Author comments

Summertime changes in climate extremes over the peripheral Arctic regions after a sudden sea ice retreat

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Anonymous Referee #1

In this new study, the authors evaluate the boreal summer thermodynamic response (temperature and precipitation) to a sudden reduction in Arctic sea ice. In particular, they run targeted experiments using two fully-coupled climate models and two different horizontal resolutions for evaluating changes in climate extremes over nearby high latitude regions. Sea-ice loss is imposed in these experiments using a modified sea ice albedo scheme, which is consistent with previous studies. However, the temperature and precipitation responses are scaled by the amount of sea-ice loss, which helps to more easily compare the two climate models that show different sea-ice forcing (both regionally and in overall magnitude). In summary, the authors find an increase in frequency and persistence of maximum surface air temperature in the high latitude regions of the Arctic, which is especially large near Svalbard. In contrast, the extreme precipitation response is less robust compared to atmospheric internal climate variability.

Overall, this is a useful study for understanding the local response to rapid changes in Arctic sea-ice loss during the boreal summer. While there is an abundance of literature for assessing Arctic-midlatitude climate linkages (especially due to sea ice), there is less work on understanding the response in summer (relative to remote linkages in winter). For the most part, the methods here are logical, and the text is well-written. However, the paper is generally too long and can be substantially shortened to focus on the novel results of these climate model experiments. The introduction is quite long and confusing as it conflates winter and summer Arctic-midlatitude linkages, which are quite different in their respective dynamical tropospheric/stratospheric mechanisms and pathways. Instead, it would be helpful to compare this study's results with those that have focused on summertime changes within the high latitude regions. Again, focusing on 'what is new here.' I have some more detailed comments, suggestions, and references below. After some shortening of the main text, this paper should be acceptable for publication in Weather and Climate Dynamics.

Reply: We thank the reviewer for all the comments and suggestions to improve our manuscript. We take note that the paper can be shortened to focus on the novel results especially in the introduction as Reviewer #2 suggested to revisit this part.

Action: We have made the following modifications to the manuscript:

- Parts of the introduction were shortened. Indeed, the paragraph about climate impacts in winter (second paragraph) was removed. Climate impacts in summer are now reported, with references such as Coumou et al. (2018), Horton et al. (2016) and Kidston et al. (2015).
- Parts of the section "Results and discussion" were also deleted to better highlight the novel results. More specifically, L250-251, L252-253, L272-274, part of L286, part of L290, L299, L313-315 of the previous version of the manuscript were removed.
- Results from studies about extreme changes in Arctic regions are now discussed in the introduction and in the section "Results and discussion" (Matthes et al. (2015), Kharin et al. (2018), Meredith et al. (2019), Chernokulsky et al., (2019), Dobricic et al. (2020) and Landrum and Holland (2020)). This discussion allows us to better put our results into perspective.

Specific Comments:

1. L2-3; This sentence is a bit confusing to me. It makes it sound like this is the first study to conduct experiments with a large reduction in Arctic sea ice, which is not the case.

Action: This sentence was changed into “Despite the existence of many studies, it is still unclear how the atmosphere will respond to a near-total retreat of summer Arctic sea ice…”.

2. L18-82; This introduction is quite long. I think it can be more concise by focusing on the direct connections with this work (e.g., L37-62), rather than restating Arctic climate change, which is already well documented in plenty of studies.

Action: Parts of the introduction were shortened. In particular, the paragraph about climate impacts in winter was removed. Climate impacts in summer are now discussed with references such as Coumou et al. (2018), Horton et al. (2016) and Kidston et al. (2015).

3. L28; Latest data from observations/reanalysis reveal that warming is now at least “three” times as fast as the global average

Reply: Thanks for this update. This is not mentioned anymore as parts of the introduction were shortened.

4. L29; Ballinger et al. (2020) can be updated to the newest Arctic Report Card 2021 (referenced below)

Reply: Not mentioned anymore as parts of the introduction were shortened.

5. L35; Review papers, such as Cohen et al. (2018) and Overland et al. (2021), are more appropriate studies to cite here

Reply: Not mentioned anymore as parts of the introduction were shortened.

6. L44-46; This might be the case for historical forcing, but some studies have shown that the remote response to future Arctic amplification is more robust than sea-ice loss alone later in the 21st century (see Labe et al. 2020)

Reply: This sentence has been deleted during the process of re-writing the introduction.

7. L52-53; In my view, this is a key/novel point for this study and should further be distinguished in the introduction (rather than conflating studies that have only focused on the peripheral response during winter).

