Toward a marginal Arctic Sea ice cover: changes to freezing, melting and dynamics

Since its last iteration, significant changes have been made to improve the clarity of the manuscript, the quality of the figure, and the presentation of the result. For that, I congratulate the authors for the effort that they have placed into assessing the reviewer's comments.

However, the clarification of the results has raised concerns regarding the scientific content that were not apparent in my initial review. Those issues must be addressed by the authors by doing additional analysis and it should be either acknowledged or clarified within the manuscript.

Additionally, I believe that the manuscript still needs improvements regarding the writing and the structure. While the changes proposed in the review are probably not all necessary, I believe they would enhance the effectiveness of the message and the quality of the scientific communication.

I maintain my opinion that the reviewed paper deals with the scientifically important research question of the widening of the marginal ice zone and the transition towards the ice-free Arctic. However, I believe that major revisions are still required to reinforce the analysis and improve the text.

Scientific concerns

1) I was expecting larger differences between the volume fluxes in the "always pack-ice", "become MIZ" and "always MIZ" regions, especially for the partitioning between top, basal and lateral melt (Figs. 8-10). It is mentioned in the text that "the partitioning of the melt between top, basal and lateral melting does differ substantially between the pack ice and MIZ", but I must admit that I was not convinced about that by looking at the figures. As far as I am concerned, the changes in the volume flux (Fig. 8) and fraction of volume flux (Fig. 9) between the "always pack" and the "always MIZ" region (e.g. Fig. 8a and Fig. 8c) are almost of the same order of magnitude as the changes in volume flux simulated due to variations in the atmospheric forcing (e.g. Fig. 8c and Fig. 8f).

   I suspect that it might be related to the use of fixed regions for the analysis that are only based on the July SIC (Fig. 6). I believe that larger, and more significant changes could be shown by computing volume flux in regions that evolve monthly. While I don't think that doing such an analysis would affect the main message of the study, I believe that it would show more contrasting and interesting results.

2) In my experience, the CICE's FSTD model has shown to be extremely sensitive to the wave forcing used. The realism of the simulated mean floe diameter (MFD) pattern can often be questionable (e.g. Roach et al. 2018, 2019), and FSTD has been hypothesized to be biased toward generating small floe size. This unresolved issue is likely related, among other factors, to an unphysical parametrization of the wave fracture.

   While this is not in the scope of the study, I believe it's important for the authors to acknowledge that the wave forcing has a major impact on the FSTD and, consequently, potential implications for the results, given that lateral melt depends on the MFD. I find the approach to handle the wave forcing in the study (i.e. prescribing a cyclic forcing from ERA-I) questionable.

   First, the authors mention that "although the wave forcing fields do not have any trends, the propagation of the waves into the ice field does respond to the changes in the ice cover over time". However, if the wave forcing is prescribed at each grid point from a climatology, how does it consider the change in the ice cover over time? Is the wave forcing only prescribed at the ice edge with an attenuation computed inside the ice cover? Then, how does it deal with a grid point that was in the pack ice in the 2010s, but that is ice-free in the 2040s? What wave properties are prescribed at that grid point then?

   Second, while the wave forcing is cyclic, the atmospheric forcing (including the winds) is not. If so, it does mean that after 2017 the wind pattern might not match with the wave pattern even if, in reality, those two concepts should be tightly interconnected.

   While those two concerns suggest that the wave forcing used in the HadGEM2-ES projection is unrealistic, it might not be significant for the results as long as the MFD remains somewhat realistic. Therefore, for the sake of the discussion, it would be beneficial to look at your simulated annual cycle of the FSDRAD field (mean floe diameter) in the 1980s, 2010s, and 2040s.

General comments

- While short sentences are typically preferred in a scientific document, I believe that the author might have pushed the limit a bit far in some sections. Reading the manuscript often feels a little bit robotic. I suggest trying to combine some of the short sentences to introduce variation in the sentence length throughout the manuscript and to enhance the rhythm of the paragraphs.

- The organization of the document is essential; the readers want to know where to quickly find the information they are looking for. Currently, results are spread between the methodology (section 2) and the results (section 3), which makes the story hard to follow.

