Reviewer comments for "Recent extreme light sea ice years in the Canadian Arctic Archipelago: 2011 and 2012 eclipse 1998 and 2007" by Howell et al.

Recommendation – accept with major modification.

Reviewer #3
This manuscript examines several anomalously high and low ice cover years in the CAA and places them into a physical process context which draws upon thermodynamic and dynamic explanations. It is interesting work to pursue. I think there is a lot to be learned from these sorts of examinations, especially now that we have crossed into a different sea ice "era". Process understanding as developed for heavier ice conditions is probably not going to be sufficient, and analyses of more recent anomalous periods are of use. The manuscript requires a fair amount of work before it could be acceptable for publication. There is some analytical work that needs to be redone, and the process descriptions must be reworked and properly referenced. In particular the manuscript makes several physical process conclusions/comments that are hard to follow, not novel, or simply incorrect. These are noted below.

The manuscript also fails to follow up on some very interesting observations, the pursuit of which would provide additional physical insight. Large scale atmospheric flow and trough/ridging patterns were not explored as SAT anomaly driving mechanisms; this is a critical failing. The strong difference between 1998 and the other years was not pursued to the extent it could have been; in particular the MYI pattern difference was very interesting. Why is land-fast ice thickness not decreasing at Resolute Bay? And – what about the action of specific weather events to modify or even dictate the course of a melt season? You mention this mechanism in the first page (August 2012 storm) but never return to it (and while that storm may not have affected Arctic Ocean ice cover, what was its effect on ice advection and the CAA?). The action of a couple of well-times storms was specifically identified in the Atkinson et al 1998 paper as a mechanism which helped make 1998 a very anomalous year. These things must be explored if the manuscript is to serve in its desired capacity to provide physical bases for these anomalies.

Howell et al.
1. Mechanisms which explain surface air temperature (SAT) anomalies are not the focus of this manuscript. The objectives were focused on the role of SAT forcing on the sea ice of the CAA. The origin of the heat anomalies is important, they can be inferred from the SLP plots, which we feel are sufficient to provide clear evidence that the warm air was of southerly origin. Also, warm air from the south also restricts Arctic Ocean inflow into the CAA which was thoroughly explored (see our next response). We feel that including z500 plots, which show very similar patterns to SLP (see below) is unnecessary.

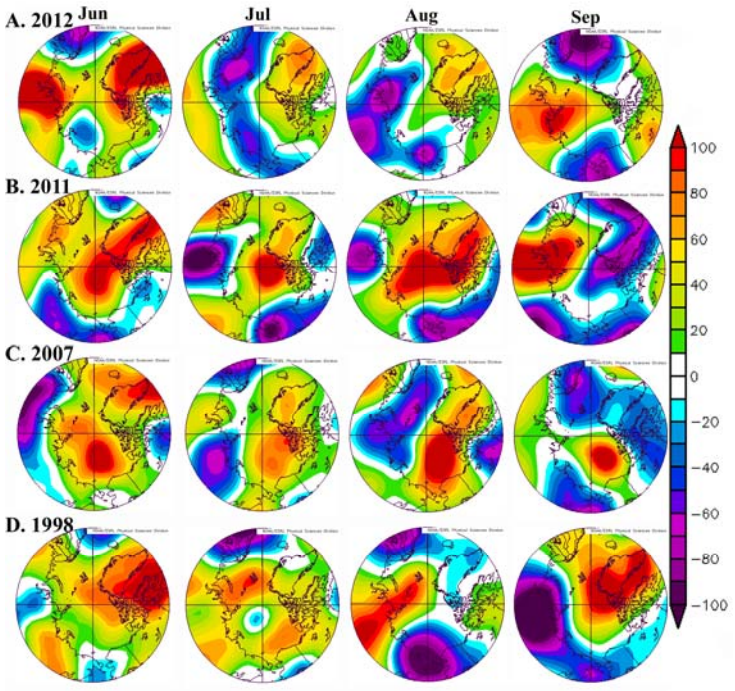

Figure. Spatial distribution of mean June to September 500 mb geopotential hight (m) anomalies for 2012, 2011, 2007 and 1998 (A-D). Anomalies calculated with respect to the 1981-2010 climatology.

