degrees of freedom (and therefore increased uncertainty) as one imposes tighter constraints. It would certainly be possible to explore the domain with thresholds of 40% or 60% old ice. However, I suggest that the 50% threshold already allows a possible low bias from up to 50% seasonal ice that is already making my premise of “little change in multi-year ice thickness” more difficult to demonstrate.

Line 317: “the area north-west of Penny Strait” is ambiguous. I would suggest to show tracks of 1970s survey in a map for clarity.

The 1970s tracks are too numerous to show clearly on an existing map and I am very reluctant to add to an already long paper. I propose to state in the text the distance to the centroids of prior data used, and refer the reader to fig. 7 of my 2002 paper for details.

Line 318: The standard deviation of each mean thickness in the 1970s should be also shown in Figure 12.

I agree. This figure will be revised.

Line 373 – 394: Though the mechanism of thick ice formation described here is plausible, it is not shown that mechanical forcing on ice pack has not been changed since 1970s. In order to strengthen the argument, I suggest to show that statistics of wind forcing (e.g., strength, variance) has not been changed between the two comparison period or to show statistics of buoy tracks (e.g., onshore drift speed) has not been changed.

I take the reviewers point, but this is not a practical suggestion given the present state of knowledge. Wind forcing does not in itself form ridges; it is the mechanical failure of ice and the piling of fractured ice in response to the force of wind that does. Regrettably, quantitative understanding of the mechanics of ice ridging is not up to the inter-decadal task proposed by the reviewer. Even accurate estimates of wind stress in this area, present and past, are plagued by lack of observations, not only of the basic field of sea-level pressure, but also of the polar inversion that suppresses turbulence-mediated stress transmission down through the atmospheric boundary layer.

Line 431: Table 4 → Table 3. Thank you

Line 455: 'commo n' → common. Thank you

Line 461 – 462: 'ice' is repeated before and after the bracket. Thank you

Line 473: This sentence seems to me a bit strange. Probably colon or semicolon could be used to split the sentence. Yes. It is missing a "because". Thank you

Line 485 – 486, "The build-up of pressure sufficient to build large ridges occurs within a few hundred kilometres of coastlines during strong onshore wind storms". I do find neither an analysis nor time series supporting this sentence in this manuscript.

The reviewer is correct. Although we know from the earliest days of Arctic navigation that pressure ridges form when ice is under irresistible pressure, this paper has examined only the kinematic response of the pack ice, not the dynamical forcing. I will modify the statement to something along the lines of "The extreme deformation of pack ice required to build large ridges occurs within a few hundred kilometres of coastlines during strong onshore wind storms".