  The methodology should be strictly about describing the data and the method used, so the reader has enough information to reproduce the results. This is not where you typically analyze data, even if it is for model validation. Therefore, I recommend creating a new result section called "Model validation" and moving Figs. 1,2,3,4,5,7, as well as their associated discussion to this new result section. Here is a suggestion for a preferred document organization:

1. Introduction
2. Data and methods: this should include what is currently sections 2.1, 2.2, 2.3 and 2.6.

   2.1. Model set-up and forcings or Model configurations.

   Include the content of what is currently sections 2.1 and 2.2.

   2.2. Model validation data or Observational data.

   Include the content of the current sections 2.3.

   2.3. Analysis method or methodology

   Include the content of the current section 2.6. Move lines 200-206 to the results.

3. Results: this should include what is currently sections 2.4, 2.5, 3.1 and 3.2. The author should also consider having shorter and more evocative subsection title.

   3.1. Model validation.

   Include what is currently sections 2.4, 2.5 and the part of 2.6 mentioned previously and the discussion about Figs. 3,4,5,7.

   3.2. Annual total volume fluxes.

   3.3. Annual cycle of the main melt and growth terms.

4. Discussion
5. Conclusion

- The length of the manuscript also increased a lot (i.e. it went from 15 pages to 21 pages). I believe that there are some lengths and redundancies, which can undermine the message. In the specific comments, I go through each of the sections of the manuscript, and I suggest changes and rephrasing to improve the conciseness and clarity.

Specific comments

1) **Abstract**

The abstract is the first part that most of the readers read and sometimes the only part that they will read. For this reason, it is probably the part of the manuscript that needs to be written with the utmost care. It needs to tell the story on its own while being as dense, concise, and as clear as possible.

- Line 1 and 4-5: Repetitive sentence; the authors should remove one.
- Line 5: Typo ? pack ice.
- Line 7: "*This is the first study (to our knowledge) that separately considers the pack ice and MIZ in this way*." This type of sentence should be avoided; you either know that this is the first study, or you don't. The authors should conduct the appropriate literature review to make sure that it is or remove the sentence.
- Lines 5-10: the method of the study is summarized, however, it is not mentioned that two atmospheric forcing (NCEP/HadGEM2-ES) are used. Then, in lines 11-13 when the discussion of the results starts with a comparison between these two atmospheric forcings. In short, the part of the abstract that talks about the method should directly lead to the results.
- Line 9: "*The model has been compared to floe size distribution observations.*" Is this true? I have been reading the manuscript repeatedly and I do not see any comparison with floe size distribution observations. I only see comparisons with SIC and ice thickness observations.
- Line 10: The abstract usually only has one paragraph.
- The last part of the abstract lists some of the results, but it should be instead focused on the principal conclusion of the study (i.e. the take-home message).

I believe that the reorganization proposed in the general comment will help to really highlight the story of the paper. Here is an example of how that structure could be used the convey the message more clearly in the abstract.

"The marginal ice zone (MIZ), defined as the region of the ice cover that is influenced by waves, is projected to become a larger percentage of the summer ice cover as the Arctic transitions to ice-free summers. Here, we compare individual processes of ice volume balance in the pack ice to those in the MIZ to establish and contrast their relative importance and examine how these processes change as the summer MIZ fraction increases over time. We use CICE, a physics-rich sea ice-mixed layer model forced with two atmospheric datasets; the HadGEM2-ES simulation (1980-2050), to simulate the ice cover in a high emission global warming scenario and the NCEP reanalysis (1979-2020), for comparison during the observational period. First, we compare both simulations to satellite-derived sea ice concentration (i.e. NASA Team/Bootstrap) and PIOMAS estimates of sea-ice thickness. Results show that [results from section 3.1] Then, we compare the annual volume fluxes for the following periods: low MIZ (1980s), high MIZ (2010s), and all MIZ (2040s), showing that [results from section 3.2]. Finally, we look at the annual cycle of the main melt and growth terms [results from section 3.3]. Those results highlight that [the take-home message]."