2. The reviewer indicates that the MYI pattern between 1998 and the other years was very interesting and then mentions we never linked specific weather events to ice advection citing 2012 as an example. This is puzzling because we did extensively focus on the pattern of MYI as it related to atmospheric circulation because ice dynamics is more influential on minimum sea ice conditions in the CAA than thermodynamics – a key result of this manuscript. Specifically, we quantified sea ice exchange between the Arctic Ocean-CAA using RADARSAT and linked it to sea level pressure patterns that have been widely used in the literature to explain ice motion. The numbers in Figures 10 and 14 reflect the impact of individual events on ice advection, and it is the exchange of MYI between the CAA and Arctic Ocean that drives minimum sea ice conditions in the CAA more so than SAT. The items the reviewer indicates we did not explore were addressed by quantifying Arctic Ocean-CAA exchange – this was noted by another Reviewer (John Walsh) as a strength of this manuscript. Moreover, the tracked ice motion in this paper is more thorough than used in Atkinson et al. 2006-AtmosphereOcean and Howell et al. 2010-JGR. Finally, the reviewer missed that we did return to the effect of the 2012 storm on ice advection into the CAA in Section 5.1:

"With respect to sea ice dynamic forcing for 2012, the minimum was initially driven by rapid melt but this ice loss was mitigated by Arctic Ocean MYI inflow. ***The majority of this inflow occurred via the M'Clure Strait in August (Fig. 10b) with MYI during August also exhibiting increases (Fig. 5b). The ice inflow is likely associated with the anomalously low SLP that tracked across the Arctic Ocean and into the CAA (Simmonds and Rudeva, 2012).*** *Following the August ice inflow event, almost zero net ice area flux occurred in September at the M'Clure Strait but a small net inflow occurred into the QEI (Fig. 10).* ***These Arctic Ocean ice inflow***

events reduced the CAA's rate of total ice decline, preventing total ice conditions from eclipsing the 2011 record low. *This driving dynamic process observed in 2012 was similar to 2007.* ***Although June to September SLP anomalies were different between 2012 and 2007 (Fig. 11a and 11c) both atmospheric circulation patterns facilitated Arctic Ocean inflow into the CAA. It should be noted however that the 2012 Arctic Ocean ice influx into the M'Clure Strait was driven by 2 single SLP driving events in August (not shown) that are lost in the August monthly SLP average.*** *The key difference between 2012 and 2007 was that despite the net Arctic Ocean ice inflow at the M'Clure Strait in August and at QEI in September for 2012, MYI conditions did not remain high through the remainder of the 2012 melt season (i.e. the imported MYI subsequently melted; Fig. 5b). Stronger positive August and September SAT anomalies in 2012 relative to 2007 may have resulted in the ablation of more MYI (Fig. 8a and 8c)."*

3. Addressing specifically why landfast ice thickness at Resolute Bay is not decreasing is not within the scope of this manuscript. The decreases observed at the other sites in the CAA are consistent with the literature (e.g. Polyakov et al., 2012-BAMS).

Comments major and minor are noted as they appear in order in the document.
I will need to see the revisions.

Reviewer #3
Title – it sounds like a newspaper headline, and it isn't reflective of what you actually are doing. Heavy ice years are given almost as much play as light ice years. The title should be something like "Dynamic and thermodynamic forcing of anomalous ice years in the Can Arc Arch". Something like that would better reflect what was done in the manuscript.

Howell et al.,
We think the title is appropriate. The point is to highlight that 2011 and 2012 set new records in the CAA. It is inappropriate to focus only on dynamic and thermodynamic forcing in the title since the study finds that preconditioning played more of a role than both dynamic and thermodynamic forcing.

