

Climate change-induced amplification of extreme temperatures in large lakes

Corresponding Author: Dr Hazem Abdelhady

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 Abdelhady,

Your manuscript titled "Climate Change-Induced Amplification of Extreme Temperatures in Large Lakes" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.
Please also consider the editorial thresholds below:

*Provide more detailed explanation for methodology.

*Provide a more thorough discussion for the main conclusion (e.g., physical mechanisms for connection between LST extremes and teleconnections).

*Provide a more thorough analysis and validation of model outputs to enhance the robustness of arguments.

Please submit your point-by-point 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.

We are committed to providing a fair and constructive peer-review process. Please don't 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 referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still 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.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Kyung-Sook Yun, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-9990-3581

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript titled "Climate Change-Induced Amplification of Extreme Temperatures in Large Lakes" appears to be a well-structured and robust study. The authors have made substantial efforts to address critical aspects of lake surface temperature (LST) variability, applying advanced modeling techniques and detailed analyses to understand the impact of climate change on large freshwater systems.

Key strengths of this work include:

- The use of a hydrodynamic-ice model driven by ERA5 climate data is a novel and rigorous approach, providing a long-term (82 years) dataset to analyze LST extremes in the Great Lakes.

- The authors have thoroughly examined LST extremes through metrics like the 10th and 90th percentiles, heatwaves, and cold-spells. Their use of nonlinear detrending enhances the clarity and reliability of results.

- The identification of links between LST extremes and climate teleconnections, such as the Arctic Oscillation and Pacific Decadal Oscillation, is a notable contribution. These results underline the cascading effects of climate variability on large lake ecosystems.

- The discussion highlights the ecological, climatic, and socio-economic implications of the findings, which are critical for policymakers and environmental managers.

Overall, I find this work a useful addition to previous research on this topic and on the study of extremes events in lakes.

I support the publication of this manuscript, as it provides a substantial contribution to the field of climate science and freshwater ecology. It addresses a pressing issue with global relevance, and the methodology, results, and discussion are clearly presented and scientifically sound. The data from the model are not yet made available, but I trust that authors will organise this prior to publication.

Reviewer #2 (Remarks to the Author):

This manuscript offers an in-depth investigation into the amplification of lake surface temperature (LST) extremes across the Laurentian Great Lakes over the past 80 years, using a hydrodynamic-ice coupled model driven by ERA5 climate data. The study provides critical insights into the temporal evolution of LST extremes, their linkages to interannual and decadal climate teleconnections, and their broader implications for regional climate and ecological systems.

The authors employ a robust methodology to separate long-term warming trends from internal variability using nonlinear detrending techniques. This approach facilitates a detailed analysis of heatwaves and cold-spells, revealing over a 100% increase in their intensities since the 1990s. The study also identifies critical breakpoints and highlights strong periodicities in LST extremes associated with major climate teleconnection indices, including the Arctic Oscillation (AO), Southern Oscillation Index (SOI), and Pacific Decadal Oscillation (PDO).

While the manuscript addresses an important and timely topic, several aspects would benefit from clarification and expansion:

The manuscript does not clearly explain how LST values were calculated during ice-covered periods, particularly when lakes are partially frozen. Since common approaches either assign LST as 0°C or exclude such periods, this could significantly affect the analysis of winter and spring LST patterns. A detailed explanation of how ice-covered conditions were handled and the potential implications for the results is necessary.

Although the authors provide a general overview of LST trends across the five Great Lakes, further elaboration on the spatial differences among the lakes and their underlying causes would add depth to the analysis.

The manuscript does not include sufficient validation of the model outputs against observational datasets, such as in situ measurements or satellite-derived LST data. Adding a section to compare the modeled results with observational data would enhance the study's robustness and credibility.

The methods section would benefit from a more explicit discussion of the assumptions underlying the hydrodynamic model, as well as potential uncertainties these assumptions might introduce into the findings. Similarly, the discussion section could include an evaluation of how these uncertainties might influence the interpretation of results.

The seasonal variability of LST trends is highlighted, but the causes of the observed differences between months are not adequately explored. A discussion of why certain months show greater variability and extremes would enhance the interpretative value of the results.

The conclusion could be expanded to discuss the practical implications of the study, including its relevance for ecosystem management, climate adaptation strategies, and water resource planning. This would underscore the broader significance of the findings.

The observed coherence between LST extremes and teleconnection patterns (e.g., AO, SOI, PDO) is an intriguing result. The manuscript would benefit from a deeper exploration of the physical mechanisms driving these relationships.

In summary, the manuscript presents a thorough and innovative investigation of LST extremes and their evolution under climate change. Addressing the above points would significantly strengthen the manuscript and ensure its impact on the scientific community and stakeholders focused on climate adaptation and ecological management.