2) Introduction

The introduction does a decent job of stating the background information and covering existing literature, but I still believe that it could use some rewriting to enhance the quality of the scientific communication. Here is what I suggest the authors to keep in mind to improve the flow of the introduction.

First, "contextualize the background information"; it needs to go from the most general (i.e. general statement about sea-ice, the MIZ, global warming, etc.) to the most precise (i.e. things that are directly related to the paper, e.g. the processes affecting the sea-ice volume budget). Second, "state the problem"; this is where similar previous literature is cited, and the knowledge gap or general misunderstanding of the problem is stated. Third, "address the problem"; this is where the methods, the scope of the paper, and what is unique about it are mentioned.

Additionally, some statements are made without citation; the introduction could use additional references especially between lines 35-51.

- Line 23: applied is repeated twice. The authors should reformulate.
- Line 26: "*More fragile*" feels vague. Thinner, more mobile, or more fractured could be more accurate.

- Line 30: This is still the "contextualize the background information" part. I suggest moving the sentence to the "address the problem" part.
- Lines 32-35: This is a general statement about sea ice. I suggest moving this part to the beginning where the different SIC threshold for the MIZ definition (i.e. lines 22-25) is discussed.
- Lines 36-40: The word *"potentially"* should be avoided. This has been the subject of many studies. Add references.
- Lines 38-40: the use of "*ice-albedo feedback"* is repetitive. The sentences could be merged or rephrased to avoid repetition.
- Lines 40-45: again, the use of "*feedback"* is repetitive and vague as it is not clearly explained here. Also, the word "*action*" feels vague. Wave-induced fracture could be more accurate. See "Floes, the marginal ice zone and coupled wave-sea-ice feedback" (Horvat, 2022), to improve that paragraph. Add references.
- Line 49: Add references.
- Lines 52-64: This is the "state the problem" section of the introduction. While other papers that made a sea-ice budget are cited, more emphasis should be placed on the lack of knowledge that persists after those studies. Instead, here, some of their results are stated, but they do not appear to be directly related to the content of this manuscript. For example, "*They also found that the initial sea ice state was important in determining projected changes to the sea ice cover, with thicker initial ice resulting in more sea ice volume change*." As far as I'm concerned, the manuscript does not investigate of the effect the initial sea-ice state on the volume budget, therefore the emphasize should not be placed on that. It should instead focus on things that are relevant to the findings of this study (e.g. do they compare their results to observations? Do they analyze the processes in the MIZ and in the pack ice separately? etc.)
- Line 65: This is the "addresses the problem" part of the introduction. Again, I suggest changing the phrasing "*the first to our knowledge*". That could be implied by improving the previous paragraphs.
- Line 69-70: The emphasis should be placed on the fact that CICE has been used in previous modelling studies that focused on the representation of the MIZ.

- Line 71-77: A grocery list to describe the content of the manuscript should be avoided. The author should instead make a story out of it or remove it. This especially applies to the last part which simply states that section 4 is called discussion and section 5 is called conclusion. I think believe that the structural change proposed in the general comment will help to make a story out of the results. Moreover, a similar grocery list is made at the end of the methodology (lines 207-209), I suggest keeping only one.

3) Methodology

As I mentioned before, the methodology should not contain any figures. The only figure that could potentially stay in this section is Fig. 6, as it serves more as a mean of better understanding the methodology rather than a result.

I recommend moving Figs. 1 & 2 to the appendix as well as the discussion of those figures, to reduce the length of the manuscript.

Moreover, the section is to be called "Data and methods", as this is also where the datasets used in this study are described.

- Line 81: what is CPOM.

- Lines 86-88: "*found to give realistic simulations of observed floe size distributions (FSD) for mid-range floe sizes in the Arctic. This model, minus the brittle fracture addition to the FSTD model, has been used previously by Rolph et al. (2020) to compare changes in the MIZ in a sea ice model to satellite observations."* This should be mentioned in the introduction, especially where it is argued that CICE is a good model to represent the MIZ (i.e. lines 68-10). The method should instead only focus on describing the component of the model used.