Reviewer #3
p1315, Line 7 – use "the" before water features other than lakes.

Reviewer #3
Howell et al.,
Changed

Reviewer #3
P1315, Line 26 – to clarify the shift in geographic focus start the sentence as:
"Within the CAA, the record lightest sea ice year was 2011 with a mean September..."

Howell et al.
Done.

Reviewer #3
P1316, line 5 – Replace Howell et al 2010 with the Atkinson et al 2006 reference here please; the southerly flow/atm circulation for the 1998 anomaly was described there first. The Alt et al 2006 reference may also be applicable; I can't recall at this point. Certainly Alt, along with Agnew, were the ones who described the atmospheric patterns that resulted in SAT anomalies that had nothing to do with what is going on at the surface.

Howell et al.
Changed accordingly:
"Atmospheric circulation during the summer months of 1998 allowed predominantly warm southerly air masses to flow over the CAA (Atkinson et al., 2006). The resultant anomalously warm SAT and restriction of Arctic Ocean MYI inflow into the CAA combined to gradually ablate the thicker MYI over a longer than normal melt season (Howell et al., 2010). In 2007, anomalously warm July SAT facilitated an intense and rapid melt which led to light ice conditions in the CAA, but this was partially mitigated by high SLP over the Beaufort Sea which facilitated the inflow of Arctic Ocean MYI into the CAA (Howell et al., 2010)."

Reviewer #3
P1315, line 19 – spell out numbers < 10 and just say "years". In this sentence years is not being used as a variable in a formula.

Howell et al.
Done. We assume the reviewer means P1316.

"Perhaps more striking is that five of the lightest ice years in the CAA since 1968 (i.e. 2007, 2008, 2010, 2011 and 2012) have all occurred within the last 6 years (Fig. 1c)."

Reviewer #3
P1318, lines 6-7 – elaborate on the technological bias or drop the comment because it seems to contradict your next comment concerning the Tivy et al results.

Howell et al.
Comment dropped.

Reviewer #3
P1318 line 11 – move the comma in front of "therefore"

Howell et al.
Done.

Reviewer #3
P1320, line 23 – "To investigated" – no ED here.

Howell et al.
Done.

Reviewer #3
P1320, line 23 – presumably annual maximum landfast thicknkess ice values but add that in for clarity. Okay I see later on – not annual values but weekly time series. Do clarify with the lead-off sentence.

Howell et al.
Done.

Reviewer #3
P1321, line 15 – say "...using a Kendall tau non-parametric function following..."
P1321, line 15 – State (justify) why in particular you have elected to use this slope estimator method.
P1321, line 15-17 – Did you test for autocorrelation? If so, state the results; if not, do so to properly justify application of pre-whitening.

Howell et al.
The assumptions of linear regression did not hold hence we used the nonparametric test for trends. Yes, we did test for autocorrelation and it had to be removed. We have revised this as follows:

"The nonparametric Mann–Kendall test for randomness against trend (Mann 1945; Kendall 1955) was used to calculate the trend in maximum landfast ice thickness from 1968-2012 at the selected sites within the CAA. The slope of each trend was calculated using a Kendall's tau following an approach by Sen (1968) because the data were not normally distributed. Prior to assessing trend statistical significance, each time series was pre-whitened to remove lag one autocorrelation which was present at each site using an approach described by Wang and Swail (2001)."

Reviewer #3
P1321, line 18 – Define what IceBridge data are; it isn't as common as IceSAT and readers might not be familiar with it.

Howell et al.
Done.

"Ice, Cloud, and land Elevation Satellite (ICESat) and IceBridge ice thickness estimates were obtained prior the melt season to investigate changes in the thickness of Arctic Ocean MYI that would flow into the CAA during the melt season. The ICESat ice thickness estimates are made using satellite based laser altimetry and are the same used in Kwok et al. (2009). A full escription of the retrieval methodology can be found in Kwok et al. (2007) and Kwok et al. (2008). IceBridge ice thickness estimates are also made by laser altimetry but from an airborne platform using the Airborne Topographic Mapper that fully is described in Krabill et al. (2002)."

