nature portfolio

Peer Review File

The role of sea ice in present and future Arctic amplification

Corresponding Author: Dr Seong-Joong Kim

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Kim,

Your manuscript titled "Potential disappearance of cold-season Arctic amplification in a future ice-free climate" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. Specifically, a revised manuscript must:

1. Fully investigate the role of lapse-rate feedback on ice-albedo feedback by comparing autumn-winter fluxes with spring-summer fluxes.

2. Fully justify the selection of the two 20-year periods with a clear rationale, and consider alternative methods or longer averaging periods to reduce the influence of decadal variability, while acknowledging and critically examining the limitations of CESM2 in simulating Arctic sea ice formation.

We hope you will find the reviewers' comments useful as you decide how to proceed. If additional work allows you to either incorporate or refute these criticisms, we will be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

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An updated and completed version of our Reporting Summary must be uploaded with the revised manuscript
You can download the form here:
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For your information, you can find some guidance regarding format requirements summarized on the following checklist: (https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This submission sets out to explore how the nature of ‘Arctic amplification’ may change as a result future GHG emissions. Their investigation is focussed on quantifying the influences of the numerous physical processes which can impact on the AA, and how these in themselves can change over time. The manuscript has the potential to make a significant contribution to the literature, but it is not quite there yet. Before I would be able to recommend acceptance, there are a number of issues which need to be addressed.

As a broad comment, the arguments presented here are, by their nature, very complex and some readers might have trouble grasping the full story. I strongly suggest breaking up the text into subsections where relevant. That way, the reader has a clear idea of what is to be tackled in the next part of the analysis.

Lines 39-: In these introductory comments I was surprised that the authors did not refer to Polar Amplification Model Intercomparison Project. The basic paper for this refers to many of the aspects discussed here. Reference should be made to …
Doug M. Smith, James A. Screen, Clara Deser, Judah Cohen, John C. Fyfe, Javier Garcia-Serrano, Thomas Jung, Vladimir Kattsov, Daniela Matei, Rym Msadek, Yannick Peings, Michael Sigmond, Jinro Ukita, Jin-Ho Yoon and Xiangdong Zhang, 2019: The Polar Amplification Model Intercomparison Project (PAMIP) contribution to CMIP6: Investigating the causes and consequences of polar amplification. Geoscientific Model Development, 12, 1139-1164, doi: 10.5194/gmd-12-1139-2019.

Line 41: Valuable to also reference the study of Screen, J.A. et al., 2018: Polar climate change as manifest in atmospheric circulation. Curr. Clim. Change Reps., 4, 383-395.

Lines 49-51: Very helpful here to also add to the References the study of Screen, J. A., Deser, C. et al. 2012. 'Local and remote controls on observed Arctic warming', Geophys. Res. Lett. 39, L10709, doi: 10.1029/2012GL051598.

Lines 53-62: In this introductory survey it would be helpful to include reference to the recent analysis of
David Docquier, François Massonnet, Francesco Ragone, Annelies Sticker, Thierry Fichefet and Stéphane Vannitsem, 2024: Drivers of summer Arctic sea-ice extent at interannual time scale in CMIP6 large ensembles revealed by information flow. Scientific Reports, 14, 24236, doi: 10.1038/s41598-024-76056-y.
They used the ‘Liang-Kleeman information flow’ method in conjunction with CMIP6 simulations (1970-2060) period to quantify the extent to which fluctuations in winter sea-ice volume, air temperature and ocean heat transport drive changes in subsequent summer Arctic ice coverage.

Lines 56: Important point made here on key role is played by the water vapor feedbacks. Beneficial here to also reference paper of Lee, S., T. Gong, S. B. Feldstein & co-authors (2017), Revisiting the cause of the 1989-2009 Arctic surface warming using the surface energy budget: Downward infrared radiation dominates the surface fluxes, Geophys. Res. Lett., 44, 10,654–10,661.

Lines 67-71: To put this in a broader context reanalyses perhaps mention that reanalyses show the Arctic lapse rate to have decreased significantly in all seasons except summer (Li et al., 2021: Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity. Ann. New York Acad. of Sciences, 1504, 167-186.

Lines 149-151: Reword this sentence, as its overall meaning is not clear. I suspect a parenthesis may have been placed in the wrong location.

Line 157: Delete the '~' here. The text within specifies precisely '-0.386'

Line 173-177: This insensitivity is not surprising, given that the sea ice boundary is traditionally taken at SIC = 15% (Simmonds et al. (2015) Comparing and contrasting the behaviour of Arctic and Antarctic sea ice over the 35-year period 1979-2013. Ann. Glaciol. 56(69): 18-28).

Lines 249-254: Suggest saying 'not linear' instead of 'non-linear'.
A very interesting aspect of Figure 4a is the INCREASE in the AA ratio from 2031 to 2091. Fig. 4b indicates that that most months of the year contribute to this annual increase. This reversal is somewhat surprising, and is worth a few words on potential explanations.

Lines 264-266: I like Figure 5 very much. It shows a lot of important information in a manner which is easy to digest.

Lines 300-307: The year-round warming below 200 m shown in Fig. S6b and c actually increases with depth to be a max at 400 m (the lowest level shown). This represents a marked decrease static stability in the layer. Some explanation of the origin of this feature should be presented.

Lines 365-366: The Sun-Seon Lee et al. (2024) paper referred to here specifies these forcings as associated with '… the reduction of fossil and industrial CO2 emissions to zero by 2250'
This is in contradiction with the stated text here ('… despite the prescribed decrease in CO2 concentration after 2250'). Please correct this. This is important in that there is actually no contradiction in the comment that 'the simulated global warming appears to persist'. Clarify the text here.

Line 428: Better to replace 'More specifically' with 'For example'

Reviewer #2 (Remarks to the Author):

Peer review of "Potential disappearance of cold-season Arctic amplification in a future ice-free climate" by Chung et al.

The manuscript concerns the Arctic temperature amplification. Based on model experiments, it discusses development of this amplification into the future when sea ice in the Arctic is expected to disappear. By applying the kernel method, the role of ice-albedo and lapse-rate feedback is discussed. The manuscript present many interesting and creative approaches to explore the dependence of the lapse-rate feedback on that of sea ice. I am impressed of all the interesting information that is condensed in to this article format of limited length. I am in favour of this work, although I see one flawed analysis that should be corrected. Although in many places the text is well written, it suffers at other places of sentences difficult to follow, often due to very long sentence constructions. Hence I recommend major revisions.

Major points:

1. I am sceptical to the relevance of the Extra-Arctic SAT experiment with the atmosphere-only model, CAM6. It is highly unlikely that the possible atmospheric energy-transport change in this experiment is comparable to the actual change between 4xCO2 and pre-industrial worlds, as this latter energy-transport change comes as a response to the full 4xCO2 climate changes. If the idea is to explore the role of the energy-transport change, as appears to be the case from L199-200, ideally the full energy transport changes from coupled 4xCO2 minus 1xCO2 should be implemented as a forcing to the CAM6 model, which is a difficult task to do. Such an approach has been taken in a coupled experiment, see Graversen and Langen, J. Clim., 2019.

A quick fix is to remove these extra-arctic SST experiments and the associated text, as they cannot be used for the effect of the energy-transport change, and probably for nothing else. However, as I understand the intension by the authors, the idea is to decompose the lapse-rate feedback into ice and non-ice related contributions. What then about the following:

The full lapse-rate feedback from both ice and non-ice source is given by the different of experiments constructed as 4xSAT+4xICE+4xCO2 minus 1xSAT+1xICE+1xCO2 (which the authors already have, red line in Fig. 3); the lapse-rate feedback from the non-ice source is given from 4xSAT+1xICE+4xCO2 minus1xSAT+1xICE+1xCO2, where the first of these has to be constructed. The difference between these two sets of experiments would give the ice-source contribution to the lapse-rate feedback.

2. Fig. 2: This is a very interesting investigation of the role of ice-albedo feedback during summer in the lapse-rate feedback during fall. Could the role of the lapse-rate feedback on the ice-albedo feedback be investigated in a similar analysis, but where autumn-winter flues are compared to the following spring-summer fluxes? This relates to the sentence L161-162.

3. Some sentences are hard to comprehend, often since they are too long and can favourably be shorten. E.g the sentences starting in L182 and L399, but many others too. In fact although the authors impressively manage to include massive interesting information, I still see places where the text can be shorten; I recommend to look carefully over the text.

Minor points:

1. Perhaps a more accurate title is: “The role of sea ice in present and future Arctic temperature amplification.”

2. L31: “continued occurrence of AA” -> “AA continues over the 22 century”.

3. L42-44: The text needs references.

4. L46: “in” after “than”.

5. L49: “sea-ice-covered”, with hyphens.

6. Paragraph starting at L51: Although the role of the lapse-rate feedback and other processes are relevant, this discussion lacks a major point about seasonality and the ice-albedo feedback: Due to enhanced summer ice melt, energy is taken up by ocean during summer (causing little AA in this season) and released primarily during autumn and winter contributing to AA during these seasons. See e.g. Chung et al, Earth's Future, 2021. This aspect should be mentioned in this paragraph, in particular since this appears to be a relevant point latter in the manuscript.

7. L59: “that in” after “to”.

8. L60-61: Comma after “Arctic”, “in the tropics/at lower latitudes” after “whereas”, remove “in lower latitudes” after “feedback”.

9. L65 and L71: “surface-albedo” with hyphen.

10. L73-76: Also Hwang et al. Geophys. Res Lett, 2011; Graversen and Langen, J Clim, 2019. The latter is relevant also for the next paragraph.

11. L82-83 “time-slice climate-model” with hyphens.

12. L116: “or” -> “and it”, “some” after “over”.

13. L127: “-vapour” after “water”.

13: L137: “.. stored in the Arctic Ocean during the sunlit season ..”, also note that energy is in the summer used for melting; with an increased annual cycle of sea ice this is another way to transfer energy from the summer to the winter season.

14. L149: “the” -> “this”, “is comparatively less tight” -> “show comparatively larger variability relative/less good fit to the regression slope”.

15. L152: “is in turn” -> “, in turn, is”.

16. L153: “therefore” as first word in the sentence.

17. L159: Not so clear how the number is calculated, and in particular its uncertainty.

18. L184-188: remove “however”, “fully” after “be, remove “alone” and the rest of the sentence since this part seems not necessary as the sentence already started with this point.

19. L198: remove “are able to”.

20. L241-242: “(i.e., a4SST minus piSST)”, I don't understand we can see the importance of sea-ice from this experiment, as the sentence seems to indicate.

21. L258: “hold up” -> “decrease”.

22. L280-281 “comparable to that in the current climate”, what is compared to the current climate?

23. L352: “SSP3-7.0”, please indicate how strong this forcing scenario is compared to other common SSP scenarios.
24. L404: Unclear what “those” refers to. The entire sentence does not give much meaning to me.

25. L405-407: This sentence should go to the end of the next paragraph.

26. L408-410: This sentence should go to after the end of the sentence in L399, but without the Fig. SI7 reference. The following sentence should start immediately after the one ending in L405.

27. L412: Not clear what "parallel to" means in this context.

28. Mathematical expression at L441-442: These variables need to be defined. Note that only TOA longwave flux associated with the Planck feedback is defined above, not the feedback parameter itself.

29. Figure reference L444: This figure does only consider radiative fluxes, and not temperature contributions which is indicated in the sentence here.

30. L458-459: This is not much of a statement concerning data availability ..

31: L644: “arctic mean”, over what area?

32: L646: “simulations with CESM2”, atmosphere only simulations are strictly with CAM6 and not CESM2.

33. Experiments mentioned in L650-651: Please indicate in the Methods section how these experiment names are constructed based on the experiments mentioned there.

34. L666-667: “the magnitude of the ensemble mean is greater than two standard deviations across the ensemble members.” Always? Is it the magnitude minus one which is considered? (A value of one indicates no amplification.)