- Lines 86-88: Also, I would be careful about saying that Roach et al. 2018,2019 give realistic simulations of the observed FSD as I do not think that there are any comparisons to observations in those studies.

- Line 90: There is no need to mention the full name of the ocean reanalysis, it can be found in the reference (e.g. "a *climatological ocean reanalysis (Ferry et al. 2011)*").

- Lines 91-92: A separate sentence for the currents is unnecessary, it can be merged.

- Lines 93-98: This paragraph can be further simplified especially if the namelist is given in the supplementary material or in the code availability section. For example:
  "*We use a number of the default CICE settings, including the layers thermodynamics of Bitz and Lipscomb (1999), Maykut and Untersteiner (1971) conductivity***,** *Rothrock (1975) ridging scheme, the delta-Eddington radiation scheme (Briegleb and Light, 2007), and the linear remapping ice thickness distribution (ITD) approximation (Lipscomb and Hunke, 2004). Additionally, we use a prognostic melt pond model (Flocco et al., 2010, 2012) and an anisotropic plastic rheology (Heorton et al., 2018; Tsamados et al.,2014; Wilchinsky and Feltham, 2006)."*

- Lines 98-114: This deserves a new paragraph; this section might need to be reworked according to the answer to the second scientific concern comment. Additionally, the authors should reorganize the paragraph by first describing the way that the wave forcing is dealt with in their own study. Then, they can briefly mention how others have done this differently and why they argue that their method is adequate. For example, the paragraph should start with:
  "The wave forcing used in this study is prescribed from on ERA-I reanalysis wave data and is repeated after 2017 (Bateson et al. 2022). The wave properties used are the significant wave height and the peak wave period, which are then extrapolated and updated every 6 hours in grid cells that contains less than 1% sea ice (Roach et al. 2018). This wave forcing set-up then differs from Roach et al. (2019) […]"

- Lines 106-109: I am not entirely convinced that the absence of trend reported in Bateson (2021) is a consensus. Many studies argue that the increase in open water in the summer that creates a larger fetch, combined with an increase in the intensity and frequency of storms in high latitudes will lead to waves of larger amplitude in the ice-free Arctic. The authors should read the following papers: *Swell and sea in the Emerging Arctic Ocean* (Thompson et al. 2014), *Growth of Wave height with Retreating Ice Cover in the Arctic* (Li et al. 2019), *Sea Ice Retreat Contributes to Projected Increases in Extreme Arctic Ocean Surface Waves* (Casas-Prat et al. 2020) and *Wind and wave climate in the Arctic Ocean as observed by altimeters* (Liu et al. 2016). It should be at least acknowledged in the paragraph that there is no consensus on that yet.

- Lines 102-103: *"The wave forcing consists of the significant wave height and peak wave period for the ocean surface waves"*. If the same FSTD model as in

Roach et al. 2019, the CICE fracture model uses a wave spectrum to compute fracture, how is the wave spectrum derived from significant wave height and peak wave period only?

- Line 110: I suggest making a new paragraph for the discussion of the spin-up (i.e. lines 110-113) or moving this section after the discussion on the component of the model (line 99).

- Line 115: At this point, the author should have already made clear that they are comparing the result from two atmospheric forcing before as this is a key part of the story. It needs to be mentioned in the abstract and the "address the problem" part of the introduction.

- Line 136: Add references.

- Figs. 1 & 2 are complementary to the main findings, but they are not essential for the story. I suggest moving them to an appendix with lines 124-136. The figure can be referred to later, when NCEP and HadGEM simulations are compared in the results. Lines 115-123 can be moved to the "*Model configuration" section.*

- Fig. 1: The colors are confusing. The author should consider using a dashed line NCEP and a solid line for HadGEM (or vice-versa).

- Line 138: If Rolph et al. (2020) have already done that comparison, why doing it again? The author needs to clearly highlight what is new about their paper and it must be clear from the "state the problem" part of the introduction.

- Lines 139-145: A basic description of the two SIC datasets used for validation should be there. How exactly is it measured? Is it both passive microwaves? Which satellite? Why is there so much difference between the datasets?

