Review of: Anatomy of Arctic and Antarctic sea ice lows in an ocean-sea ice model

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Manuscript Synopsis

This manuscript, using an atmosphere forced ocean-ice historical reconstruction attempts to analyze sea ice flux mass contributions for exceptional sea ice events relative to climatology. The paper uses the common methodology to compare and contrast sea ice events in the Arctic and Antarctic, as well as comparing/contrasting melt and freeze-up season events. The paper could provide a useful measure examining exceptional sea ice events, but suffers considerably by confusing and incomplete graphics as outlined in my major comments below. In particular, the pre-existing sea ice anomaly plays a huge role in allotment of mass balance fluxes into anomalous fluxes, particularly for melt events, where there is a definitive upper bound on sea ice removal (you cannot remove more ice than what already exists). The authors discuss this *"pre-conditioning"* (their term) in the text, but the lack of a graphical representation of this term in their budget can lead to confusing interpretation of the results, particularly by a reader seeking quick visual summarization of the results.

My recommendation is Major Revisions

Major Comments

1. Fig. 4 & 5. I found the presentation of these figures very hard to follow, as the discussion relies heavily on an additional term (preconditioning or initial mass anomaly) that requires careful reading of the manuscript to draw out. As the figures stand now, it is very easy to convince oneself that positive flux anomalies mean an decrease in sea ice mass, when in actual fact they mean the the opposite (increase in sea ice mass), but in a vast proportion of the sea ice thickness anomalies, this incorrect assumption does seem to visually confirm – and in some cases (Labrador Sea / Baffin Bay) seems to be wholly nonsensical. To make graphical interpretation much simpler: .

    - The sea ice thickness anomaly **must** be over the **same** period (May to August) as the flux anomalies (and not just August).
    - This still does not lead to a visual flux closure as it does not account for the *initial* sea ice mass anomaly (pre-conditioning in the author's terminology), which in many, if not all cases is the main offsetting factor. Therefore an additional "preconditioning" pseudo-flux should be added to the bar chart representing the initial mass anomaly (technically it should be for 30 April, but an average over April should be close enough if more convenient/smoother). I initially though you would need to convert this into a flux – but if I understand correctly the fluxes are already time integrated into mass gain over the 4 months? [You would likely not wish to add this pseudo-flux to the total, just leave it separate.]
    - Only then will it the figure visually balance the fluxes in the sector with the mass loss/gain contours.

- It will also visually confirm large segments of the text which discuss that the apparent flux anomaly is actually due to "pre-conditioning" (i.e. the initial mass anomaly), with the *increase* in anomalous ice mass fluxes (i.e. mass growth) being *largely* offset by the initial anomaly. In other words, there is an increase in anomalous ice growth largely due to there being less ice than climatology to melt!
- Ultimately, the usage of anomalous fluxes seems to be less than informative, the size of the flux ultimately being hugely dependent on the underlying sea ice volume. A better strategy (with no guarantee of success) might be to use normalized (either by total ice volume, or ice volume change, the latter assuming a definitive melt/freeze sign by sector) fractional flux anomalies. For instance does the fraction of basal sea ice melt increase or decrease from climatology in the exceptional years? Note: The fractional flux could be greater than 1, or less than zero. Sign conventions, for lack of better terminology, might be messy. I do not suggest pivoting to such an analysis now, I would view this manuscript as a learning process in best practices in this regard.
- Examples of confusing aspects:
  (a) Erroreous statement of Major Comment #3.
  (b) Statement concluding Subsection 4.1 (Minor Comment #1)
  (c) Using text explanations to highlight effect of pre-existing mass anomaly *(preconditioning)* without additional graphical assistance (ll. 311, 314, 338, 360, 363, 383, 394, 489–505).
  (d) Large sea ice growth flux anomaly in Baffin and Hudson Bays/Labrador Sea sector with only a small manifestation of sea ice loss in the Canadian Archipelago.