Action: A new paragraph (the third one) has been added which highlights better the originality of our study.

8. L52-53; Coumou et al. (2018) should be cited here. See references within for summertime studies.

Reply: Thanks for this suggestion.

Action: Coumou et al. (2018) has been added in the new paragraph and other important references (Horton et al, 2016; Kidston et al., 2015; Matthes et al., 2015; Dobricic et al., 2020 ; Meredith et al., 2019) were added in this paragraph and the next one.

9. L70-71; Are the perturbations here realistic compared to CMIP projections of sudden/rapid ice loss events (on this timescale)?

Reply: No, these perturbations (in sea ice loss) are unrealistic, especially for the CNRM model which simulates a large amount of sea ice loss.

Action: This information is now added in section 3.1.

10. L79-82; This outline paragraph is extra text and unnecessary.

Action: This paragraph was removed.

11. L88; This table is very helpful!

Reply: Thank you.

12. L107-108 & L127; Do the models include an internally generated QBO? This has been shown to modulate the response to Arctic sea-ice loss in some climate models.

Reply: Yes, for the CNRM-CM6-1 model, Richter et al (2020) noticed that the model simulates abroadly realistic QBO. For ECMWF, we have analyzed the zonal mean component of the wind (averaged between 5°S-5°N) from the control run at both resolutions with a Hovmoller plot (see below). We can observe a QBO-like variability with a magnitude and a period which are consistent with models which simulate well the QBO in Richter et al (2020).

Action: We added a sentence for each model at the end of section 2.1.1 ("Both configurations of the model simulate reasonably well the Quasi-Biennial Oscillation (QBO) variability (not shown).") and 2.1.2 ("and enable to simulate the QBO (Richter et al., 2020).")

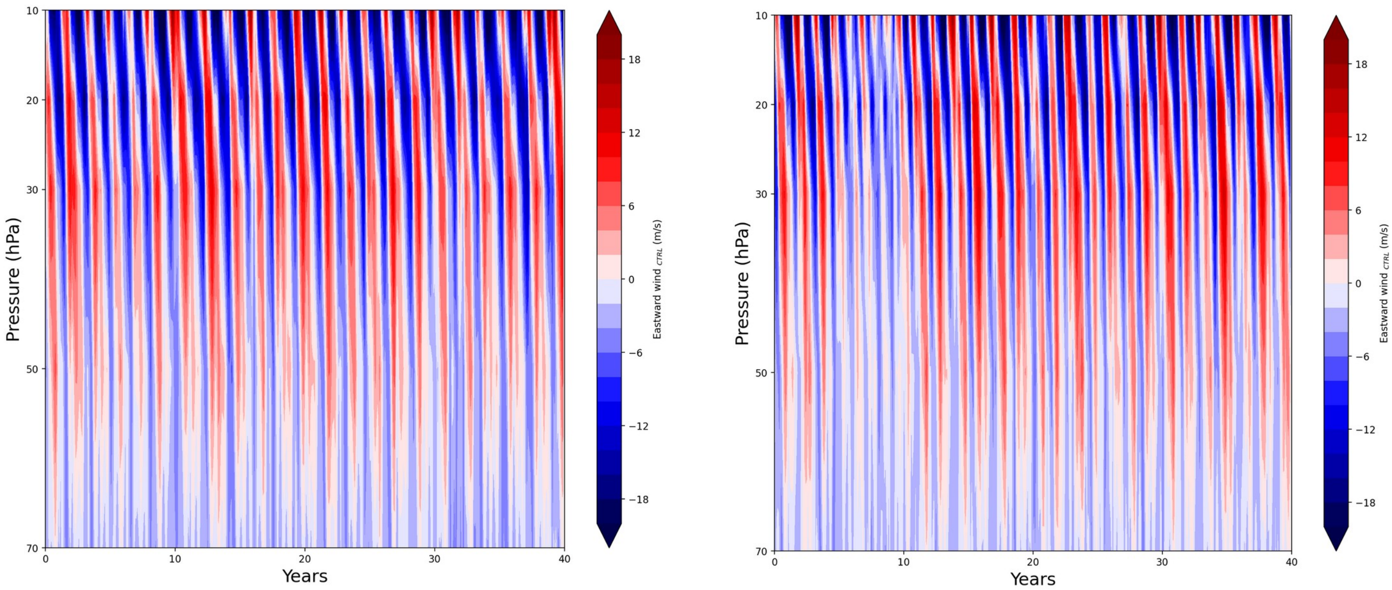

Figure : Zonal mean wind (m/s) zonally averaged between 5°S and 5°N in CTRL for ECMWF-LR (left) and ECMWF-HR (right). Eastward wind has a positive value.