35. L681: “vapour” after “surface”.

36. L717-718: The same here as in point 24: is comparison to 1?

37. Fig. SI7: What is the difference between "Sea ice" and "Polar sea ice"?

Reviewer #3 (Remarks to the Author):

This is an interesting study. The authors investigate changes in Arctic amplification (AA, 1991-2010 minus 1951-1970) and the attribution factors based on model simulations. The topic is important and worth to be further investigated. It is a thorough analysis; however, I do have several concerns regarding their discussion and results:
1) The concept of AA has been defined as the warming in the Arctic compared to the global mean. Keep this in mind, I suggest to revise the sentence in line 40 (“in comparison to lower latitude”), which is not accurate.
2) What is the reason for you to choose comparing the AA changes in the two 20-year period? 20-year average could be impacted greatly by the decadal variability, and so it is hard to extend current analysis to the future. The selection needs a justification, especially we are in 2025 already.
3) Figure 1a shows the monthly AA index based on observations data sets and model simulations. The large differences between the observations (colored markers) in the cold season need to be addressed before moving forwards. What if you change the averaging period? Will you see similar pattern if you choose another period (say two 30-year average, or moving the time axis by 10 year) for comparison?
4) Supplementary Figure 4 is something bothers me the most: CESM2 model simulations show sea ice extent in March is completely zero starting from about year 2200. Giving that the winter in the Arctic is completely dark and cold (no incoming solar radiation for several months), so sea ice will be formed. This figure (Supplementary Fig. 4c) shows that model has serious problem interpreting the physics of the Arctic system.

A few minor points:
1) Line 96-97: not clear to me what do you mean. Summer AA factor is close to 1 is not associated with albedo effect.
2) Would you please specify clearly which months are included when you say “summer months”, “cold season”, “late fall” etc.
3) Line 186: “longwave feedback” ? missing word here
4) Lines 230-231: what is “the former”, and what is “the latter”? It’s better to state them out clearly.

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Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Kim,

Your revised manuscript titled "The role of sea ice in present and future Arctic amplification" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alireza Bahadori, PhD
Senior Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I thank the authors for the careful and comprehensive revision of their manuscript.

Many of the scientific issues I and my two co-reviewers raised have been appropriately addressed. Also, by presenting the detailed arguments into discrete sections has made the paper much easier to read and the important messages comes across more clearly.

The paper would benefit from a few minor additions. Once this is done, I will be able to recommend publication of this insightful study:

Lines 42-44: A very relevant point here is made in connection with the impact of AA changes on jet stream perturbations. In this context it would be valuable to also cite the recent analysis of Luo et al., 2024: Arctic amplification-induced intensification of planetary wave modulational instability: A simplified theory of enhanced large-scale waviness. Quart. J. Roy. Met. Soc., 150, 2888- which presents a neat explanation for the increasing jet 'waviness'.
Setting the scene in these introductory comments it would also be beneficial to comment on how the nature of the AA impacts on cold winters over Asia. Reference, e.g., Wenqin Zhuo, Yao Yao, & co-authors (2023) 'The key atmospheric drivers linking regional Arctic amplification with East Asian cold extremes'. Atmospheric Res., 283, 106557.

Reviewer #2 (Remarks to the Author):

Second peer review of "The role of sea ice in present and future Arctic amplification" by E.-S. Chung et al.

The authors have done a good work with the revision taking the reviewers' points into account. It is an interesting manuscript, and I recommend publication after some minor points are considered.

Minor points:

L62: Comma after "latitudes".

L103: Add "indicating little AA" after the parenthesis.

L119-122: Move the years to the end of the sentence. Stability could better be represented by the potential temperature (in stead of temperature inversions), hence, potential temperature at 1000 hPa minus that at 850 hPa.

L158-161 regarding Fig. 2. I assume the fluxes associated with the lapse-rate and surface-albedo feedback as shown in Fig. 2 are derived based on the kernel method. Please indicate that here, as well as in the Fig. 2 caption.

Paragraph in L302: The poleward ocean heat transport have a significant effect on the the lapse-rate and the ice-albedo feedback (Eiselt and Graversen, J. Cim., 2023). CMIP models vary in the projection of this poleward heat transport, this could be discussed here.

L413-416: I assume the atmosphere-only experiments have fixed sea-surface temperatures and sea-ice SST/SI, please make that clear here. Are the CAM ensemble based on SST/SI from different CMIP models? Please clarify that here. If this is not the case, it should be noted that results of this ensemble are based on a single model, and that models differ in their representation of future Arctic changes as well as the lapse-rate and surface-albedo feedback (see e.g. Eiselt and Graversen, J. Clim., 2022).

L424-426: I appreciate the attempt here to estimate the none-sea-ice effects. But motivation for the formulae in this sentence is not so clear. Please provide the motivation in a follow up sentence, indicating that first term provides the SST contribution whereas the second and third term the CO_2 contribution. It is assumed here that these contributions are additive, which could be mentioned.

Paragraph starting at L430: Consider to move this paragraph up to after the sentence ending in L422.

Nice work!

Reviewer #3 (Remarks to the Author):

The authors have addressed all the points I raised in my previous review. I appreciate the additional SAT and ocean temperature analyses they did, which provided strong support for their arguments. evidence for the points they raised.
I believe the revised manuscript is in very good shape for publication.

Two minor points:
1) In the figure R1, they showed that whether using 20- or 30-year segment in their analysis, the AA sensitivity shows similar pattern. Why not using 30-year differences in Fig 4b, giving that WMO suggests using a 30- year period as a climatological mean?
2) Figure 2 captions is too long and hard to comprehend. It would be nice shorten it. For example, using “cold season” for October-to-February mean, (similar for warm season) and put a label of “cold season” and “warm season” inside the figure.

Response to Reviewer #1

This submission sets out to explore how the nature of 'Arctic amplification' may change as a result future GHG emissions. Their investigation is focussed on quantifying the influences of the numerous physical processes which can impact on the AA, and how these in themselves can change over time. The manuscript has the potential to make a significant contribution to the literature, but it is not quite there yet. Before I would be able to recommend acceptance, there are a number of issues which need to be addressed.

We are grateful to the reviewer for evaluating our manuscript and for the constructive comments and suggestions. We have revised the manuscript incorporating the comments and suggestions of the reviewer.

As a broad comment, the arguments presented here are, by their nature, very complex and some readers might have trouble grasping the full story. I strongly suggest breaking up the text into subsections where relevant. That way, the reader has a clear idea of what is to be tackled in the next part of the analysis.

Following the suggestion of the reviewer, we have increased the number of the subsections of Results from three to five, with the two additional subsections entitled 'Interconnectedness of feedback processes in the Arctic' and 'Temporal evolution of physical processes linked to Arctic amplification'. We have also attempted to clarify the text in the revised manuscript.

Lines 39-: In these introductory comments I was surprised that the authors did not refer to Polar Amplification Model Intercomparison Project. The basic paper for this refers to many of the aspects discussed here. Reference should be made to …

Doug M. Smith, James A. Screen, Clara Deser, Judah Cohen, John C. Fyfe, Javier Garcia-Serrano, Thomas Jung, Vladimir Kattsov, Daniela Matei, Rym Msadek, Yannick Peings, Michael Sigmond, Jinro Ukita, Jin-Ho Yoon and Xiangdong Zhang, 2019: The Polar Amplification Model Intercomparison Project (PAMIP) contribution to CMIP6: Investigating the causes and consequences of polar amplification. Geoscientific Model Development, 12, 1139-1164, doi: 10.5194/gmd-12-1139-2019.

We have cited Smith et al. (2019) in the revised manuscript. We thank the reviewer for letting us know this important study.

Line 41: Valuable to also reference the study of Screen, J.A. et al., 2018: Polar climate change as manifest in atmospheric circulation. Curr. Clim. Change Reps., 4, 383-395.

We have cited Screen et al. (2018) in the revised manuscript.

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David Docquier, François Massonnet, Francesco Ragone, Annelies Sticker, Thierry Fichefet and Stéphane Vannitsem, 2024: Drivers of summer Arctic sea-ice extent at interannual time scale in CMIP6 large ensembles revealed by information flow. Scientific Reports, 14, 24236, doi: 10.1038/s41598-024-76056-y.

They used the 'Liang-Kleeman information flow' method in conjunction with CMIP6 simulations (1970-2060) period to quantify the extent to which fluctuations in winter sea-ice volume, air temperature and ocean heat transport drive changes in subsequent summer Arctic ice coverage.

In the revised manuscript, we have cited Docquier et al. (2024) as follows:

"This mismatch in timing, along with the influence of winter air temperature on summer sea-ice extent [e.g., Docquier et al., 2024], might indicate that AA is driven by … ."

Lines 56: Important point made here on key role is played by the water vapor feedbacks. Beneficial here to also reference paper of Lee, S., T. Gong, S. B. Feldstein & co-authors (2017), Revisiting the cause of the 1989-2009 Arctic surface warming using the surface energy budget: Downward infrared radiation dominates the surface fluxes, Geophys. Res. Lett., 44, 10,654–10,661.

We have cited Lee et al. (2017) in the revised manuscript.

Lines 67-71: To put this in a broader context reanalyses perhaps mention that reanalyses show the Arctic lapse rate to have decreased significantly in all seasons except summer (Li et al., 2021: Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity. Ann. New York Acad. of Sciences, 1504, 167-186.

Following the suggestion of the reviewer, we have added the following text to the revised manuscript:
"Note that reanalyses show the Arctic lapse rate to have decreased significantly in all seasons except summer [e.g., Simmonds and Li, 2021]."

Simmonds, I. & Li, M. Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity. Ann. N.Y. Acad. Sci. 1504, 167−186 (2021).

Lines 149-151: Reword this sentence, as its overall meaning is not clear. I suspect a parenthesis may have been placed in the wrong location.

In the revised manuscript, we have clarified the text as follows:
"As shown in Fig. 2b, the increases in heat uptake over the months from April to September are followed by increases in heat release from October to February, although r^2 value is not high (~0.45); the increases in heat release, in turn, are accompanied by increases in TOA downward longwave radiative flux linked to lapse-rate feedback over the same season (Fig. 2c)."

Line 157: Delete the '~' here. The text within specifies precisely '-0.386'

Deleted.

Line 173-177: This insensitivity is not surprising, given that the sea ice boundary is traditionally taken at SIC = 15% (Simmonds et al. (2015) Comparing and contrasting the behaviour of Arctic and Antarctic sea ice over the 35-year period 1979-2013. Ann. Glaciol. 56(69): 18-28).

In response to the comment of the reviewer, we have added the following text in the revised manuscript:
"This insensitivity may not be surprising, given that the sea ice boundary is traditionally taken at SIC = 15% [Simmonds, 2015]."

Lines 249-254: Suggest saying 'not linear' instead of 'non-linear'.
A very interesting aspect of Figure 4a is the INCREASE in the AA ratio from 2031 to 2091. Fig. 4b indicates that that most months of the year contribute to this annual increase. This reversal is somewhat surprising, and is worth a few words on potential explanations.

In the revised manuscript, we have replaced 'non-linear' by 'not linear'.

Meanwhile, regarding the increase in the AA index from 2031 to 2091, we examined the temporal evolution of the monthly AA index and found a delayed occurrence of the AA peak over time (Fig. R1a). Considering that a similar increase in the AA ratio does not continue in the 21st century with marked weakening of January-to-March AA ratio (Fig. 4b), we suspect that the temporary increase in the AA ratio might be related to sea ice change via its ice-insulation effect during the cold season [e.g., Deser et al., 2010; Chung et al., 2021]. In agreement with this conjecture, pronounced March sea-ice decreases are found over 2091−2150 (Fig. R1e), whereas decreases are inconspicuous over 2031−2090 (Fig. R1b). Supplementary Figure 4c (Supplementary Fig. 6c in the revised manuscript) also indicates a sharp decrease in March Arctic SIE from the late 21st century to mid-22nd century. However, more in-depth analysis is needed to find the exact cause, which is the beyond the scope of the current study. Hence, we have just added the following text to the revised manuscript:
"Although the temporal evolution is not linear (e.g., temporary increase in the AA index from 2031 to 2091, which appears to be related, in part, to cold-season sea-ice decrease and resulting

heat release) and exhibit substantial … .”