2. Ocean Heat Content: I am not entirely convinced of all the claims made in the manuscript with regards to increased heat content leading to increased basal melt.
   (a) The stated alignment of the increased heat content (Figure 6; red; numerically positive) and decreased sea ice volume (Figure 5b; red, numerically negative) do not line up as well as suggested as demonstrated in the enclosed animated gif which purposes to overlay the two (I see a lot of alternating red/blue). Caveat: As with my comments with regards to Figures 4 & 5, the heat content (Figure 6; April to September) does not align in time perfectly with the sea ice volume (Figure 5b; September) either.
   (b) The choice of the 100-200m heat content is a little confusing, and not justified. The winter time mixed layer depths [Uotila et al., 2019] range from 100 to 300m, which means an increase or decrease in the mixed layer could have opposing tendencies in the top 100m and 100-200m – if the mixed layer increases one might expect the upper layer to warm while the lower layer cools (increased surface mixing with the warmer below mixed layer waters), with the opposite cooler surface, warmer 100-200m if the mixed layer decreases (isolates the surface).
   (c) The previous point is very well illustrated in Figure 2 of the reference Zhang et al. [2022]. Indeed, the zero lag in that paper would seem to require an accompanying negative anomaly in 100-200m heat content. The mechanism also requires a long period lagged relationship that I see no evidence of here.
   (d) I might speculate that the heat contents are sea ice driven: Lower sea ice creation implies lower brine rejection and increased stratification (isolation) of the surface waters, increasing the 100-200m heat content. This would explain the relative uptick in Antarctic September heat content relative to April heat content seen at the end of the pan-Antarctic time series in Figure 6 – but it is also difficult to see if this is an isolated event, or a common occurrence.
   (e) As the authors state, the increased/decreased basal melt/growth may be driven by the atmospheric forcing, especially in low sea ice thickness states as the downward heat fluxes directly heat the ocean surface layers.
   (f) I do not advocate that my speculations, or any of the alternative explanations are more or less likely than the mechanisms suggested by the authors. I do suggest there is a lack of current evidence in the manuscript for any conclusions connecting the heat content to the loss of sea ice volume. (Seasonal) Lead/lag relationships may be critical.

3. Erroneous statement: l. 328. The manuscript states there is an increase in basal melt in the Chukchi Sea in July 2007. Supplementary figure S2m-o shows a blue (positive) basal melt anomaly in the Chukchi Sea. But positive flux anomalies are defined as anomalous gain of ice mass. Therefore this is not an increase in basal melt, but a decrease in basal melt. If I am incorrect, please correct me, but this does demonstrate my confusion generated by the figures. I suspect this positive basal melt anomaly is completely due to "*preconditioning,*" – i.e. there is a anomalous lack of sea ice to melt.
    - Similarly, the Chukchi and Bering Seas sector shows a net positive basal melt flux (so again decreased basal melt) in the bar charts of Figure 4a.
    - ll. 329. If I am not confused, and the basal sea ice melt is actually decreased, the connection to ocean heat transport may no longer be appropriate, however, you should have stated (the perhaps obvious, nevertheless still useful) that there are observations of increased **northward** heat transport. I briefly contemplated the authors meant there was an observed *southward* transport of heat to match the flux anomaly.
    - The statement "this increase (in sea ice mass) is not sufficient to compensate the export of ice" is correct – but again only added to my level of confusion.
    - Please check your characterization of your flux sign convention elsewhere in the manuscript. Having noticed this, I cannot convince myself there may be other instances where I have matched my interpretation of the sign convention in the graphics to match the text commentary (i.e. I can be easily confused into agreement).