13. L139-140; Sun et al. (2021) compared methods between modifying ice albedo and nudging

Action: This citation has been added. Moreover, a discussion in the section 3.1 has been added :"Using the albedo reduction technique underestimates the sea ice loss in winter, and thus impacts the magnitude of the climate responses (Sun et al., 2020). Nonetheless, a good consistency in these responses among different techniques to impose sea ice reductions has been observed (Sun et al., 2020). Moreover, the albedo reduction technique estimates well the sea ice loss during summer, the season studied here, compared to other techniques (Sun et al., 2020)"

14. L142-143; Although this may still be an underestimate (Peings et al. 2021) for some of the regions outlined in Figure 2 (e.g., "ER")

Reply: Indeed, this is why Fig. 7 is shown where we can see the signal to noise ratio (S/N). For the maximum surface air temperature (Fig. 7a), the signal is usually larger than the noise poleward of 50-60°N but, southward in some regions, the S/N can be below 1. For the precipitation response, the noise is larger than the signal in all regions. Actually, even in Screen et al. (2014), some regions in high latitudes (Greenland, Scandinavia) do not show a significant response in surface air temperature even with 100 members.

Action: The sentence is now less strong and suggests that even 40 members could not be enough in some high-latitude regions : "This number of members was chosen because it allows to reach a good level of statistical significance in several high latitude regions, mainly in the surface air temperature response (Screen et al., 2014), without demanding too much computing time"

15. L154; This is just a suggestion, but it might be helpful putting these climate indices in a table. It can be a bit cumbersome to read in the paragraph.

Reply: Yes, this is a good idea.

Action: Table 2 was added.

16. L188; This study is not relevant here.

Action: The study of Walsh et al. (2017) is now cited. Onarheim et al (2018) used the data from this paper to show the sea ice extent before 1979.

17. L194-196; How does the effective sea-ice thickness actually change between PERT and CTRL in these experiments? Is the mean state realistic, such as compared to CryoSat-2 or PIOMAS? Changes in sea-ice thickness can influence surface turbulent fluxes and thus the local thermodynamic response.

Reply: The models are integrated with a constant forcing (year 1950), which makes a comparison to historical sea ice thickness estimates difficult, since they are only available after 1979 (PIOMAS) or 2003 (CryoSat) and correspond to a transient climate state. If we look at the year 1980 in PIOMAS, the sea ice thickness maximum was around 2.6m and the minimum was around 1.8m. This suggests that ECMWF seems closer to observations compared to CNRM which underestimates the sea ice thickness.

Action: Two new figures were added in the Appendix (Figs. A1 and A2) showing the change in sea ice thickness between PERT and CTRL. Moreover, in section 3.1 after "with a large mean sea ice thickness in the ECMWF configurations" we have added ", which is closer to first estimates (Zhang and Rothrock, 2003) than CNRM,".

18. L209-210; Again, could this also be due to difference in the sea-ice thickness mean state?

Reply: Yes the large amount of sea ice thickness in the ECMWF model compared to the CNRM one leads to a loss of only around 20-25% of sea ice thickness in PERT (in ECMWF). In summer in PERT, the EMCWF model still simulates 2m-thick sea ice, whereas the CNRM has no sea ice.

Action: "[in ECMWF, which still simulates more than 2m-thick sea ice in PERT in summer (Fig. A1b),] and can limit the warming in that model". The part in bracket has been added

19. L224-235; Is there any role for changes in ocean heat transport that result in the temperature response? This is one advantage in using the fully-coupled experiments here.
Reply: Following the sudden reduction in albedo in our model experiments, Arctic sea ice extent decreases strongly (Figs. 3-4), which leads to increased surface air temperatures over the Arctic Ocean (Fig. 5). We think that this surface warming is a direct effect of the albedo reduction via increases in open water (ice-albedo feedback), rather than an indirect effect via increased ocean heat transport. Indeed, the impact of sea ice changes on ocean dynamics usually operates on longer time scales (e.g. Wang et al., 2018).

20. L228; This is not necessarily the case during summer, where sea ice is mostly confined to the Fram Strait and northern Greenland.

Action: We have added "partially surrounded by sea ice"

21. L228-229; And major differences in topography/elevation

Action: We have added "where the altitude is high" after "over central Greenland".