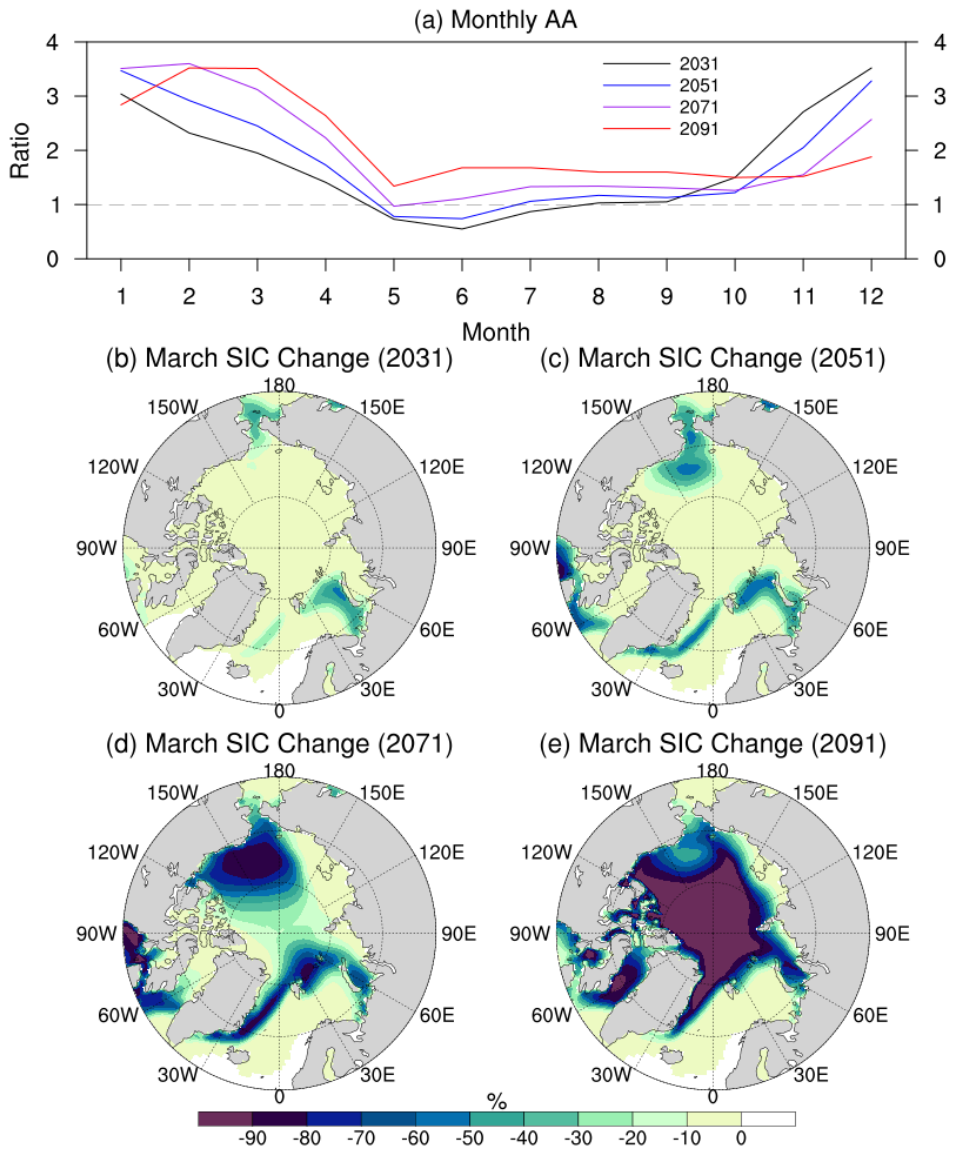


Figure R1: (a) Seasonal variation of the monthly AA index over four 60-year periods. The year in the legend denotes the first year of a given 60-year period. (b) March SIC change between 2031−2050 and 2071−2090. (c) Same as in (b), but for change between 2051−2070 and 2091−2110, (d) Same as in (b), but for change between 2071−2090 and 2111−2130. (e) Same as in (b), but for change between 2091−2110 and 2131−2150.

Lines 264-266: I like Figure 5 very much. It shows a lot of important information in a manner which is easy to digest.

We thank the reviewer for the positive comment.

Lines 300-307: The year-round warming below 200 m shown in Fig. S6b and c actually increases with depth to be a max at 400 m (the lowest level shown). This represents a marked decrease static stability in the layer. Some explanation of the origin of this feature should be presented.

It seems that this year-round warming in the Arctic Ocean mid-depth appears to be related to the influx of the warmed Atlantic water [e.g., Khosravi et al., 2022]. We have added the following text to the revised manuscript:

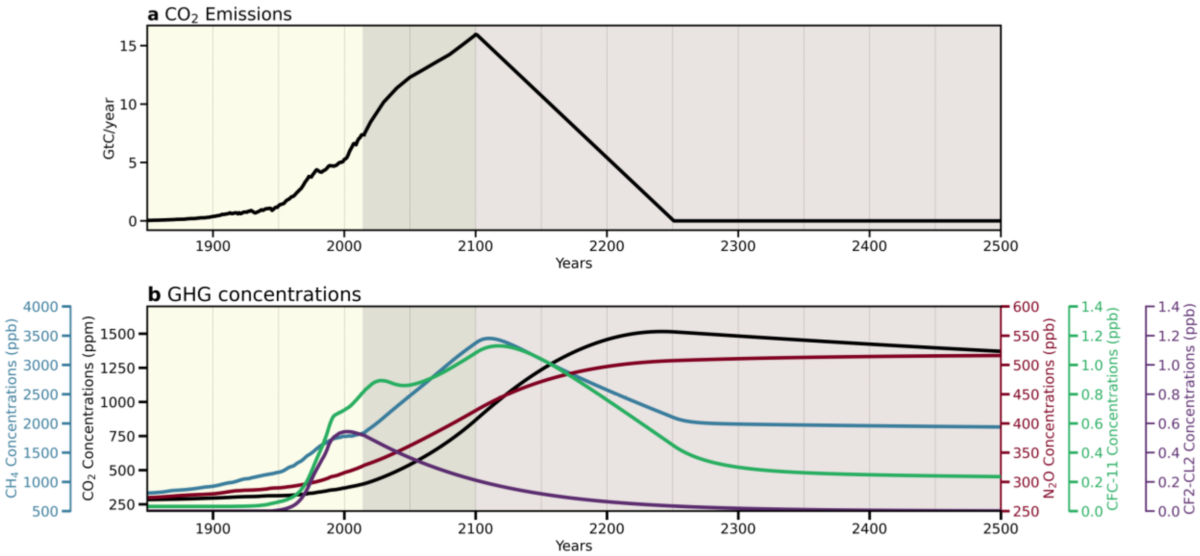
"The simulated year-round warming in the Arctic Ocean mid-depth appears to be related to the influx of the warmed Atlantic water [e.g., Khosravi et al., 2022]."

Khosravi, N., Wang, Q., Koldunov, N., Hinrichs, C., Semmler, T., Danilov, S. & Jung, T. The Arctic Ocean in CMIP6 models: Biases and projected changes in temperature and salinity. Earth's Future 10, e2021EF002282 (2022).

Lines 365-366: The Sun-Seon Lee et al. (2024) paper referred to here specifies these forcings as associated with '… the reduction of fossil and industrial CO2 emissions to zero by 2250' This is in contradiction with the stated text here ('… despite the prescribed decrease in CO2 concentration after 2250'). Please correct this. This is important in that there is actually no contradiction in the comment that 'the simulated global warming appears to persist'. Clarify the text here.

We thank the reviewer for pointing out this potential error. However, Figure 1b in Sun-Seon Lee et al. (2024) appears to indicate a slight decrease in CO_2 concentration over time after 2250 (please refer to black line in panel (b) below). Given multi-century to multi-millennia residence time of CO_2 in the atmosphere [Archer et al., 2009], the atmospheric CO_2 concentration (i.e., mole fraction) peaks around the year 2240 (at approximately 1515 ppm) and declines modestly thereafter even though fossil and industrial CO_2 emissions decrease after the year 2100 and are ramped down to zero by 2250 in the simulations.

Archer, D., Eby, M., Brovkin, V., Ridgwell, A., Cao, L., Mikolajewicz, U., Caldeira, K., Matsumoto, K., Munhoven, G., Montenegro, A. & Tokos, K. Atmospheric lifetime of fossil fuel carbon dioxide. Annual Review of Earth and Planetary Sciences 37, 117-134, 2009.



Line 428: Better to replace ‘More specifically’ with ‘For example’

Replaced.

Response to Reviewer #2

Peer review of "Potential disappearance of cold-season Arctic amplification in a future ice-free climate" by Chung et al.

The manuscript concerns the Arctic temperature amplification. Based on model experiments, it discusses development of this amplification into the future when sea ice in the Arctic is expected to disappear. By applying the kernel method, the role of ice-albedo and lapse-rate feedback is discussed. The manuscript present many interesting and creative approaches to explore the dependence of the lapse-rate feedback on that of sea ice. I am impressed of all the interesting information that is condensed in to this article format of limited length. I am in favour of this work, although I see one flawed analysis that should be corrected. Although in many places the text is well written, it suffers at other places of sentences difficult to follow, often due to very long sentence constructions. Hence I recommend major revisions.

We are grateful to the reviewer for evaluating our manuscript and for the constructive comments and suggestions. We have revised the manuscript incorporating the comments and suggestions of the reviewer.

Major points:

1. I am sceptical to the relevance of the Extra-Arctic SAT experiment with the atmosphere-only model, CAM6. It is highly unlikely that the possible atmospheric energy-transport change in this experiment is comparable to the actual change between 4xCO2 and pre-industrial worlds, as this latter energy-transport change comes as a response to the full 4xCO2 climate changes. If the idea is to explore the role of the energy-transport change, as appears to be the case from L199-200, ideally the full energy transport changes from coupled 4xCO2 minus 1xCO2 should be implemented as a forcing to the CAM6 model, which is a difficult task to do. Such an approach has been taken in a coupled experiment, see Graversen and Langen, J. Clim., 2019.

A quick fix is to remove these extra-arctic SST experiments and the associated text, as they cannot be used for the effect of the energy-transport change, and probably for nothing else.

However, as I understand the intension by the authors, the idea is to decompose the lapse-rate feedback into ice and non-ice related contributions. What then about the following:

The full lapse-rate feedback from both ice and non-ice source is given by the different of experiments constructed as 4xSAT+4xICE+4xCO2 minus 1xSAT+1xICE+1xCO2 (which the authors already have, red line in Fig. 3); the lapse-rate feedback from the non-ice source is given from 4xSAT+1xICE+4xCO2 minus1xSAT+1xICE+1xCO2, where the first of these has to be constructed. The difference between these two sets of experiments would give the ice-source contribution to the lapse-rate feedback.

Following the suggestion of the reviewer, we have crudely computed the lapse-rate feedback component from the non-ice source as (a4SST – piSST) + (piSST-4×CO2 – piSST)/2 + (a4SSTice-4×CO2 – a4SSTice)/2. Then, the ice-source contribution to the lapse-rate feedback is estimated by subtracting this non-ice source component from the full lapse-rate feedback arising from both the ice and non-ice sources (i.e., a4SSTice-4×CO2 – piSST). A revised version of Fig. 3 is presented below.