- Line 145: *"Daily values are used to compute monthly values of sea ice and MIZ extent that are plotted in Figure 3."* This should be in the caption of the figure instead.

- Fig. 3: I am not sure of the necessity of: *"Uncertainty levels of +/−10% were used for the satellite values in Rolph et al. (2020), they have been left out in these figures to make them clearer."* as it is not discussed in the text nor shown in the figure.

- Lines 146-151: Most of it can be cut as it is general sea-ice knowledge. The paragraph could be simplified as:
  "To validate our simulated sea-ice thickness, we use the Pan-Arctic Ice Ocean Modeling and Assimilation System (PIOMAS) (Zhang and Rothrock, 2003), a model that assimilates a range of sea ice area/concentration observations to give an estimate of continuous Arctic Sea ice volume changes over time. As with the satellite data, PIOMAS has been interpolated on the ORCA tripolar 1◦ grid and the CICE land mask has been applied."

- Lines 153-155: Most of it is already in the caption of Figure 3, it can be deleted and rephrased.

- Line 158: The sentence "*slighter weaker sea-ice trend"* is unclear. Is it really about the trend (i.e. the rate at which the sea-ice extent changes), or about the sea-ice extent as shown in the figure? Also, is it about the MIZ extent or the total sea-ice extent? The author can be more precise when referring to a figure (e.g. *"slightly weaker MIZ extent trend (see thick line in Fig.3)"*).

- Lines 158-159: "*Whilst*" is used in two subsequent sentences. Rephrase.

- Line 164: *"An increasing trend*" in what? Again, the authors can be more precise in their statement.

- Lines 178-179: Unnecessary, this is already mentioned in the caption. Also, this part does not require a separate section, just add that paragraph to the previous section and call it "model validation".

- Lines 187-189: Again, this is unnecessary as it is already mentioned in the caption of the figure. This comment applies to most of the figures. For example, the paragraph could be simplified as:
  "We consider three different ice cover states within the simulations: a low MIZ state in the 1980s; a high MIZ state in the 2010s; and an all MIZ state in the 2040s. In each case we use the last 5 years of daily July SIC and assign each grid cell as pack ice (SIC≥80%), MIZ (15% ≤SIC<80%) or open water (SIC<15%) (Fig. 6).

  The authors should avoid breaking the flow of the paragraph with sentences like *"Figure 6 shows … [content of the caption of Fig. 6].".* This is repetitive as the reader will already look at the caption to understand what is in the figure.

- Line 184: Typo ? "*for the scope of this study."*

- Line 198: This is already mentioned in line 141.
- Lines 200-206: This should also be moved to the new "model validation" results section as well as Fig. 7.
- The caption of Figure 6 can be simplified. Change *"Region 1 (blue) indicates the area that is pack ice in both the 1980s(2010s) and 2010s(2040s) in subplots a-d(e). Region 2 (green) indicates the area that is pack ice in the 1980s(2010s) and becomes MIZ in the 2010s(2040s) in subplots a-d(e). Region 3 (orange) indicates the area that is MIZ in both the 1980s(2010s) and the 2010(2040s) in subplots a-d(e)." to "Region 1 (blue) indicates the area that is pack ice, region 2 (green) indicates the area that is pack ice and becomes MIZ and region 3 (orange) indicates the area that is MIZ".* Identify the period in the title of the subplot (e.g. (a) Bootstrap 1980s-2010; (b) NASA Team 1980s-2010; (c)… etc.)
- Lines 207-208: Unnecessary, it could be removed.

4) Results

The figures and the description of results are much clearer than in the last iteration and the results paragraphs are straightforward. However, as I mentioned previously, the author should avoid repeating the content of the caption in the text when referring to a figure.

Also, when referring to a figure, direct and indirect citations are possible. The direct method is to directly describe the figure in the text. For example.

"The annual volume fluxes for sea ice processes are shown in Figure 8.",

The indirect method is the cite the figure in parentheses at the end of the sentence that describes the results.

"Analysis annual volume fluxes for sea ice processes show that congelation growth dominates sea ice growth […] (Fig. 8)."