Reviewer #3
P1321, line 21 – "provided by the NSIDC"
P1322, line 5 – "from the National Center..."

Howell et al.
Changed.

Reviewer #3
P1322, line 5 – Reanalysis – why would you use a 2.5 degree resolution product that has no hope of capturing SAT in a region as complex as the CAA? Redo SAT and SLP work using the NARR.

Howell et al.
Atkinson et al. 2006, Tivy et al., 2011 and Howell et al. 2010 all used NCEP reanalysis for looking at linkages between CAA sea ice and SAT. While there are uncertainties with NCEP in the CAA (i.e. cooler temperatures), we want to be consistent with previous studies. Just because NARR is higher resolution does not mean it is much better within the CAA and we found little difference when we also used it in Howell et al. 2010. For SLP, the spatial domain of NARR is too small.

Reviewer #3
Table 2: Add to the caption which row is heavy/which row is light. "Monthly June to September total, multi-year ice (MYI – top row) and first-year ice (FYI – bottom row)..."

Howell et al.
Done.

Reviewer #3
p1322, line8-9 - "A summary of standardized ice cover anomalies for the extreme light and heavy ice years is shown in Table 2." - Go ahead and state what they are at this point, e.g. a sentence like this would be better: "Standardized anomalies are provided in Table 2 for monthly total, FYI and MYI, for heavy and light years."
p1322, line 12- "These 2011 June anomalies were both weaker..." ? Presumably drop 2011?
p1322, lines 12-20 – this is all really clunky – Get rid of this stuff; Table 2 says all this. You want to just drill down to the analysis/summary comments, such as appear starting line 21.
p1322, line 20 – "Not surprisingly, the..." Don't say things like this unless you state why it is not surprising. State the observation, and then if it is of particular interest, note this and state why. Not all of your readers will be as close to the material.

Howell et al.
We have revised this paragraph as well as included some excellent points recommended by this reviewer as it pertains to specific Figure 2 comments.

"The monthly time series of June to September total ice and MYI area within the CAA is shown in Fig. 2. Total ice has been relatively stable in June and July because sea ice within the CAA remains landfast well into July. It terms of FYI and MYI for June and July, there has been a

decrease in MYI and a corresponding increase in FYI most noticeable since 1998. Over the entire period, both August and September FYI have experienced a slow downward trend, whereas MYI remained steady and only began to drop off since 1998. The September total ice series points out how the strong 1998 was as ice conditions where relatively stable until then. While subsequent minima was low (2007) or lower (2011 and 2012) they are within a context that now favors light years which was not the case for the 1998 minima.

Standardized anomalies are provided in Table 2 for monthly total, MYI and FYI, for light and heavy ice light years. The heavy ice years tended to experience mostly positive total ice standardized anomalies for June and July. Total ice anomalies for heavy ice years during August and September were all positive. In addition, total, MYI and FYI for 2011 and 2012 in September where lower than 1998 and 2007."

Reviewer #3:
P1323, line 25-26 – "Although FYI melts more easily, which in turn is more likely facilitate light ice conditions under anomalous atmospheric forcing, " - clarify this please.

Howell et al.
Added it melts more easily it is thinner than MYI

Reviewer #3
P1324, lines 21-23 – "In addition, the FYI time series illustrates that virtually zero FYI remained following the melt season for all four extreme light ice years." – too colloquial; restate more like "virtually all FYI melted during the light ice years"

Howell et al.
Changed.

Reviewer #3
P1327,lines 1,2 – "... 1998 was gradual as oppose to rapid that is likely attributed to the increased presence of thick MYI (Fig. 5a, b)." – it's "oppose**d** to" and the sentence needs to be broken at "that", so "...to rapid. This is likely...".

Howell et al.
Changed.

Reviewer #3
P1327, line5 – process, not processes.