22. L258-264; This transition paragraph is a bit confusing to me. I suggest rewriting to improve clarity for the readers, especially when discussing the effect of temperature extremes over the continental regions. While there is a quick investigation of the NAO response, is it possible there is another dynamical contributor to the changes in temperature extremes (i.e., not just a turbulent heat flux response)?

Reply: Yes, other dynamical contributors could be responsible for the change in temperature extremes such as blocking events (which can be related to the NAO index), but this has not been explored here. Furthermore, the main effect at high latitudes remains the thermodynamical effect.

Action: This paragraph has been modified and clarified. A figure in the appendix (Fig. A4) was added and reveals that the change in extreme persistence (Warm spell duration index) hardly extents to continents.

23. L324; Some studies have considered the response to very rapid ice loss events (e.g., Semmler et al. 2016)

Reply: Yes we agree. However, Semmler et al (2016) looked at winter sea ice loss.

Action: Changed from "not been explored" into "been little explored".

24. L325-328; Sorry, but I am not sure I understand what you mean here.
Action: We tried to clarify this part : "To our knowledge, this study is the first one to address this last question in depth following a coordinated (fully coupled) two-model approach in which idealized albedo experiments have been conducted. These experiments help to isolate as much as possible the effect of the Arctic sea ice loss without confounding factors, such as a change in sea surface temperature or in radiative forcing."

25. L334-335; It may be helpful to remove “cold days” and “warm days” to improve interpretation of this result

Action: Changed

26. L348-350; But this could be a product of assessing responses in summer versus winter
Reply: Yes, we agree that the atmospheric responses in winter could be more related to the dynamical effect than in summer.

Action: We have added “in summer” to be more precise. The sentence was changed from “This shows the minor importance of the role of the dynamical response, which tends to be non-linear (Petoukhov and Semenov, 2010), compared to the role of the thermodynamical response in high latitudes” into “This shows the low importance of the role of the dynamical response in high latitudes, which tends to be non-linear (Petoukhov and Semenov, 2010), compared to the role of the thermodynamical response in summer”

27. How do these horizontal resolution results compare to Streffing et al. (2021)?

Reply: Streffing et al. (2021) have not observed a significant change between different horizontal resolutions and we also observed that even if this is not the main focus of this study.

Action: The parallel with Streffing et al. (2021) has been added for the surface air temperature response in the first paragraph of section 3.2 and for the precipitation response in the third paragraph of section 3.3.

Technical Comments:

1. L19; “more pronounced in [late] summer”

Action: Done

2. L274; change “models” to “experiments”

Reply: We think keeping “models” here seems more relevant because “experiments” is more related to the perturbation or the control experiments in this study.

Figures/Tables:

1. Figures 6/10; Is there any way that statistical significance could be denoted here? For example, comparing the CTRL and PERT PDFs in each respective region and adding a star for statistical significance.

Action: A star next to the name of the region shows if the distribution change is statistically significant according to a 5% level Kolmogorov-Smirnov test.

Appendix:

1. Figure A1; Are any of these changes in the NAO statistically significant? If so, could they be indicated on the graph?

Action: The p-value of a Kolmogorov-Smirnov test between PERT and CTRL is now given below each boxplot

2. Could the authors include a data availability statement for the climate model experiments?

Action: A data availability statement has been added: “Data availability. The data from ECMWF model can be accessed using www.jasmin.ac.uk via https://prima-dm1.jasmin.ac.uk. The data from the CNRM model are openly available and can be shared upon request.”

Anonymous Referee #2

The authors examine changes in climate extremes in temperature and precipitation in the Arctic summer using 40-member climate model ensembles where the sea ice albedo is replaced by the open ocean value. They include results from two different climate models ad two different resolutions, allowing some sampling of model uncertainty. The authors focus on impacts in the peripheral Arctic regions, where changes will likely have societal relevance. This is a welcome addition to the literature, which has often focused more on Arctic-midlatitude linkages, rather than impacts in the peripheral Arctic regions. The methods are generally appropriate, although some aspects require clarification. Some of the presentation and discussion should be improved.

