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# *Interactive comment on* "Long-term variation of sea ice and its response to thermodynamic factors in the Northwest Passage of the Canadian Arctic Archipelago" *by* Xinyi Shen et al.

**Anonymous Referee #2**

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Review of "Long-term variation of sea ice and its response to thermodynamic factors in the Northwest Passage of the Canadian Arctic"

By: Shen et al.,

The Cryosphere – TC-2020-215

This paper examines changes in sea ice concentration and thickness in the southern portion of the Canadian Arctic and attempts to ascribe these changes to thermodynamic forcing from either either surface air temperatures or sea surface temperatures. A mix of observed sea ice concentration and modelled ice thickness is used, while air temperature and sea surface temperature were retrieved from an ERA- reanalysis



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product. All of this work is done in the context of declining sea ice and the opening of the Northwest Passage for marine shipping. While I think there is some interesting work done on sea ice concentration and thickness trends in the CAA, the manuscript suffers from oversimplification of key details on both shipping and declining sea ice because I think it is trying to cover too much and looks at too coarse of a time period. The paper would benefit from a detailed discussion of shipping through the NWP and how declining sea ice has led to increased shipping through the NWP over the last decade. In particular this would highlight the fact that shipping (non ice-strengthened, or ice-strengthened, non-ice-breakers) can only occur under very specific ice-free conditions during a short window in late-summer. The presentation of shipping pathways based on the probability of sea ice being thinner or less concentrated than the historical mean isn't practical. Even if the ice cover is thinner than it was historically, it is still very likely too thick for shipping to occur along the NWP. In terms of sea ice, again there are some interesting results, but the section correlating sea ice conditions to thermodynamic factors is very confusing and doesn't reveal a clear outcome. The authors present a lot of information and have included some nice analysis, but I think the objectives of the paper need to be clarified before the paper is revised.

Considering the major revisions I encourage the authors to make I would suggest they focus on the major comments first. I have attached minor comments as well, but considering the paper will likely be heavily revised I suggest they leave these minor comments until later.

Major Comments: 1. Shipping along the NWP is given as a motivator for this work, but there is very little actual discussion of shipping along the NWP. I would suggest that the authors provide a detailed introduction to shipping along the NWP that discusses its benefits (shorter route), its limitations (sea ice), the recent increase in ships along the NWP, its seasonality (which is key), and the projected potential for shipping along the NWP in a warming Arctic. In particular I would recommend the authors look at the following list of works and really strengthen the motivation for this work. âĂć Piz-





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zolato et al., (2014), Changing sea ice conditions and marine transportation activity in Canadian Arctic waters from 1990 and 2012, Climatic Change, 123, 161-173, doi: 10/1007/s10584-013-1038-3. âĂć Pizzolato et al., (2016), The influence of declining sea ice on shipping activity in the Canadian Arctic, Geophysical Research Letters, 43, doi: 10.1002/2016GL071489. âĂć Melia, Haines and Hawkins (2016), Sea ice decline and 21st century trans-Arctic shipping routes, GRL, 43, doi: 10.1002/2016GL069315 âĂć Ng, Andrewsm Babb, Lin, Becker (2018), Implications of climate change for shipping: Opening the Arctic Seas, WIRES. âĂć Dawson et a., (2018), Temporal and Spatial patterns of ship traffic in the Canadian Arctic from 1990 to 2015, Arctic, 71(1), doi: 10.14430/arctic4696

Based on this revised discussion I think the discussion of shipping pathways needs to be heavily revised or removed. Basing a pathway on the probability of sea ice being lighter than the historical mean isn't realistic, because an ice cover that is thinner than the mean may still be too thick for a majority of vessels to travel through. Instead, I would suggest using polar codes or literature on arctic shipping to define thresholds and then examine when ice conditions that meet those thresholds exist. This would be much more practical, but is likely moving away from your thermodynamic analysis and more into the realm of shipping focused papers.

2. For all of the correlation and trend analysis, I would suggest only presenting significant ($p < 0.05$) values. This would highlight real changes and remove some questionable results like small trends towards increasing sea ice concentration during winter and spring.