Reviewer #3

P1327, lines 4-7 – "Perovich et al. (2007) demonstrated the melt processes is enhanced from an earlier melt onset by increasing the energy available for melt during the melt season that exerts an influence on the minimum area reached over the season." – I don't really know what this is saying. I suspect it's a grammar issue.

Howell et al.
Changed.

Reviewer #3
P1327, lines 7-9 – "This was the case in 1998 when the earlier melt onset (−6.6 days) resulted in more total ice loss early in the melt season and contributed to the gradual melt of MYI throughout a long melt season (Figs. 5a, b, 9)." – I don't see this I'm afraid. The rate of total melt from Fig 5 is the same in 1998 as for the other years. MYI decline in that summer is noteworthy because it doesn't gradually decline. It holds almost steady until well into August, then exhibits a rapid decline. Remove this sentence because you can't use 1998 as a case in point for gradual MYI decline – it just isn't there. I don't disagree that total melt starts Fig 5 at a value lower than other years, which I suppose could be early melt and not simply a result of a winter that was not favorable to ice formation, but it is clear that the heavy toll is being taken on FYI.

Howell et al.
When we were referring to gradual MYI loss, we mean gradual MYI loss beginning in August. MYI loss in 1998 is certainly gradual from August onward.

"This was the case in 1998 when the earlier melt onset (-6.6 days) resulted in more total ice loss early in the melt season and contributed to the gradual melt of MYI beginning in ***August*** *throughout the remainder of the melt season (Fig. 5a-b; Fig. 9)."*

Reviewer #3
P1327, lines 14-16 – "However, once melt onset began in 2011 and 2012, the sea ice-albedo feedback served to intensify the strong positive July SAT anomalies and facilitated an even more rapid melt than 2007." - How would sea ice melt intensify an SAT anomaly? What would the screen-height air temperature be above an ice surface in the CAA in July, and what would it be over the open water? There would be very little difference. Is there a reference for someone measuring this? And – your SAT values/anomalies are derived from NCEP/NCAR RI – there is no way this reanalysis has the spatial resolution or process sensitivity (i.e. Where/when has a given ice-covered bay melted) to ever draw this conclusion for a region as complex as the CAA. This must be far more strongly supported with physical analyses or dropped. I would imagine it was likely an extended period of minimal cloud cover and/or strong southerly advective flow, eg like 1998. That is the scale of physical process RI will pick up.

Howell et al.
This was just a simple transposition error in our sentence. What we meant to state was:

"However, once melt onset began in 2011 and 2012, the sea ice-albedo feedback intensified because of the strong positive July SAT anomalies and facilitated an even more rapid melt than 2007"

Reviewer #3:
P1327, Line 16 – "also not anomalous**ly** late "

Howell et al.
Changed.

Reviewer #3
p1327, line 16-17 – "Freeze onset dates for 2011 and 2012 were also not anomalous late providing more support for rapid melt driving ice loss in 2011 and 2012 (Fig. 9)." - What does this mean? At least explain how one follows from the other.

Howell et al.
Freeze onset dates for 2011 and 2012 were also not anomalously late providing more support for rapid melt driving ice loss in 2011 and 2012 as opposed to a gradual loss from a long melt season (Fig. 9).

Reviewer #3:
P1329, line 9-10 – "The three processes that are associated with heavy ice years are short melt seasons, low mean JJAS SAT anomalies and Arctic Ocean MYI import." write "...and import of MYI from the Arctic Ocean."

Howell et al.
Changed.

Reviewer #3
P1332, lines 5-7 – "We suggest that longer melt seasons within the CAA (Fig. 9; Howell et al., 2009) are resulting in the increased absorption of solar radiation and subsequently delaying fall freeze onset that in turn has reduced the length of time for FYI within the CAA to grow." - This process is being presented as though it were a new idea. It isn't; provide appropriate citations. ...and check your grammar. Were that sentence to stay as is, it would need to be written as: "We suggest that longer melt seasons within the CAA (Fig. 9; Howell et al., 2009) allow for increased absorption of solar radiation [into the upper part of the water column], warming the water column and so delaying fall freeze onset, which in turn has reduced the length of time for FYI to grow."