More indirect references should be made throughout the text, especially for figures that do not require a lot of explanation, as it is less wordy. This will also help to avoid repeating the content of the caption. Also, while using the indirect approach use the abbreviation Fig. instead of Figure to lighten the text.

Finally, the author should consider the opportunity of using the indirect approach to add additional and more detailed references to the figures throughout the result section.

The description of the figures can often be confusing, especially for Figs. 8-10 which contains many panels, with many different lines and bars.

- Fig. 8: It is unnecessary to repeat the *"Volume flux*" in the y-label three times in the subplot. I suggest putting one y-label for the whole subplot.
- Fig. 9: "Fraction of the melt" in the y-label is inaccurate as the dynamic term is not melting. "Fraction of the total ice loss" would be more accurate. Again, the y-label does not need to be repeated. In the caption, I suggest the following phrasing: "*The evaporation term is neglected.*"
- Line 257: "*The same regions and time periods defined in Figure 6 have been used*." Already mentioned in the caption.
- Line 270: The threshold for the melting should be mentioned at the beginning of the paragraph. Is this threshold applied to the total melt or to one of the individual melt terms?
- Fig. 10: I believe that the y-label is incorrect. In Figure 8, the y-label is volume flux with units of m3/m2 and then in Figure 10 the y-label is also volume flux, but now with units of m3/m2 days. In the text, the author refers to growth and melt rates in lines 284-299 and I think that they are referring to Figure 10. I believe that the y-label should be growth/melting rate, but this needs to be clarified.
- Line 272: Which of the melting rates are you talking about? The total (black), the top (red), the lateral (purple) or the basal (yellow)?
- Lines 278-298: Are those results related to Figure 10? If so, refer to the figure in the text, at least one reference per paragraph. For example,
"In the always pack region peak melt increases and the melt season gets longer in the 2010s relative to the 1980s by 13 days in the NCEP and 6 days in the HadGEM2-ES forced simulations (Fig. 10a,d)."

5) Discussion and conclusion

The material in the discussion is adequate. I believe that the two last paragraphs of the discussion could be improved by considering all the changes mentioned previously and by adding more direct references to the results or to previous literature (just like in the two first discussion paragraphs).

In the conclusion, the author is doing a great job at summarizing their results. However, I believe that the authors are missing the opportunity to emphasize a clear take-home message. Which one of those results was unexpected, novel, or significant? Which unanswered questions were addressed in the manuscript? What are the key findings uncovered by the methodological framework? This ties back to my earlier comment in the introduction about finding the knowledge gap in the literature, which I believe is never clearly highlighted in the manuscript.

- Line 305: New paragraph?

- Line 323*: "The differences in lateral melting were very likely caused by the inclusion of the FSTD model (Roach et al., 2018, 2019; Bateson et al., 2022), although we did not directly test this within this study."* I believe that your methodological framework allows to investigate such a question for example, by comparing the mean floe diameter in the MIZ and pack ice regions. My concerns about the wave forcing should be addressed or acknowledged here.

- Line 325: New paragraph?

- Lines 331-338: While the other paragraphs bring interesting discussion points (e.g. the use of a coupled climate model vs a forced sea-ice model, the absence of warming trend in the oceanic forcing, the effects of using an FSTD and a melt pond scheme), I am struggling to understand what is the point that the author is trying to make in that paragraph. For example, with the sentence: "*Our approach also has the advantage of simplicity, the more concentration categories the MIZ is split into, the more complex the analysis becomes, and the less clear the results."* This appears to be a simple general statement that does not integrate or extend any of the results.

- Lines 343-344: Rephrase.

- Lines 344-345: *"The MIZ is defined as having a sea ice concentration (SIC) between 15% and 80% and pack ice is defined as SIC>80%."* I believe that this should be included in the first sentence of the conclusion.

- Line 370: Typo? "particularly in the region that is remains MIZ."

- Line 374: Typo? "*Our analysis demonstrates a different balance of processes control the volume budget…"*

- Line 374-378: This is a rather vague and general way of closing the paper. I believe that a stronger concluding remark could be made by including future research directions.