As noted by Sun et al. (2020, https://doi.org/10.1029/2019GL08578) and others, the use of albedo reduction to investigate the response to sea ice loss mainly impacts summer sea ice, with little to no impact during the winter. This can be seen in the present manuscript in Fig. 3, panels c and d. However, satellite observations show a decrease in winter sea ice area over 1979-present, and CMIP6 models project this to increase over the 21st century (see e.g. Fig. 2a in SIMIP Community 2021, https://doi.org/10.1029/2019GL086749). This manuscript focuses on summer, but the authors should add some discussion on the seasonality of sea ice loss imposed in these experiments, and how that may affect the interpretation of the results. The existence of other techniques for considering the impact of sea ice loss and the seasonality is hinted at in the Conclusions at L357, but I think it deserves more attention in the manuscript, perhaps in the Discussion.

Reply: We thank the reviewer for these interesting comments and suggestions. We think that the results are not significantly influenced by the type of experiments used in this study (albedo) compared to other techniques such as nudging or ghost forcing. As reported in Sun et al. (2020), albedo experiments underestimate the sea ice loss in winter. However, our results only show the response in the first summer (July to September) after a significant Arctic sea ice loss (actually starting in April (Fig. 3c,d)). As shown in Sun et al. (2020), the change in net surface heat flux in summer is really close among the three techniques mentioned above.

Action: A discussion in the section 3.1 has been added : “Using the albedo reduction technique underestimates the sea ice loss in winter, and thus impacts the magnitude of the climate responses (Sun et al., 2020). Nonetheless, a good consistency in these responses among different techniques to melt the sea ice has been observed (Sun et al., 2020). Moreover, the albedo reduction technique estimates well the sea ice loss during summer, the season studied here, compared to other techniques (Sun et al., 2020)”.

I think the Introduction should be revisited – some of the logical flow is a bit unclear.

Action: Parts of the introduction were shortened. Indeed, the paragraph about climate impacts in winter (second paragraph) was removed. To account for comments made by reviewer #1, climate impacts in summer are now reported, with references such as Coumou et al. (2018), Horton et al. (2016) and Kidston et al. (2015), to better highlight the originality of our study.

Importantly, there is no data availability statement.

Action: A data availability statement has been added: “Data availability. The data from ECMWF model can be accessed using www.jasmin.ac.uk via https://prima-dm1.jasmin.ac.uk. The data from the CNRM model are openly available and can be shared upon request.”

Specific comments

L44: I don’t follow the logic of the sentence beginning ‘Furthermore’

Action: Finally, this sentence was removed during the process of re-writing the introduction.

L55: I think this sentence is too strong. Suggest rephrasing to ‘An increase of climate extremes (…) can have substantial impacts on human activities’ or similar.

Action: This sentence was changed into “An increase of climate extremes (frequency, intensity or persistence) can have greater impacts on human activities ...”

L58: Suggest adding a reference to Landrum & Holland (2020, http://dx.doi.org/10.1038/s41558-020-0892-z) somewhere in this paragraph.

Action: The following sentence was added: “Moreover, a "new Arctic" climate could even emerge during this century (Landrum and Holland, 2020)”.

L159: Can you explain further why these eight variables were chosen? I don’t understand what is meant by ‘because they can show extreme changes in frequency or in persistence’

Action: The following sentence was added: “These indices are able to show extreme changes in surface air temperature and in precipitation over high latitude regions because they use either a relative change based on a percentile or a threshold suitable for these regions, such as a threshold to 0°C for the ice days index.”

L165: I understand that the naming convention of ETCCDI, but the term ‘ice days’ is confusing here, since it doesn’t refer to the presence of sea ice, but rather than the occurrence of freezing conditions.

Action: This was clarified by adding the sentence: “This last index (ID) should not be confused with sea ice conditions”. Using another name such as freezing days could be likened to the index “frost day” which is not exactly the same index.

L181: Are land and ocean grid cells treated equally in these regions? At L255, the text says that Screen et al. 2015 takes oceanic areas into account – does this manuscript not do that? It might also be helpful to add the fraction of each region that is land/ocean in Table 2.

Reply: Thanks for this important remark. Only continental areas are taking into account when referring to the different areas in this manuscript.

Action: “only the continental grids of each region are used in this study” has been added in section 2.4.

L181: May be worth stating that there is no longitudinal variation in grid cell area.

Action: “There is no longitudinal variation in grid cell area” was added in section 2.4.

Figure 3: Please add the satellite observations to the top panels.

Reply: The constant forcing in these experiments corresponds to the year 1950 and no satellite retrievals are available for that period. Moreover, the perturbations experiments are unrealistic (in sea ice loss) compared to observations and projections, especially for the CNRM model in which a large amount of sea ice loss occurs (this information is now mentioned in section 3.1). We do not think that adding the observations would be relevant here.