3. With respect to the Cryosat2 ice thickness data. Since it is only used to quickly assess the accuracy of the modelled ice thickness then I would suggest moving this discussion and Figure 5 to your methods sections. I would also suggest only using the CS2SMOS product as it is much more accurate over areas of thin ice (which you note with its accuracy during fall) and as opposed to presenting the comparison in a time series, present it as a scatterplot of monthly means. Further to this comment and this

section a. L 81-82: What areas of the NWP did the CS2SMOS product not cover? b. L 82 - 83: It's worth noting here why there is no ice thickness data from Cryosat-2 during the melt season. c. L83-84: Beyond saying the modeled ice thickness was "reasonably validated" please provide an exact measure of correlation or bias here. Also consider rewording as you don't use the modelled ice thickness to "fill the temporal and spatial gaps" but you instead use it throughout your full analysis.

4. The correlation section is very difficult to understand and doesn't provide a clear result. I also think it needs to be reinforced that this is a thermodynamic analysis and as Howell has shown in several papers, dynamics, particularly the transport of multiyear ice within the CAA and along the NWP is an important process.

Minor Comments:

L 12: This comment applies throughout the paper, but when referring to your study region it is the Canadian Arctic Archipelago and the NWP runs through it. I would suggest revising this sentence to read ". . . we studied the temporal and spatial characteristics of sea ice from 1979 to 2017 in the CAA and evaluated the sea ice conditions along the southern and northern routes of the NWP".

L 14-15: The term "heavy" ice conditions isn't really clearly defined, so I would suggest revising to "the region remained ice covered throughout winter and spring during this period". Additionally based on my suggestion to present only significant trends, I think the text about there being a slight increasing trend can be removed.

L 17: I don't see evidence from Figure 3 of increasing SIC in Lancaster Sound. Please check this statement

L 18: replace "Based on the sea ice concentration and thickness, however the sea ice conditions . . ." with "Generally, sea ice conditions were heavier along the northern route than the southern route, with a longer ice season and thicker ice".

L 20-24: I have other comments about the correlation analysis that will likely cause this





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text to be revised. But when your revising this please be specific in your statements. An example is "...Thermodynamic factors had a greater impact on sea ice in the summer and fall, than during winter and spring".

L 24-26: I don't think "residual" is the correct word for this. You're talking about the remaining ice that persisted through summer and already exists at the start of fall freeze-up. Also this remaining ice is not only influenced by fall SST and SAT, but also summer SST and SAT. I think this statement needs to be revised.

L 30-34: Back to one of my major comments, but this introductory text can be strengthened. The NWP connects Europe and Asia > it is shorter than the Panama Canal Route, but historically it has been ice covered and unsafe for marine vessels. However, as ice declines the NWP is becoming increasingly accessible... I would then reference the works of Pizzolato and Dawson about increase shipping activity along the NWP.

L 33: "The opening of the NWP will bring huge economic benefits", please provide a reference for this and specify who will benefit? Also what about the additional risk for communities and the environment around the NWP?

L 34 – 35: Remove "the" from in front of M'Clure Strait and Barrow Strait.

L 36: Note right away that there are 3 southern routes that all rely on Lancaster Sound and Amundsen Gulf but pass through different channels in the central part of the CAA. Essentially, this description of the routes can be tightened up.

L 48-55: This text on sea ice in the CAA is good, but to the point of warming increasing ice severity along the NWP, I think it should be noted that MYI enters the northern CAA from the Arctic Ocean and migrates through to the southern CAA during summers as the ice cover opens up. Additionally, Haas and Howell (2015) observed modal thicknesses of 1.8 and 2.0 m along the NWP with deformed MYI having a mode of 3.0 m. This would be good to include so you can refer back to it later when presenting your modeled ice thicknesses. Also it would be worth noting the previous minima in 1998





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and 2007 described by Howell et al., (2010) âĂć Haas and Howell (2015), Ice thickness in the Northwest Passage, GRL, 42, doi: 10.1002/2015GL065704. âĂć Howell, Tivy, Agnew, Markus, Derksen (2010), Extreme low sea ice years in the CAA: 1998 versus 2007, JGR, 115, doi: 10.1029/2010JC006155.