Minor Comments

1. ll 362-363: "It (2012) is therefore a low not only in sea ice extent, but also in volume, in contrast with summer 2007." This comment **cannot** be made here without specifying you are excluding the seasonally ice covered Labrador Sea / Baffin Bay and Greenland Sea and Barents-Kara Seas sectors as previously mentioned in the text. Readers just reading the section concluding remarks (it does happen) will immediately refer to figure 4 and both conclude you have this backward – 2012 has no change in volume, and 2007 has a low in sea ice volume (i.e. invert your sign convention).
    - But if you also include the pre-conditioning flux this will also be rendered visually correct.
2. ll. 81-82. There are considerably more examples and research concerning climatic implications of changes to sea ice [e.g. Screen, 2013] – I would normally provide a more extensive list, but I am stressed for time here (no conflicts in solitary suggestion).
3. ll. 110-111. The tri-polar grid is designed to remain eddy-permitting in the Arctic (grid cells of order ∼12km). I should probably know this, but even so, others readers might not. Does the eORCA025 grid remain eddy-permitting throughout the Antarctic domain?
4. l. 117. Is it standard to have equal numbers of sea ice and snow layers (2+2)? I obviously do not know, but I seem to recall the multi-layer thermodynamics sea ice models I have dealt with have more sea ice layers than snow layers. Is there a rationale for this?
5. l. 151: *entire time series*. I assume this is your entire *analysis* time period (1979-2023), but perhaps it is worth repeating here? And the last two decades are presumably 2004-2023?
6. Figure 1b/d: From the looks of the plot, I assume the numerical minimum for both the Arctic and the Antarctic has a time value assigned by *exact* time in year (year + month + day), or in other words, the Arctic sea ice minimum for 2009 is more closely aligned (2009.75, or slightly to the left) of the 2010 grid line than the 2010 (2010.75) minimum. The same applies to the Antarctic, but in this case it is more closely aligned with the correct calendar year. If so (or if not) this should be communicated in the caption. The same question can apply to Figure 6 – with not as much consequence – are the annual mean (+0.5) aligned in time with April (+0.25) and September (+0.75), or are they offset by 0.25? Similarly figures S1 and S4.

7. l. 171. Units are slightly confusing, suggest reordering somewhat (in kg for sector analysis, and kg m^{-2} for individual grid points). However, it is also important this information be added to the figure captions – at least in the first instance of occurrence in a figure. (Figure 3, 4, 5 for sector flux values; Already included for grid values (S2, S3, S5, S6).

8. l. 192. Keen et al. [2021] is an extension of Keen and Blockley [2018] to a multi-model analysis. I would think that (no conflicting interest) the original budget analysis would be a more appropriate citation. Keen et al. [2021] would remain applicable for placing this manuscript's results within the CMIP context (ll. 224, 226, 289, 290). Note the DOI is correct (`https://doi.org/10.5194/tc-15-951-2021`, but the link does not work properly (across two lines, with the automatic line numbering interfering?) for Keen et al. [2021].

9. The sector Labrador Sea and Baffin Bay also includes Hudson Bay, which is likely an equal contributor to the sea ice mass changes over the May-August period. While Labrador Sea / Baffin and Hudson Bays is likely too lengthy for labelling purposes, I favour the more accurate East Canada Arctic moniker (Eastern Canadian Arctic is more grammatically correct, but longer).

Grammar and Typographical Errors

1. ll. 71-75. Mixing explicit time specific explanations (anticyclonic flow in 2007, summer storm in 2012, ...) with more generic explanations (anomalous ocean heat inflow, subsurface conditions, ...). The paragraph would likely read better if you separated this lengthy sentence into time specific explanations and generic explanations.

2. l 280. an hemispheric → a hemispheric (as correct on l.174) (ChatGPT: *Correct: an hemispheric level*)

3. l 469. but it is not at record high neither → but it is not at a record high either

   - Personally, I would refrain from using "record high." This implies more information than is known. I would recommend "but it is not at the highest point in the modelled timeseries either. Only Bellingshausen Sea sector heat content is close to its peak value.
   - It is also very difficult to precisely locate 2021 on the displayed timeseries. I assume this peak is confirmed at 2021 in the numerical data?

4. l. 517. a few percents of difference → a few percent of difference

5. l. 539. radiations → radiation (non-countable quantity; ChatGPT: *Correct: There are many types of radiations.*)

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