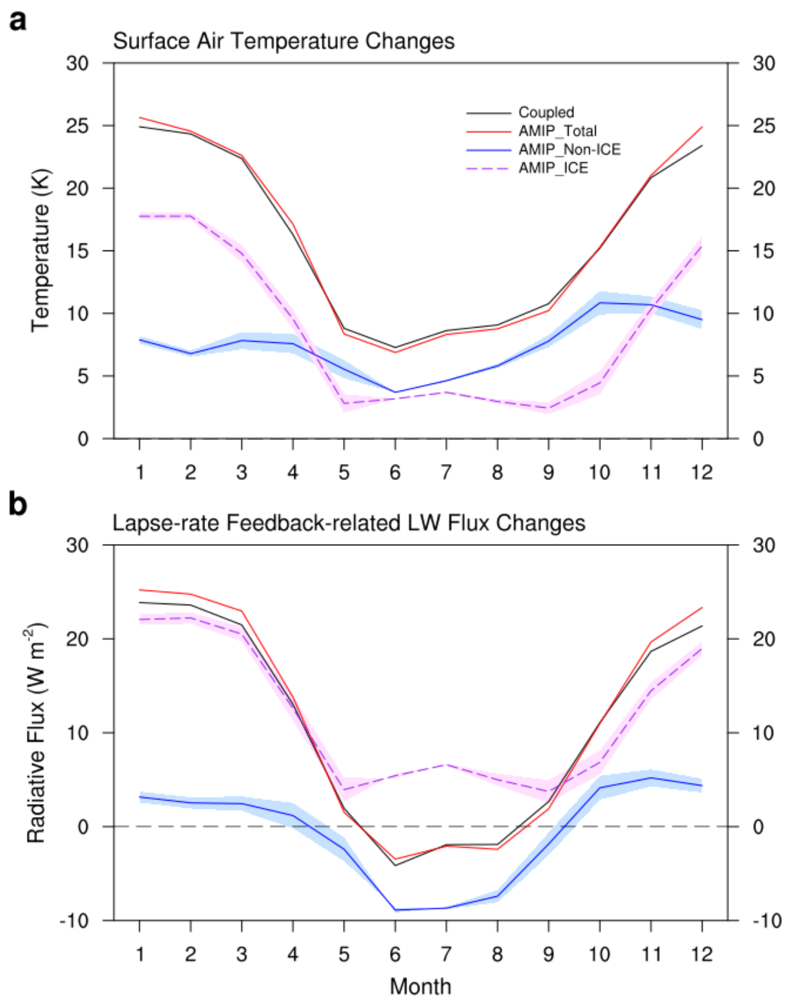


Figure 3: Decomposition of model-simulated Arctic-mean changes into the ice and non-ice source components. **a**, Monthly evolution of Arctic (70°N-90°N)-mean SAT changes in the CESM2 coupled model simulation forced by abrupt CO_2 quadrupling from a pre-industrial level (black line) and related atmosphere-only time-slice simulations with CAM6 (other lines). The solid line in red denotes the SAT changes arising from both the ice and non-ice sources in atmosphere-only model simulation in which the monthly varying SST and sea ice concentration changes were simultaneously prescribed along with a quadrupling of CO_2 concentration (AMIP_Total). The solid line in blue and dashed line in purple, respectively, represent the non-ice and ice source components, estimated from atmosphere-only model simulations, with shading denoting uncertainty resulting from differences in the boundary conditions between piSST and a4SSTice. **b**, Same as in **a**, but for lapse-rate feedback-related changes in downward longwave radiative flux at the top of the atmosphere.

2. Fig. 2: This is a very interesting investigation of the role of ice-albedo feedback during summer in the lapse-rate feedback during fall. Could the role of the lapse-rate feedback on the ice-albedo feedback be investigated in a similar analysis, but where autumn-winter fluxes are compared to the following spring-summer fluxes? This relates to the sentence L161-162.

In response to the comment of the reviewer, we have computed April-to-September-mean surface albedo feedback-related radiative flux changes and October-to-February-mean lapse-rate feedback-related radiative flux changes for two cases: (1) April 1951−March 1971 vs. April 1991−March 2011 (i.e., sunlit-season surface albedo feedback leads fall/winter lapse-rate feedback) and (2) October 1951−September 1971 vs. October 1991−September 2011 (i.e., fall/winter lapse-rate feedback leads sunlit-season surface albedo feedback). As shown in Fig. R1, April-to-September-mean surface albedo feedback and October-to-February-mean lapse-rate feedback appear to interact in both directions.

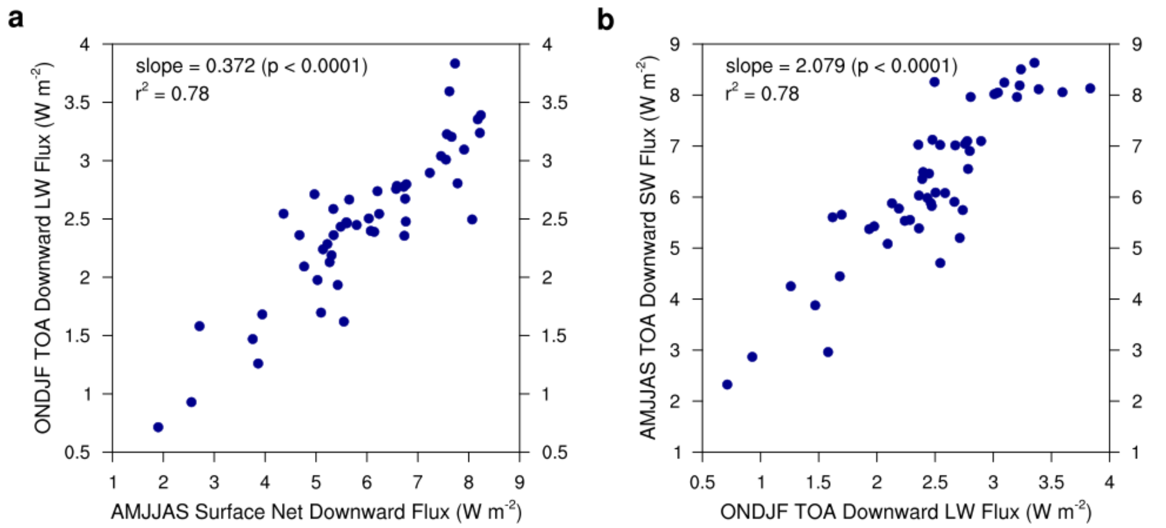


Figure R1: Linkage between sunlit-season surface-albedo feedback and cold-season lapse-rate feedback associated radiative fluxes over the Arctic in CESM2 LE. **a**, Inter-ensemble relationship of the Arctic-mean October-to-February-mean lapse-rate feedback-related TOA downward longwave radiative flux change between April 1951–March 1971 and April 1991–March 2011 with the corresponding April-to-September-mean TOA downward shortwave radiative flux change related to surface albedo feedback. Each dot represents an ensemble member of the CESM2 LE SMBB subset. The regression slope and r^2 values are presented in the upper-left corner. **b**, Same as in **a**, but for relationship between the April-to-September-mean surface-albedo feedback-related TOA downward shortwave radiative flux change between October 1951–September 1971 and October 1991–September 2011 and the corresponding October-to-February-mean lapse-rate feedback-related TOA downward longwave radiative flux change.

3. Some sentences are hard to comprehend, often since they are too long and can favourably be shorten. E.g the sentences starting in L182 and L399, but many others too. In fact although the authors impressively manage to include massive interesting information, I still see places where the text can be shorten; I recommend to look carefully over the text.

We thank the reviewer for pointing out this deficiency. In the revised manuscript, we have attempted to improve the clarity of the text. For instance, regarding the sentences starting in L182 and L399, we have modified the text as follows:

L182: The interconnection between sea-ice loss and cold-season lapse-rate feedback documented in some previous studies[3,41] and also presented in the previous subsection raises a question: can the magnitude and seasonality of AA in fully coupled model simulations be fully accounted for by local longwave feedback processes, such as lapse-rate feedback, and/or poleward energy transport from lower latitudes?

L399: As this possible interconnection has a profound implication for AA in an ice-free climate, a set of CMIP6 atmosphere-only time-slice experiments[54] (conducted with CAM6) is analyzed in conjunction with relevant coupled simulations forced by an abrupt quadrupling of CO_2 concentrations (conducted with CESM2). The CMIP6 Cloud Feedback Model Intercomparison Project[43] designed these atmosphere-only time-slice experiments to replicate the corresponding piControl and abrupt-4×CO2 simulations. We used the following 30-year atmosphere-only time-slice experiments to decompose the changes in top-of-atmosphere (TOA) longwave radiative flux associated with lapse-rate feedback in coupled model simulations into flux changes due to (1) non-ice source and (2) ice source: piSST, a4SST, piSST-4×CO2, a4SSTice, and a4SSTice-4×CO2. The total change in TOA longwave radiative flux resulting from both the non-ice and ice sources is computed by subtracting piSST from a4SSTice-4×CO2. Regarding the non-ice component, we used piSST, a4SST, piSST-4×CO2, and a4SST4ice×4CO2: (a4SST – piSST) + (piSST-4×CO2 – piSST)/2 + (a4SSTice-4×CO2 – a4SSTice)/2. Given differences in the boundary conditions between piSST and a4SSTice, the non-ice component is also estimated by adding (piSST-4×CO2 – piSST) or (a4SSTice-4×CO2 – a4SST) to (a4SST – piSST). Then, the ice-component is estimated by subtracting the non-ice component from the total change.

Minor points:

1. Perhaps a more accurate title is: "The role of sea ice in present and future Arctic temperature amplification."

We have changed the title following the suggestion of the reviewer:
"The role of sea ice in present and future Arctic amplification".

2. L31: "continued occurrence of AA" -> "AA continues over the 22 century".

We have changed the text as follows:
"although AA continues over the 22^{nd} century"

3. L42-44: The text needs references.

Post et al. (2013), Smith et al. (2019), and Smith et al. (2022) are cited in the revised manuscript.

Post, E. et al. Ecological consequences of sea-ice decline. Science 341, 519–524 (2013).
Smith, D. M. et al. The Polar Amplification Model Intercomparison Project (PAMIP) contribution to CMIP6: investigating the causes and consequences of polar amplification. Geosci. Model Dev. 12, 1139–1164 (2019).
Smith, D. M. et al. Robust but weak winter atmospheric circulation response to future Arctic sea ice loss. Nat. Commun. 13, 727 (2022).

4. L46: "in" after "than".

Added.

5. L49: “sea-ice-covered”, with hyphens.

Added.

6. Paragraph starting at L51: Although the role of the lapse-rate feedback and other processes are relevant, this discussion lacks a major point about seasonality and the ice-albedo feedback: Due to enhanced summer ice melt, energy is taken up by ocean during summer (causing little AA in this season) and released primarily during autumn and winter contributing to AA during these seasons. See e.g. Chung et al, Earth's Future, 2021. This aspect should be mentioned in this paragraph, in particular since this appears to be a relevant point latter in the manuscript.

In response to the comment of the reviewer, we have added the following text to the revised manuscript:

“The mismatch in seasonality between AA and surface albedo feedback, however, has been attributed in large part to sea ice-related seasonal energy transfer. Due to enhanced summer ice melt, energy is taken up by the Arctic Ocean during summer (causing little AA in this season) and released primarily during autumn and winter contributing to AA during these seasons[23,24,43].”

7. L59: “that in” after “to”.

Added.

8. L60-61: Comma after “Arctic”, “in the tropics/at lower latitudes” after “whereas”, remove “in lower latitudes” after “feedback”.

Corrected.

9. L65 and L71: “surface-albedo” with hyphen.

Added.

10. L73-76: Also Hwang et al. Geophys. Res Lett, 2011; Graversen and Langen, J Clim, 2019. The latter is relevant also for the next paragraph.

We thank the reviewer for letting us know these papers. Hwang et al. (2011) and Graversen and Langen (2019) have been cited in the revised manuscript.

Hwang, Y. T., Frierson, D. M. W. & Kay, J. E. Coupling between Arctic feedbacks and changes in poleward energy transport. Geophys. Res. Lett. 38, L17704 (2011).

Graversen, R. G. & Langen, P. L. On the role of the atmospheric energy transport in 2×CO_2-induced polar amplification in CESM1. J. Clim. 32, 3941−3956 (2019).

11. L82-83 “time-slice climate-model” with hyphens.

Added.

12. L116: “or” -> “and it”, “some” after “over”.

Added.

13. L127: “-vapour” after “water”.

Added.

14: L137: “.. stored in the Arctic Ocean during the sunlit season ..”, also note that energy is in the summer used for melting; with an increased annual cycle of sea ice this is another way to transfer energy from the summer to the winter season.

In response to the comment of the reviewer, we have revised the text as follows:
“Previous studies indicated that a mismatch in the monthly evolution between Arctic-mean surface-albedo feedback and corresponding surface air temperature change arises from

seasonal energy transfer. More specifically, a large fraction of increased downward shortwave radiative flux resulting from sea-ice loss is stored in the Arctic Ocean during the sunlit season (April to September) and then released to the atmosphere in subsequent fall and winter[23,24,43]. Given that while energy is used for melting in the sunlit season, refreezing of sea water involves ocean-to-atmosphere heat release in the cold season, the seasonal energy transfer also operates via sea ice changes."