When discussing sea ice within the CAA it needs to be clear that the ice is mobile and there is a mix of first year and multiyear sea ice in the CAA. Specifically the Drain trap mechanism for MYI in the central part of the CAA described by Howell et al., 2008 should be described. As well as the fact that ice is imported and exported from the three gateway regions (Amundsen, M'Clure and Lancaster), particulary during spring and summer.

L 59-60: This connects back to a previous comment, but when expanding the discussion of shipping through the NWP note the difference between the open water shipping season and potential for ice-breakers. The difference in vessels is critical for shipping along the NWP.

L 63: revise "we utilize a combination of remotely sensed sea ice concentration data and modelled ice thickness data to examine the sea ice conditions . . .."

L 68-69: There's a comment below about this selection of a ship path, but I think this needs to be explained in more detail and presented as the "optimal" or "route through the lightest ice conditions".

Methods:

L 74-76: Please elaborate on the description of this dataset. Howe is it collected and what are its limitations. In particular passive microwave data is known to underestimate sea ice concentration during the melt period. This error should be consistent through the time series so it wont dramatically affect your results, but it should be discussed.

Regarding the interpretation of sea ice concentration data, you commonly refer to extent, but I believe you are calculating sea ice area. This is good, but the figure labels

and text need to be revised. Also please add a sentence about this in the methods.

Regarding sea ice concentration data in Prince of Wales Strait – this is a fairly narrow channel and with 25 km resolution I'm wondering how many actual sea ice pixels are contained in this channel and how reliable that data is. Also I haven't seen this channel discussed as part of the NWP before, typically there is one of the two southern routes or the northern route through M'Clure.

L 88: resolution is up to 1 km , but what is the range?

L 95: It is just ICESat, not ICESat-2.

L 94 – 100: In this text please note that all of this work was presented by Zhang et al., (2016). Adjust the start of this text from "We conducted. . ." to "Zhang et al., (2016) validated ice thickness from AO-FVCOM with a multisource dataset. . .".

L 102-104: Which ERA reanalysis did you use? -Interim or -5? Please specify.

Results: L 108 and throughout: revise the text "significant spatial distribution differences" to " significant differences in the spatial distribution" or "significant spatial differences". As it's written it is tricky to read.

L116 – 120: I'm not sure these means are really worth presenting given the significant negative trends you are about to present in the next section. I think the 1979-2017 mean shows the general pattern of ice loss, but I wouldn't get too focused on the actual values.

L 120: "After September, the sea ice started to freeze", this is pretty informal, I'd suggest adding some more detail here.

L 124-125: Again check that these trends are significant. The Prince of Wales Strait is not significant, so this text needs to be revised. Also just a note that the figures show sea ice extent (area) and the trends are presented. Perhaps provide both the trend in extent and then provide the % for further context.





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L 126-127: back to one of the major comments, but I am really suspicious of this increasing trend during winter. There may be some variability, but that is likely due to the error of passive microwave sea ice concentration retrievals and not real.

L 143 – 147: With respect to Figure 4, the key takeaway is the negative trends in each sub-region. I don't think discussing the mean annual sea ice concentration in each region over the 38 year record is that useful, especially given the substantial changes taking place.

L 153: Replace "The larger sea ice extent" with "A near complete ice cover. . ."

L 154: The change that occurred around 1997 is related to the 1998 minimum discussed by Howell et al., 2010. Please add the reference of 1998 to the introduction and then you can refer back to it here. Howell, Tivy, Agnew, Markus, Derksen, 2020, Extreme low sea ice years in the Canadian Arctic Archipelago: 1998 versus 2007, GRL.

L 163: "Larger sea ice extent resumed in October. . ." this is a part of fall freeze-up. Beyond listing the regions, I think it would be more useful to state that freeze-up begins earlier in the central and northern part of the CAA (M'Clintock, Peel, Prince of Wales, M'Clure, Viscount, Barrow) in October and then expands to the southern and peripheral part of the CAA during November.

Section 3.3.1: See one of the major comments above about removing this section and moving the brief validation of the model to the methods.

L 178: Remove the 2 from ICESat-2.

L 182: revise " significant spatial distribution differences"

L 183 – 184: As opposed to saying "the sea ice thickness was larger in spring and small in late summer and early fall", simply say " the sea ice was thicker in spring and thinner in later-summer and early fall".