Reviewer #3 (Remarks to the Author):

In this manuscript, Abdelhady et al. analyze extreme lake surface water temperature (LSWT) changes over the past 80 years in the Laurentian Great Lakes using a hydrodynamic-ice model driven by ERA5 climate data. The study examines temperature extremes by analyzing changes in the 10th and 90th percentiles, as well as the occurrence, duration, and intensity of heatwaves and cold spells. The results are interpreted in relation to teleconnection indices.

The manuscript is interesting and well-written. However, there are some aspects that require clarification before it can be considered for publication.

Major Comments:

- The authors use a 3D model calibrated only with LSWT data. Moreover, they rely solely on lake-averaged values for model validation, thus not fully leveraging the advantages of 3D modeling. From a methodological perspective, this is an oversimplification, as a 3D model requires thorough validation. Given this approach, a 1D model might be preferable for two reasons: (1) using a 3D model while performing spatial averaging does not take full advantage of the benefits of 3D modeling, and (2) if a 3D model is used, it must be properly validated. The authors should clarify the advantages of using a 3D model over a 1D model if only mean values are considered, and they should provide model validation details (e.g., at least comparing simulated vs observed vertical temperature profiles). In this regard, lines 83-84 do not clearly specify whether daily or temporally aggregated data are used to calculate RMSE.

- The methodology is often unclear, particularly regarding the temporal resolution considered at different processing steps. For instance, line 333 mentions the "entire time series," but its meaning is unclear. The same issue arises on line 363. The spectral analysis is only briefly described. For example, it is unclear what is meant by "red noise spectrum based on a lag-one autocorrelation coefficient ($\alpha=0.5$)."

More details should also be provided regarding "the 95% confidence levels from the red noise spectrum using the F-statistic, with 2 degrees of freedom." Similarly, "performed spectral coherence analysis" lacks sufficient explanation. These details should be explicitly included in the Methods section rather than inferred from reading the Results. Additionally, Figure 8 lacks axis labels, and the interpretation of the red noise fitting and 95% confidence interval are unclear.

- The analysis could be more detailed and sometimes remains at a superficial level. For instance, the double peak in Lake Superior in May and June is worth discussing: could it be related to early stratification dynamics? While I agree that lakes have lower thermal inertia than the ocean (L235-239), in Lake Superior, thermal inertia plays a crucial role (see <https://doi.org/10.1002/2014WR016555>, <https://doi.org/10.1002/Ino.10331>, <https://www.nature.com/articles/s41598-017-04058-0>). The effect of stratification should be investigated.

- L118-119: "All lakes showed a significant increase in annual low temperature extremes (10th percentile)." Do the authors mean that the temperature associated with the 10th percentile increased or decreased? Looking at the cdfs in the figures, I would assume the latter, but in the text the authors use the term "increase", which may be misleading.

- L219: The Atlantic Multidecadal Oscillation (AMO) has not been investigated. I assume the authors are referring to the Arctic Oscillation (AO).

- I think that some literature on Great Lakes thermal/climate patterns is missing. The authors may consider adding the following studies: <https://www.mdpi.com/2072-4292/15/17/4253>, <https://doi.org/10.1080/20442041.2021.1873698>, <https://doi.org/10.3389/frwa.2021.782265>, <https://doi.org/10.1016/j.jhydrol.2022.128655>, <https://doi.org/10.1007/s00382-018-4481-z>, <https://doi.org/10.1023/A:1022140604052>, <https://iopscience.iop.org/article/10.1088/1748-9326/ad8ee0/meta>

Minor Comments

In the Abstract, the term "vital" could be replaced with a better term. Additionally, the authors refer to "tipping points" but this concept is not further investigated in the manuscript.

L38: The cited works likely include the Great Lakes.

L323: "The" → "the".

L331-332: Use "LST" instead of "STL".

L340: "ΔLST" should be introduced at L332.

L391: Double "the".

L396: "The" → "the".

Figure 1: The lettering of subplots is missing. In the caption, the use of parentheses is inconsistent, as is capitalization.

Figure 1: The first plot should be commented and referenced in the text.

Typically, Lake Surface Water Temperature (LSWT) is used instead of Lake Surface Temperature (LST) to avoid confusion with Land Surface Temperature (also abbreviated as LST). The authors may consider adopting LSWT for clarity and consistency.

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Version 1:

Decision Letter:

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Dear Dr Abdelhady,

Your revised manuscript titled "Climate Change-Induced Amplification of Extreme Temperatures in Large Lakes" 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 reviewer 3. 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,

Kyung-Sook Yun, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-9990-3581

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

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have addressed my concerns very well and the manuscript was improved so much. Thus, it can be accepted for publishing in CEE.