15. L149: "the" -> "this", "is comparatively less tight" -> "show comparatively larger variability relative/less good fit to the regression slope".

In the revised manuscript, we have clarified the text as follows:
"As shown in Fig. 2b, the increases in heat uptake over the months from April to September are followed by increases in heat release from October to February, although r^2 value is not high (~0.45); the increases in heat release, in turn, are accompanied by increases in TOA downward longwave radiative flux linked to lapse-rate feedback over the same season (Fig. 2c)."

16. L152: "is in turn" -> ", in turn, is".

Corrected.

17. L153: "therefore" as first word in the sentence.

Corrected.

18. L159: Not so clear how the number is calculated, and in particular its uncertainty.

In the revised manuscript, we have provided more detailed explanation in the relevant Method section. Please also refer to our response to the reviewer's minor point #30 below.

19. L184-188: remove "however", "fully" after "be, remove "alone" and the rest of the sentence since this part seems not necessary as the sentence already started with this point.

Modified.

20. L198: remove “are able to”.

Deleted.

21. L241-242: “(i.e., a4SST minus piSST)”, I don't understand we can see the importance of sea-ice from this experiment, as the sentence seems to indicate.

In the revised manuscript, we have clarified the text as follows:
“The suggested interconnection between sea-ice loss and cold-season lapse-rate feedback, the latter of which is proposed as the main contributor to AA[28,39], however, implies that Arctic sea ice may be an important element of the current AA regime. We note that this hypothesis aligns with the suppressed seasonal contrast in the non-ice source-induced Arctic warming shown in Fig. 3a.”

22. L258: “hold up” -> “decrease”.

We have changed the text as follows:
“Although this seasonal contrast exists throughout the 21st century”

23. L280-281 “comparable to that in the current climate”, what is compared to the current climate?

We have clarified the text as follows:
“This enhanced poleward moisture transport in future ice-free climates contributes to warming in the Arctic; however, the cold-season AA is much weaker in comparison to that in the current climate (Fig. 4b).”

24. L352: “SSP3-7.0”, please indicate how strong this forcing scenario is compared to other common SSP scenarios.

In the revised manuscript, we have indicated that SSP3-7.0 is the strongest forcing scenario after SSP5-8.5.

25. L404: Unclear what “those” refers to. The entire sentence does not give much meaning to me.

The related text has been deleted in the revised manuscript.

26. L405-407: This sentence should go to the end of the next paragraph.

Corrected.

27. L408-410: This sentence should go to after the end of the sentence in L399, but without the Fig. SI7 reference. The following sentence should start immediately after the one ending in L405.

Corrected.

28. L412: Not clear what "parallel to" means in this context.

We have clarified the text as follows:
“In piSST, monthly and annually varying SSTs and sea ice along with atmospheric constituents are taken from a 30-year subsection of the corresponding piControl simulation, with the time period corresponding to the years 111–140 of abrupt-4×CO2 experiment.”

29. Mathematical expression at L441-442: These variables need to be defined. Note that only TOA longwave flux associated with the Planck feedback is defined above, not the feedback parameter itself.

We have modified the text as follows:
“A change in Arctic-mean surface air temperature (ΔTAS) resulting from the Arctic-mean

change in TOA radiative flux related to a given feedback process (ΔR) can be roughly estimated by dividing the Arctic-mean TOA radiative flux change by Arctic-mean Planck feedback (λ_{PL}) (i.e., $\Delta TAS \approx -\frac{\Delta R}{\lambda_{PL}}$)[23,32]."

30. Figure reference L444: This figure does only consider radiative fluxes, and not temperature contributions which is indicated in the sentence here.

In the revised manuscript, we have modified the text as follows:

"Using this approach, we crudely estimate to what extent the sunlit-season surface-albedo feedback may contribute to cold-season near-surface warming via lapse-rate feedback in Fig. 2d. First, the Arctic-mean October-to-February-mean TOA downward longwave flux change related to Arctic-mean April-to-September-mean TOA downward shortwave flux change is estimated for each ensemble member by multiplying April-to-September-mean shortwave flux change by the regression slope presented in the top left corner. Next, the resultant Arctic-mean October-to-February-mean surface air temperature change is roughly estimated by dividing the computed longwave flux change by $-\lambda_{PL}$. Then, the ratio of the estimated to the simulated Arctic-mean October-to-February-mean surface air temperature change is computed for each ensemble member. Finally, the ensemble mean and associated inter-ensemble standard deviation are computed for the ratio."

31. L458-459: This is not much of a statement concerning data availability.

This section is for code availability. As our manuscript does not use custom code or mathematical algorithm that is deemed central to the conclusions, we just mentioned that figures were generated using the NCAR Command Language.

32: L644: "arctic mean", over what area?

We have clarified the text as follows:

"Decomposition of mode-simulated Arctic (70°N-90°N)-mean changes"

33: L646: “simulations with CESM2”, atmosphere only simulations are strictly with CAM6 and not CESM2.

We have corrected the text as follows:
“coupled simulations with CESM2 (solid line in black) and atmosphere-only time-slice simulations with CAM6 (other lines)”

34. Experiments mentioned in L650-651: Please indicate in the Methods section how these experiment names are constructed based on the experiments mentioned there.

We have modified Fig. 3 and related description in the revised manuscript.

35. L666-667: “the magnitude of the ensemble mean is greater than two standard deviations across the ensemble members.” Always? Is it the magnitude minus one which is considered? (A value of one indicates no amplification.)

In response to the comment of the reviewer, we have modified the figure in the revised manuscript.

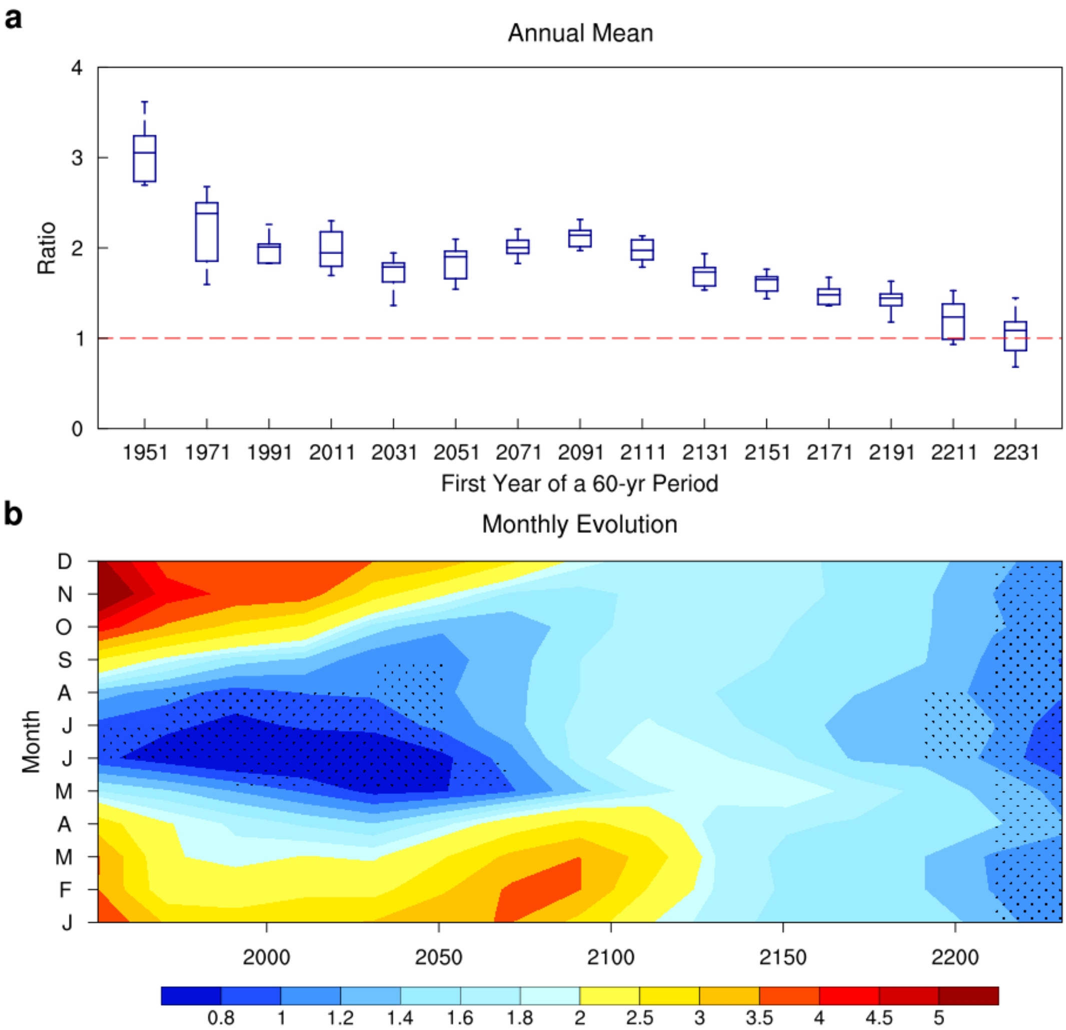


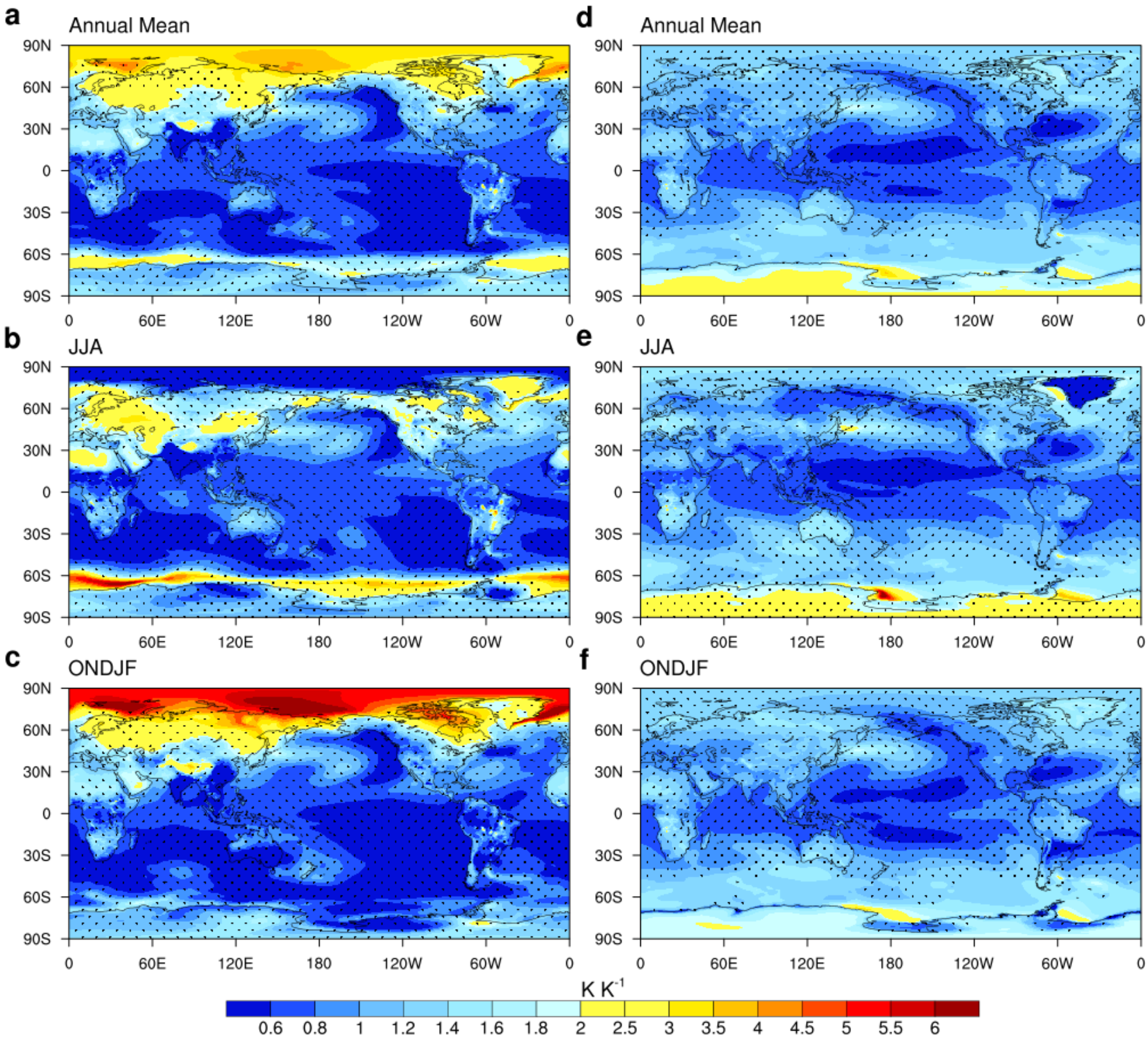
Fig. 4. Evolution of Arctic amplification in CESM2 LE. **a**, Ratio of the Arctic-mean annual-mean SAT change to global-mean annual-mean SAT change in the 10-member CESM2 LE extended simulations, with the x-axis representing the first year of various 60-year periods. The SAT changes are computed by subtracting the first 20-year average from the last 20-year average over a given 60-year period. The box covers the inter-quartile range with the line inside the box representing the median value across the ensemble members and whiskers denoting the maximum and minimum values. **b**, Ensemble-mean ratio of the corresponding Arctic-mean monthly-mean SAT change to global-mean monthly-mean SAT change. In **b**, the y-axis denotes month from January to December. Stippling denotes cases where the ensemble-mean ratio minus 1 is smaller than two standard deviations across the ensemble members. Regions with the ensemble-mean ratio smaller than 1 are also stippled. Note non-linear colour scales.