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L 186 – 187: Thicker ice is located in the Queen Maud Gulf, M'Clintock Channel, and Peel Sound because it operates as a drain trap for Multiyear ice within the CAA as described by Howell et al., 2008. Please add discussion of this to your introduction so you can refer back to it here. Howell, Tivy, Yackel, McCourt (2008), Multi-year sea-ice conditions in the western Canadian Arctic Archipelago Region of the northwest passage: 1968-2006, Arctic.

L 203 and Figure 8: Again please only display and discuss the significant trends.

L 205 – 206: With respect to the increasing trend in the Labrador Strait, first is this significant? If so what is the mechanism for this?

L 223: "deepest" I think this is in reference back to Figure 1, but the bathymetry of the CAA isn't actually shown in that figure. Please revise or add a reference to this point.

L 226-228: The process of selecting pathways based on "light sea ice conditions" needs to be revised and considerably improved. Thinner ice doesn't necessarily make a route passable of the ice is still relatively thick and therefore hazardous for all but a few ships. This goes back to the first major comment, a better introduction of shipping along the NWP and the focus on the open water shipping season is needed before this discussion is suitable. Also, if basing the route on the change relative to the historical mean, its important to note the historical mean represents a concentrated, thick ice cover that didn't break up during summer.

Section 4.2.2: I find this section to be very confusing and difficult to understand. I would encourage you to clarify this discussion. In particular I would suggest you only focus on significant correlations. Additionally, it's important to remember that this only reflects thermodynamic forcing and not dynamics, which Howell has shown to be key for the CAA in several of his works.

L263-264: "the low temperatures did not affect the sea ice melting", I'm not sure what is meant by this statement.





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L 264-265: How are the SST's observed during winter? Are they simply set to be at the freezing point when under sea ice? This should be discussed in the methods and will likely cause this text to be revised.

L 311: "Suggest" instead of "suggested"

L 311: I'm not sure "residuals" is the right word for what you're describing here. This is the state of the ice cover after the September minimum and at the start of fall-freeze-up.

L 321-323: Connecting the state of the ice cover in fall to the state of the ice cover at the end of winter is interesting, but the last sentence about fall SAT and SST affecting sea ice thickness the following winter is a little misleading, because the state of the ice cover in fall is predominantly dictated by the summer conditions and not just fall.

Conclusions:

Based on the comments above it seems that the conclusions may be revised considerably, but here are some minor comments to handle now.

L 325: remove the "ed" from "exerted".

L 329: Based on a previous comment, you don't really use observed sea ice thickness data. It's all from the model.

L 336 and in other places within the paper: Please be consistent and present sea ice concentration as a percentage (%) as opposed to just "1".

L 339: Another instance the "increasing trend" during winter. Please check that this is significant.

L 343: revise the first sentence to read " from 1979-2017, sea ice thickness in the NWP decreased. . ."

L 344: Revise " The multiyear mean seasonal sea ice thickness" to read " The monthly mean sea ice thickness. . ."

L 347-348: Remove “the” from “In the most. . .” and “Lancaster Sound, the sea ice. . .”.

L 348: In the conclusions provide a value for these trends.

L 361: Revise the word “dominant”, the end of winter ice cover is influenced by the fall ice cover, but I don’t think it is the dominant factor.

Figures:

Figure 1: Note that you don’t actually show bathymetry in the CAA so I would suggest removing bathymetry from this figure.

Figure 3: Is the top panel the trends in the annual mean sea ice concentration? I would suggest removing that and focusing on the seasonal means.

Figure 4: For the inset of annual cycles can you provide some bounds of the standard deviation or

Figure 5: This figure can very likely be removed based on comments above.

Figure 6: I’d suggest removing “distribution” from the caption as an ice thickness distribution is something other than this figure.

Figure 7: Note in the caption that this is the “annual mean thickness”.

Figure 8: Again, only show the significant trends and consider removing the annual mean.

Figure 10: Are these the annual mean changes in SAT and SST? Also, instead of the % please consider revising to show the magnitude (°C) of the trends and only showing the significant trends.

Figure 11 and 12: It is slightly counter intuitive to flip the colorbar so that blue is positive correlations and red is negative. Also, once again consider showing only the significant correlations.

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