Howell et al.
Changed to:
"Evidence of thinner FYI is apparent from the decreasing trends in maximum landfast ice thickness within the CAA. Every location except Resolute Bay is experiencing statistically significant declines in maximum landfast ice thickness (Fig. 16; Table 2). The decreases are equivalent to ~27 cm of ice thickness loss since 1968. The likely processes responsible as previously suggest by both Dumas et al. (2006) and Polyakov et al. (2012) was longer melt seasons that allow for increased absorption of solar radiation (into the upper part of the water column), warming the water column and so delaying fall freeze onset, which in turn has reduced the length of time for FYI to grow."

Reviewer #3
p1333, lines 10-12 – “There is a strong inverse correlation between the length of the navigation and mean MYI area present over shipping season of r = −0.72.” – more details here. What/how many years were used? It would be useful to see this plotted.

Howell et al.
This is plotted in Fig 16 and represents the entire time series. We added a reference to Fig 16.

Reviewer #3
P1334 – you really can't answer the question, “what is the likelihood”, where likelihood in this context must be taken at its statistical meaning. You have to change the wording on this objective.

Howell et al.
Changed to “could the CAA experience…”

Reviewer #3
Figure 1: The placemap with labels needs to be a fair bit larger. Reverse your color scheme for the ice concentration plot – typical is darker for heavier values; it will also allow the islands to be more readily distinguished.

Howell et al.
Figure 1 is now bigger because of the removal of a panel as suggest by another Reviewer. The ice concentration legend is the same as used by the NSIDC and we wish to keep it as it is.

Reviewer 3
Figure 2: Place circles on your selected min/max years, or add fine vertical lines, on each of the four monthly plots for clarity. Modify the caption accordingly.

Howell et al.
We have added vertical lines.

Reviewer 3
Figure 3: I know it's noted in the text but please also note in the captions which year set you are dealing with (heavy or light). This makes the figures self contained if someone is just skimming the paper.

Howell et al.
Done.

Reviewer 3
Figure 8: These plots should correspond to Figs 2 and 3. Remove July and August plots and instead make it June/sept Light and June/Sept Heavy.

Howell et al.,
We need to keep July because that is the month that corresponds to rapid melt that initially drove the minima of 2007, 2011 and 2012. Fig 2 and 3 are there to show initial and final sea ice conditions.

Reviewer #3
Looking at Figure 2 there are two interesting things I see:
1) It looks like FYI in both August and September have been on a slow but persistent downward trend for a long time, whereas MYI held steady and only after 1998 did it really start to drop off.

2) The Sept plot really highlights just how strong a negative anomaly 1998 was, and the extent of the departure it represented. Trends had been flat until then. The subsequent minima, while low or lower, are working within a context that now favors lower ice years; for 1998 it is clear that wasn't the case. One could even argue for an upward trend 1980-1990 in Sept total anomaly.

Howell et al.
Thanks for these insightful comments. We have added them to Section 4.

Reviewer 3
Polar plot figures in general – why are you showing SAT anomalies for the entire circum-Arctic? Your focus is the CAA; the maps should stay focused. It makes it very difficult to examine details.

Howell et al.
The polar plots for SAT and SLP are important to show for the pan-Arctic. This was done in previous studies (i.e. Atkinson et al. 2006) and we feel this domain in appropriate in order to compare conditions over the CAA with other sectors of the Arctic.

Reviewer #3
Most of your citations are your own work. Statements are made that need to be backed up by the literature.

Howell et al.
We have modified the citation for Atkinson et al. 2006 and the thermal inertia processes as suggested. We feel most of the other citation are appropriate. The majority of the recent work in the CAA has been conducted by Howell and others and the citations to previous work are appropriate.