36. L681: “vapour” after “surface”.

Added.

37. L717-718: The same here as in point 24: is comparison to 1?

In response to the comment of the reviewer, we have modified the figure in the revised manuscript.



Supplementary Fig. 5. (Supplementary Fig. 7. in the revised manuscript) Dependence of Arctic amplification on the mean climate state in CESM2 LE. **a**, Ensemble-mean ratio of the annual-mean SAT change between 1951–1970 and 1991–2010 to the corresponding global-mean change in the 10-member CESM2 LE extended simulations. Stippling denotes regions where the ensemble-mean ratio minus 1 is smaller than two standard deviations across the ensemble members. Regions with the ensemble-mean ratio smaller than 1 are also stippled. **b**, Same as in **a**, but for June-to-August mean. **c**, Same as in **a**, but for October-to-February mean. **d-f**, Same as in **a-c**, but changes between 2211–2230 and 2251–2270. Note the non-linear colour scales.

38. Fig. SI7: What is the difference between "Sea ice" and "Polar sea ice"?

In the revised manuscript, we have replaced ‘Polar Sea Ice & SST’ by ‘Sea Ice & SST in the Polar regions’.

Response to Reviewer #3

This is an interesting study. The authors investigate changes in Arctic amplification (AA, 1991-2010 minus 1951-1970) and the attribution factors based on model simulations. The topic is important and worth to be further investigated. It is a thorough analysis; however, I do have several concerns regarding their discussion and results:

We are grateful to the reviewer for evaluating our manuscript and for the constructive comments and suggestions. We have revised the manuscript incorporating the comments and suggestions of the reviewer.

1) The concept of AA has been defined as the warming in the Arctic compared to the global mean. Keep this in mind, I suggest to revise the sentence in line 40 ("in comparison to lower latitude"), which is not accurate.

Following the suggestion of the reviewer, we have modified the text as follows:
"Observations indicate that near-surface warming resulting from increasing concentrations of greenhouse gases is greatly amplified in the Arctic in comparison to the global mean."

2) What is the reason for you to choose comparing the AA changes in the two 20-year period? 20-year average could be impacted greatly by the decadal variability, and so it is hard to extend current analysis to the future. The selection needs a justification, especially we are in 2025 already.

We thank the reviewer for pointing out this deficiency. In the submitted version of the manuscript, we quantified AA by computing the ratio of Arctic-mean to global-mean surface air temperature change between the first and last 20-year segments of a given 60-year period. To determine whether our conclusion is robust regardless of how to compute the AA index, we have additionally quantified AA for the following four cases:
(1) Change between the first and last 20-year segments of a given 70-year period
(2) Change between the first and last 20-year segments of a given 80-year period

(3) Change between the first and last 30-year segments of a given 80-year period

(4) Change between the first and last 30-year segments of a given 90-year period.

Figure R1 shows the development of the CESM2 LE ensemble-mean monthly AA into the future when sea ice in the Arctic is expected to disappear. Although the magnitude of AA varies depending on how the AA index is defined, all four cases are consistent with Fig. 4b in that Arctic amplification and its seasonality weaken greatly in a future ice-free climate.

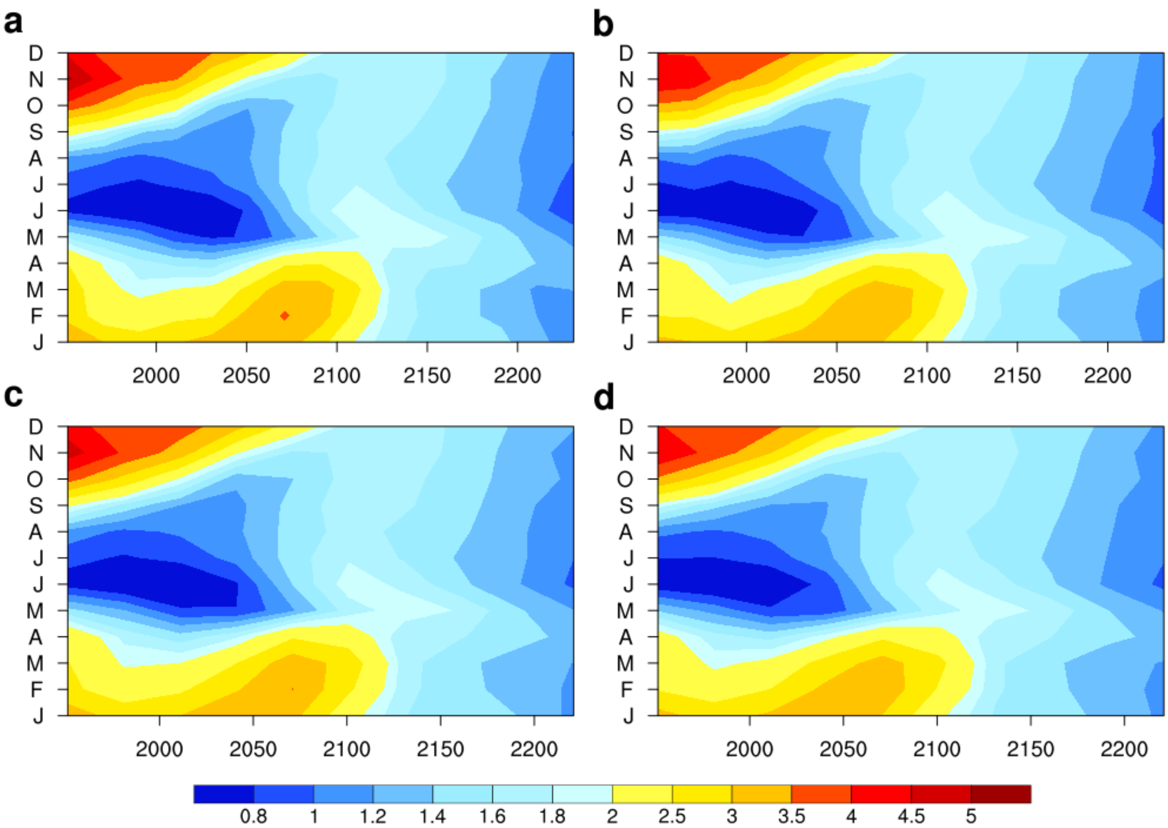


Figure R1: Assessment on the sensitivity of Arctic amplification (AA) to how the AA index is defined. **a**, Ensemble-mean ratio of the Arctic-mean to global-mean monthly-mean SAT change between the first and last two 20-year segments of a given 70-year period in the 10-member CESM2 LE extended simulations. **b**, Same as in **a**, but for change between two 20-year segments of a given 80-year period. **c**, Same as in **a**, but for change between two 30-year segments of a given 80-year period. **d**, Same as in **a**, but for change between two 30-year segments of a given 90-year period.

We have added the following text in the revised manuscript:

"Due to decadal climate variability, the temporal evolution of AA might vary when AA is quantified in a different way. As shown in Supplementary Fig. 9, however, qualitatively consistent temporal evolution is obtained regardless of how to determine the AA index."

3) Figure 1a shows the monthly AA index based on observations data sets and model simulations. The large differences between the observations (colored markers) in the cold season need to be addressed before moving forwards. What if you change the averaging period? Will you see similar pattern if you choose another period (say two 30-year average, or moving the time axis by 10 year) for comparison?

In response to the comment of the reviewer, we compared among the observational datasets the time series of annual-mean surface air temperature anomaly relative to the 1981−2010 climatology. As shown in Fig. R2, while global-mean anomalies are largely consistent between the datasets (Fig. R2a), pronounced discrepancies between ERA5 and the reconstructed datasets are found for Arctic-mean anomalies prior to the 1970s (Fig. R2b). Given this observational uncertainty prior to the 1970s, we also quantified the monthly AA index by computing the ratio of Arctic-mean to global-mean surface air temperature change between 1971−1990 and 2001−2020 (Fig. R2c). Unlike in ERA5, the magnitude of cold-season AA in the reconstructed datasets exhibits large differences between Fig. 1a (1951−1970 vs. 1991−2010) and Fig. R2c (1971−1990 vs. 2001−2020), implying potential uncertainties in Arctic-mean temperature anomalies prior to the 1970s.