L193: ‘in the Eastern Arctic’ – I would highlight the Barents Sea here.

Action: We have modified to “in the Barents and Kara Seas and in the eastern Arctic” to be more precise as in HR the loss in not largest in the Barents Sea (Fig. 4c).

L194: Suggest softening this sentence from ‘The main reason for these discrepancies’ to ‘These discrepancies may arise due to’, as it could be due to other factors.

Action: This sentence was modified following reviewer’s suggestion.

L196: Can you comment on which is more realistic? It would be helpful to show sea ice thickness maps in the Appendix.

Reply: The models are integrated with a constant forcing (year 1950), which makes a comparison to historical sea ice thickness estimates difficult, since they are only available after 1979 (PIOMAS) or 2003 (CryoSat) and correspond to a transient climate state. If we look at the year 1980 in PIOMAS, the sea ice thickness maximum was around 2.6m and the minimum was around 1.8m. This suggests that ECMWF seems closer to observations compared to CNRM which underestimates the sea ice thickness.

Action: Two new figures were added in the Appendix (Figs. A1 and A2) showing the change in sea ice thickness between PERT and CTRL. Moreover, in section 3.1 after “with a large mean sea ice thickness in the ECMWF configurations” we have added “, which is closer to first estimates (Zhang and Rothrock, 2003) than CNRM,”.

L230: Which kind of ice? Sea ice?

Action: “sea ice” was added.

L147 and Figure 5 caption (and elsewhere) – when referring to ‘amount of sea ice loss’, is this total Northern Hemisphere sea ice area loss or sea ice volume?

Action: “the amount of Arctic sea ice loss” has been modified by “the amount of Arctic sea ice extent loss” in this line and in the figure captions.

L244: Please clarify how the regional average metric is computed. Regional average of temperature first, then compute e.g. number of days below freezing?

Action: The way to compute the regional average metric is now explained in section 2.4.: “The eight climate extreme indices are first determined for each grid cell, then the regional average is computed”

L263: Why not show this? This seems like an interesting point.

Action: A figure in the appendix (Fig. A4) showing the change in extreme persistence (warm spell duration index) was added to see that extreme indices of maximum surface air temperature hardly extent to continents.

Technical comments

L11: Remove 'statistically speaking' – how else would you define robustness?

Action: Removed.

L19: Replace 'Actually' with 'In particular', or similar

Action: Replaced by "In particular".

L30: Replace 'associated to' with 'associated with'

Reply: This sentence is not part anymore of the manuscript as parts of the introduction were shortened.

L40: Replace 'the same confounding factor' with 'a common factor'?

Action: Replaced.

L52: Replace 'less attention was paid' to 'less attention has been paid'

Reply: This sentence was removed from the text.

L60: Replace 'The projected Arctic sea ice loss can be responsible' with 'The projected Arctic sea ice loss could be responsible'

Action: Replaced.

L150: I found this wording a bit confusing. How about 'which limits spurious local test rejections', or similar?

Reply: Indeed, that sounds better.

Action: Modified following your suggestion.

L191: Replace 'even if the same protocol has been applied' with 'although the experimental set up is the same'?

Action: Replaced.

L196: Replace 'could lead to more sea ice in this model' with 'could lead to more sea ice being retained in this model'?

Action: Modified by "which could lead to more sea ice being retained in PERT in this model".

L207: Replace 'continents' with 'landmasses' – the places named are not continents

Action: Done.

L226: Replace 'a further south island' with 'an island further south'

Action: Replaced.

L227: Replace 'is the second region in terms of warming' with 'is the region with the second-strongest warming'

Action: Replaced.

Figure 5 caption: Replace 'associated to' with 'associated with'

Action: Done for all the captions (Figs. 5, 9, 12, A4 and A5).

L261: Replace 'the importance to study' with 'the importance of studying'

Action: This part of the sentence was changed into "highlighting the usefulness of studying the response of extreme …".

L263: Replace 'extreme frequency' with 'frequency of extremes' and similarly for 'extreme persistence'

Action: Replaced.

L289: Missing 'm' in symmetrical

Action: Added.

L294: Replace 'in the northern Canada' with 'in northern Canada', 'generates' with 'generate'

Action: Done.

L350: Avoid using the word 'probably' -it's vague.

Action: Replaced by "could produce".

Figure 6 – Is there a better way of showing the PDFs for the different regions besides adding 0.1 to the y-axis for each region?

Reply: We have tried but, unfortunately, we did not find another better way.

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