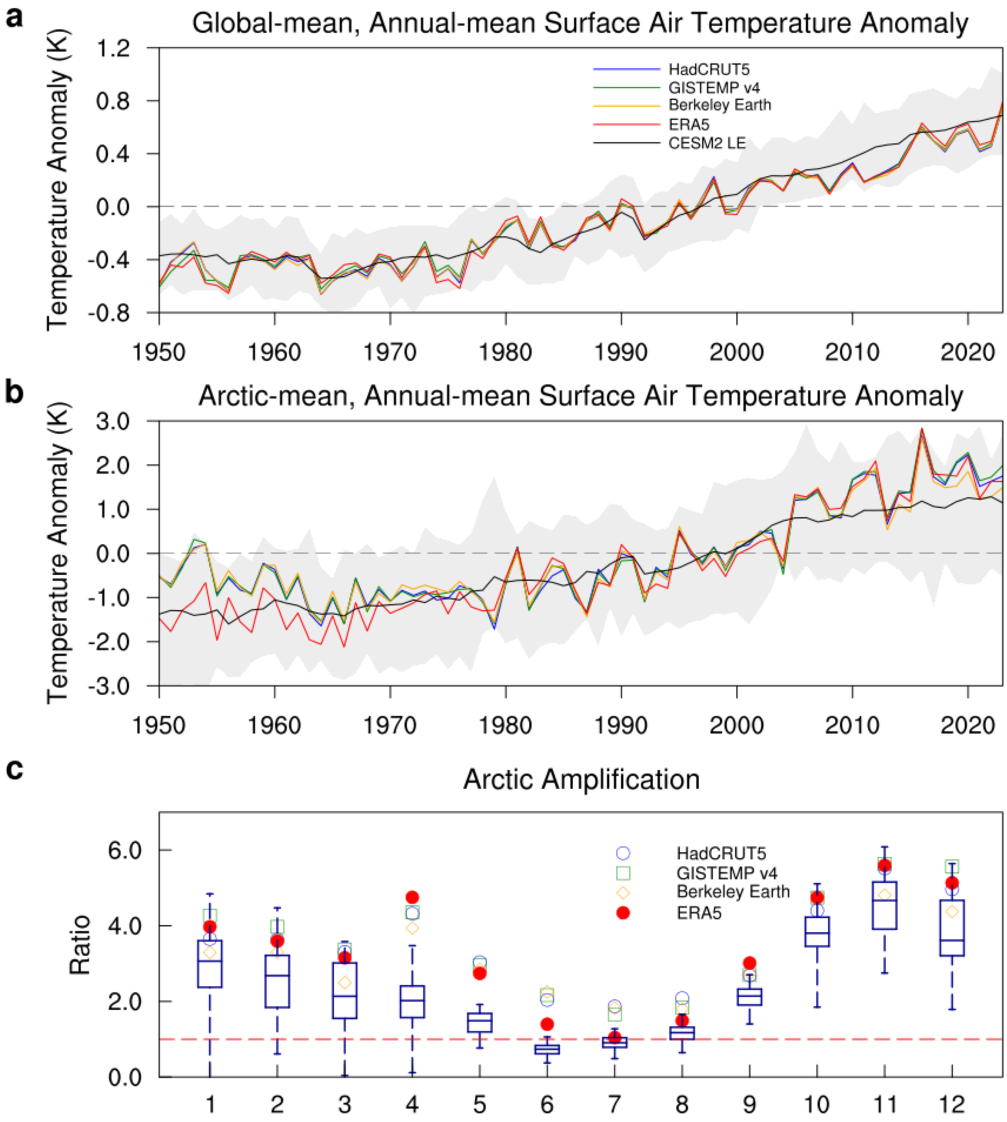


Figure R2: Observed and model-simulated Arctic amplification (AA). **a**, Time series of global-mean, annual-mean surface air temperature anomaly relative to the 1981–2010 climatology for HadCRUT5, GISTEMP v4, Berkeley Earth, and ERA5 (lines in colour) as well as for the CESM2 LE (black line and grey shading denote the ensemble mean and the minimum-to-maximum range, respectively). **b**, Same as in **a**, but for Arctic (70ºN-90ºN)-mean anomaly. **c**, Monthly evolution of AA for the observational datasets (symbols) and the CESM2 LE, defined as the ratio of Arctic-mean to global-mean monthly-mean surface air temperature change between 1971–1990 and 2001–2020. The box covers the inter-quartile range with the line inside the box representing the median value across the 50 ensemble members and whiskers denoting the maximum and minimum values.

4) Supplementary Figure 4 is something bothers me the most: CESM2 model simulations show sea ice extent in March is completely zero starting from about year 2200. Giving that the winter in the Arctic is completely dark and cold (no incoming solar radiation for several months), so sea ice will be formed. This figure (Supplementary Fig. 4c) shows that model has serious problem interpreting the physics of the Arctic system.

We thank the reviewer for pointing out this deficiency of CESM2. To examine whether the winter atmospheric and oceanic conditions are favorable for forming sea ice in a much warmer climate in CESM2, we computed the mean surface air temperature and upper 100-m ocean subsurface potential temperature over the period 2211–2230. Figure R3 shows that both future atmospheric and oceanic conditions in a much warmer climate do not appear to be favorable for forming sea ice in CESM2. However, given the possibility that CESM2 may have serious problem interpreting the physics of the Arctic system, further studies are needed to resolve this issue.

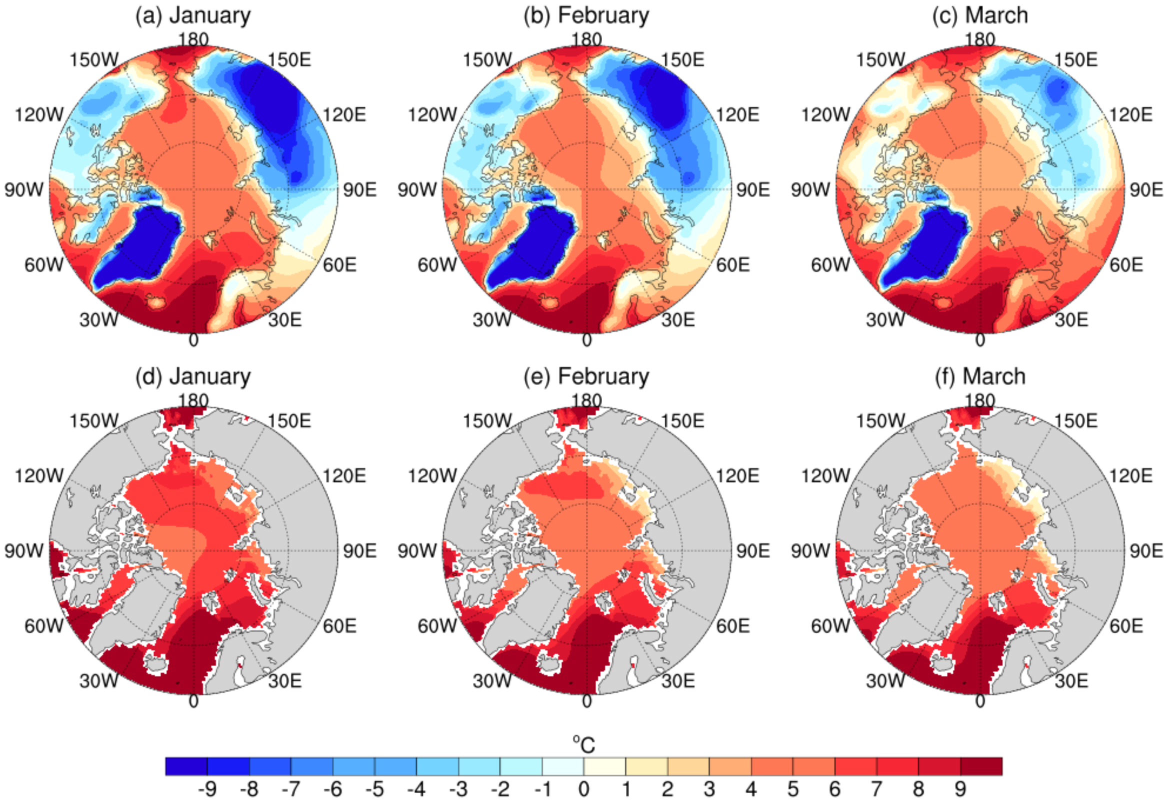


Figure R3: (a) Ensemble-mean January surface air temperature averaged over the period 2211–2230 in the CESM2 LE extended simulation. (b) Same as in (a), but for February. (c) Same as in (a), but for March. (d-f) Same as in (a)-(c), but for subsurface potential temperature averaged over the upper 100-m layer.

A few minor points:

1) Line 96-97: not clear to me what do you mean. Summer AA factor is close to 1 is not associated with albedo effect.

We have clarified the text as follows:

"Sea-ice loss-related surface-albedo feedback is expected to induce enhanced warming. However, the ratio is close to 1 during summer (June to August)."

2) Would you please specify clearly which months are included when you say "summer months", "cold season", "late fall" etc.

We apologize for the lack of clarity about our use of the cited terms. In the revised manuscript, we have attempted to specify months more clearly in the Introduction section.

3) Line 186: "longwave feedback" ? missing word here.

We have clarified the text as follows:
"… raises a question: can the magnitude and seasonality of AA in fully coupled model simulations be fully accounted for by local longwave feedback processes, such as lapse-rate feedback, and/or poleward energy transport from lower latitudes?"

4) Lines 230-231: what is "the former", and what is "the latter"? It's better to state them out clearly.

Following the suggestion of the reviewer, we have spelled them out:
"while the warming in the free troposphere is largely linked to the non-ice source, amplified near-surface warming in the cold season is distinctly related to sea-ice loss and associated SST warming in the Arctic"

Response to Reviewer #1

I thank the authors for the careful and comprehensive revision of their manuscript.

Many of the scientific issues I and my two co-reviewers raised have been appropriately addressed. Also, by presenting the detailed arguments into discrete sections has made the paper much easier to read and the important messages comes across more clearly.

The paper would benefit from a few minor additions. Once this is done, I will be able to recommend publication of this insightful study:

We are grateful to the reviewer for evaluating our manuscript once again and for the constructed comments. We have revised the manuscript to address the remaining concerns of the reviewer.

Lines 42-44: A very relevant point here is made in connection with the impact of AA changes on jet stream perturbations. In this context it would be valuable to also cite the recent analysis of Luo et al., 2024: Arctic amplification-induced intensification of planetary wave modulational instability: A simplified theory of enhanced large-scale waviness. Quart. J. Roy. Met. Soc., 150, 2888- which presents a neat explanation for the increasing jet 'waviness'.
Setting the scene in these introductory comments it would also be beneficial to comment on how the nature of the AA impacts on cold winters over Asia. Reference, e.g., Wenqin Zhuo, Yao Yao, & co-authors (2023) 'The key atmospheric drivers linking regional Arctic amplification with East Asian cold extremes'. Atmospheric Res., 283, 106557.

Following the suggestion of the reviewer, we have cited Luo et al. (2024) and Zhuo et al. (2023) in the revised manuscript, as follows:

"…, and perturbed jet streams due to a reduced meridional surface temperature gradient [e.g., Smith et al., 2019; Smith et al., 2022; **Luo et al., 2024**]. It is also an important issue how the nature of AA affects cold winters over East Asia [e.g., **Zhuo et al., 2023**]."

Luo, D., Luo, B., Zhang, W., Zhuo, W., Simmonds, I. & Yao, Y. Arctic amplification-induced intensification of planetary wave modulational instability: A simplified theory of enhanced large-scale waviness. *Q. J. R. Meteorol. Soc.* **150**, 2888–2905 (2024).

Zhuo, W., Yao, Y., Luo, D., Simmonds, I. & Huang, F. The key atmospheric drivers linking regional Arctic amplification with East Asian cold extremes. *Atmos. Res.* **283**, 106557 (2023).

Response to Reviewer #2

Second peer review of "The role of sea ice in present and future Arctic amplification" by E.-S. Chung et al.

The authors have done a good work with the revision taking the reviewers' points into account. It is an interesting manuscript, and I recommend publication after some minor points are considered.

We are grateful to the reviewer for evaluating our manuscript once again and for the constructed comments. We have revised the manuscript to address the remaining concerns of the reviewer.

Minor points:

L62: Comma after "latitudes".

Corrected.

L103: Add "indicating little AA" after the parenthesis.

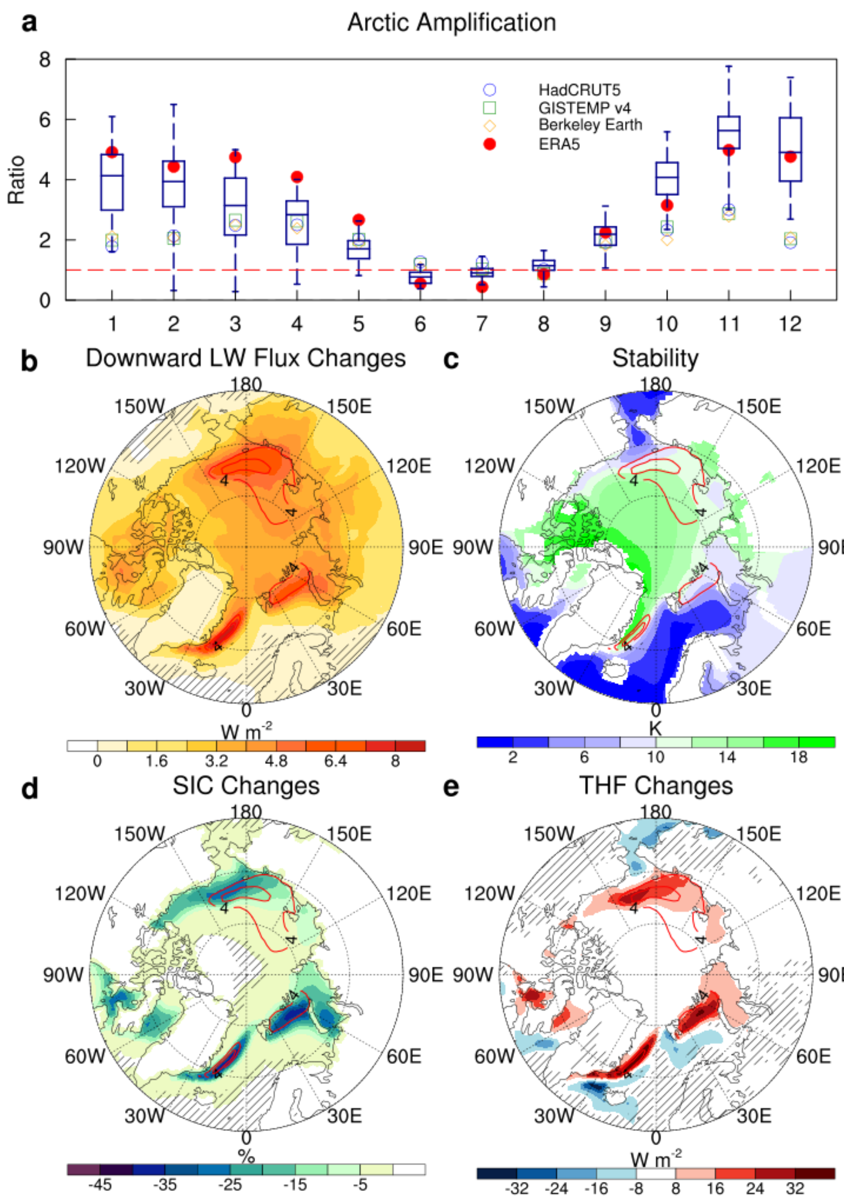
Added.

L119-122: Move the years to the end of the sentence. Stability could better be represented by the potential temperature (in stead of temperature inversions), hence, potential temperature at 1000 hPa minus that at 850 hPa.

Following the suggestion of the reviewer, we represented stability by the potential temperature difference (please refer to a revised version of Fig. 1c below). We have modified the text accordingly, as follows:

"To explore this possible causality, the strength of atmospheric stability is estimated by taking the difference between the potential temperature climatology at 850 hPa and 1000 hPa (i.e.,

$\Theta_{850} - \Theta_{1000}$). Figure 1c indeed indicates that a stable condition is prevalent in the Arctic lower troposphere; however, there is a pronounced mismatch in spatial pattern between the stability and changes in both surface air temperature and lapse-rate feedback-related TOA downward longwave radiative flux."



L158-161 regarding Fig. 2. I assume the fluxes associated with the lapse-rate and surface-albedo feedback as shown in Fig. 2 are derived based on the kernel method. Please indicate that here, as well as in the Fig. 2 caption.

Following the suggestion of the reviewer, we have added the following text to the revised manuscript.
"A radiative kernel method is used to estimate TOA radiative flux changes associated, respectively, with surface-albedo feedback and lapse-rate feedback."

Figure 2, caption: "Note that in **a**, **c** and **d**, TOA radiative flux changes are estimated using a radiative kernel method."

Paragraph in L302: The poleward ocean heat transport have a significant effect on the the lapse-rate and the ice-albedo feedback (Eiselt and Graversen, J. Cim., 2023). CMIP models vary in the projection of this poleward heat transport, this could be discussed here.

In response to the comment of the reviewer, we have modified the text in the revised manuscript, as follows:

"The poleward ocean heat transport has a significant effect on the lapse-rate and the ice-albedo feedback [e.g., **Eiselt and Graversen, 2023**]. Therefore, the projected changes in the strength and seasonality of AA over time might be linked more closely to changes in poleward oceanic heat transport. … Hence, it is unlikely that the projected changes in the strength and seasonality of AA can be attributed mainly to changes in poleward oceanic heat transport. Nonetheless, given large inter-model spread in the projection of poleward heat transport, further study is needed to confirm this conclusion."

Eiselt, K.-U. & Graversen, R. G. On the control of Northern Hemispheric feedbacks by AMOC: Evidence from CMIP and slab ocean modeling. *J. Clim.* **36**, 6777–6795 (2023).

L413-416: I assume the atmosphere-only experiments have fixed sea-surface temperatures and sea-ice SST/SI, please make that clear here. Are the CAM ensemble based on SST/SI from different CMIP models? Please clarify that here. If this is not the case, it should be noted that results of this ensemble are based on a single model, and that models differ in their representation of future Arctic changes as well as the lapse-rate and surface-albedo feedback (see e.g. Eiselt and Graversen, J. Clim., 2022).

In response to the comment of the reviewer, we have modified the text in the revised manuscript.

"As this possible interconnection has a profound implication for AA in an ice-free climate, a set of CMIP6 atmosphere-only time-slice experiments[56] (conducted with CAM6, with fixed SSTs and sea ice from CESM2) is analysed in conjunction with relevant coupled simulations forced by an abrupt quadrupling of CO_2 concentrations (conducted with CESM2). … We note that the results might vary depending on climate models due to inter-model discrepancy in future Arctic changes as well as the lapse-rate and surface-albedo feedbacks [e.g., **Eiselt and Graversen, 2022**]."

Eiselt, K.-U. & Graversen, R. G. Change in climate sensitivity and its dependence on the lapse-rate feedback in $4\times CO_2$ climate model experiments. *J. Clim.* **35**, 2919–2932 (2022).

L424-426: I appreciate the attempt here to estimate the none-sea-ice effects. But motivation for the formulae in this sentence is not so clear. Please provide the motivation in a follow up sentence, indicating that first term provides the SST contribution whereas the second and third term the CO2 contribution. It is assumed here that these contributions are additive, which could be mentioned.

Following the suggestion of the reviewer, we have added the following text to the revised manuscript.

"Regarding the non-ice component, assuming additivity, we used piSST, a4SST, piSST-4×CO2, a4SSTice, and a4SSTice-4×CO2: (a4SST – piSST) + (piSST-4×CO2 – piSST)/2 + (a4SSTice-4×CO2 – a4SSTice)/2, in which the first term represents the extra-Arctic SST contribution whereas the second and third term the CO_2 contribution."

Paragraph starting at L430: Consider to move this paragraph up to after the sentence ending in L422.

Moved.

Nice work!

Once again, we deeply appreciate your valuable comments and suggestions.

Response to Reviewer #3

The authors have addressed all the points I raised in my previous review. I appreciate the additional SAT and ocean temperature analyses they did, which provided strong support for their arguments. evidence for the points they raised.
I believe the revised manuscript is in very good shape for publication.

We are grateful to the reviewer for evaluating our manuscript once again and for the constructed comments. We have revised the manuscript to address the remaining concerns of the reviewer.

Two minor points:
1) In the figure R1, they showed that whether using 20- or 30-year segment in their analysis, the AA sensitivity shows similar pattern. Why not using 30-year differences in Fig 4b, giving that WMO suggests using a 30- year period as a climatological mean?

In response to the comment of the reviewer, we have modified the text in the revised manuscript, as follows:

"The ratio of the Arctic-mean to global-mean surface air temperature change between the first and last 20-year segments of given multiple 60-year periods is computed for each ensemble member over the period 1951–2290 (Supplementary Fig. 6 indicates that qualitatively similar results are obtained regardless of how to quantify the AA index)."

2) Figure 2 captions is too long and hard to comprehend. It would be nice shorten it. For example, using "cold season" for October-to-February mean, (similar for warm season) and put a label of "cold season" and "warm season" inside the figure.

Following the suggestion of the reviewer, we have modified Fig. 2 and related cation in the revised manuscript.

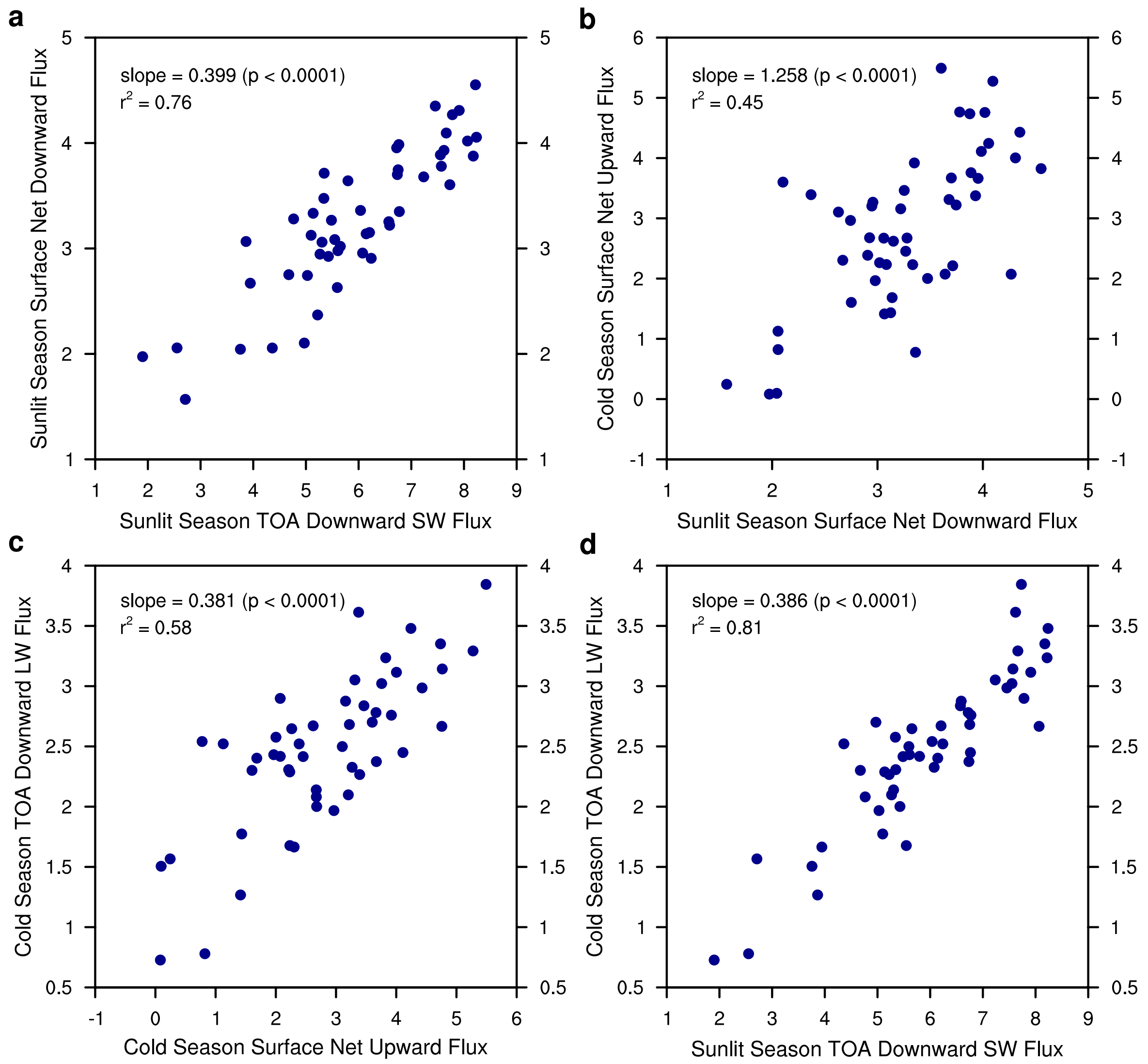


Fig. 2. Linkage between sunlit-season surface-albedo feedback and cold-season lapse-rate feedback associated heat fluxes over the Arctic in CESM2 LE. **a**, Inter-ensemble relationship of the Arctic-mean sunlit season (April-to-September)-mean net downward flux change at the surface between 1951–1970 and 1991–2010 with the corresponding TOA downward shortwave radiative flux change related to surface albedo feedback. Each dot represents an ensemble member of the CESM2 LE SMBB subset. The regression slope and r^2 values are presented in the upper-left corner. **b**, Same as in **a**, but for relationship between cold season (October-to-February)-mean net upward flux change at the surface and sunlit season-mean net downward flux change at the surface. **c**, Same as in **a**, but for relationship between cold season-mean lapse-rate feedback-related TOA downward longwave radiative flux change and cold season-

mean net upward flux change at the surface. **d**, Same as in **a**, but for relationship between cold season-mean lapse-rate feedback-related TOA downward longwave radiative flux change and sunlit season-mean surface albedo feedback-related TOA downward shortwave radiative flux change. In **a**-**d**, units are in W m^{-2}. Note that in **a**, **c** and **d**, TOA radiative flux changes are estimated using a radiative kernel method.