Reviewer #3 (Remarks to the Author):

The authors have responded positively to the previous comments. Overall, I appreciate the authors' revisions, but I still have some concerns that need clarification:

L 37-40: Referring back to comment R3-8 in the previous review round, I still do not agree that the Great Lakes are excluded or included with special validation in global studies (e.g., references 3 and 23). In many cases, they are used for validation due to the availability of long and good quality data records.

L 122: Instead of "normalized," I suggest using "relative to the reference period 1941-1970" for consistency, as the manuscript refers to "relative" and does not use "normalized" elsewhere. Additionally, the authors' response in the rebuttal letter ("Since the 10th percentile DLST values are negative, normalization results in a positive trend") clarified that an increasing relative 10th percentile indicates colder low temperatures. Explicitly stating this in the manuscript would improve clarity.

L 221-225: This section remains somewhat unclear. Why has AMO been considered separately from NAO, AO, SOI, PDO, and ONI? AMO is not mentioned in the methods section, and no figures are provided to support its discussion. Finally, the statement that "the Atlantic Multidecadal Oscillation (AMO) was not explicitly used to interpret the periodicity of heatwaves and cold spells, since the long-term nonlinear trend was already removed" needs further clarification: why is this an issue for AMO but not for the other teleconnection indices?

R3-1: A brief comment in the manuscript explaining why a 3D model was chosen over a 1D model would be beneficial, as many readers may wonder about this decision. The authors provided a clear explanation in their response in the rebuttal letter, and including it in the text would strengthen the justification for using a 3D model.

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Response to Reviewers' Comments

We thank the reviewers for their valuable comments and suggestions. We have revised the manuscript to address all comments and implement suggestions. Please, see below for our detailed response to comments and explanation of manuscript edits. The reviewer's comments are in black, italic font, with our response in regular blue font. All line numbers refer to the clean revised manuscript unless stated otherwise.

Reviewer #1

The manuscript titled "Climate Change-Induced Amplification of Extreme Temperatures in Large Lakes" appears to be a well-structured and robust study. The authors have made substantial efforts to address critical aspects of lake surface temperature (LST) variability, applying advanced modeling techniques and detailed analyses to understand the impact of climate change on large freshwater systems.

Key strengths of this work include:

-The use of a hydrodynamic-ice model driven by ERA5 climate data is a novel and rigorous approach, providing a long-term (82 years) dataset to analyze LST extremes in the Great Lakes.

-The authors have thoroughly examined LST extremes through metrics like the 10th and 90th percentiles, heatwaves, and cold-spells. Their use of nonlinear detrending enhances the clarity and reliability of results.

-The identification of links between LST extremes and climate teleconnections, such as the Arctic Oscillation and Pacific Decadal Oscillation, is a notable contribution. These results underline the cascading effects of climate variability on large lake ecosystems.

-The discussion highlights the ecological, climatic, and socio-economic implications of the findings, which are critical for policymakers and environmental managers.

Overall, I find this work a useful addition to previous research on this topic and on the study of extremes events in lakes.

I support the publication of this manuscript, as it provides a substantial contribution to the field of climate science and freshwater ecology. It addresses a pressing issue with global relevance, and the methodology, results, and discussion are clearly presented and scientifically sound. The data from the model are not yet made available, but I trust that authors will organise this prior to publication.

Thank you for your positive comments. Yes, the lake surface temperature data will be made available prior to final publication.

Reviewer #2

This manuscript offers an in-depth investigation into the amplification of lake surface temperature (LST) extremes across the Laurentian Great Lakes over the past 80 years, using a hydrodynamic-ice coupled model driven by ERA5 climate data. The study provides critical insights into the temporal evolution of LST extremes, their linkages to interannual and decadal climate teleconnections, and their broader implications for regional climate and ecological systems.

The authors employ a robust methodology to separate long-term warming trends from internal variability using nonlinear detrending techniques. This approach facilitates a detailed analysis of heatwaves and cold-spells, revealing over a 100% increase in their intensities since the 1990s. The study also identifies critical breakpoints and highlights strong periodicities in LST extremes associated with major climate teleconnection indices, including the Arctic Oscillation (AO), Southern Oscillation Index (SOI), and Pacific Decadal Oscillation (PDO).

Thank you!

While the manuscript addresses an important and timely topic, several aspects would benefit from clarification and expansion:

R2-1 The manuscript does not clearly explain how LST values were calculated during ice-covered periods, particularly when lakes are partially frozen. Since common approaches either assign LST as 0°C or exclude such periods, this could significantly affect the analysis of winter and spring LST patterns. A detailed explanation of how ice-covered conditions were handled and the potential implications for the results is necessary.

The FVCOM-CICE model directly accounts for ice cover when calculating LST for each grid point in the lakes. In general, LST is close to 0°C when ice is present, and the model explicitly represents this by assigning near-zero values in fully ice-covered regions. We used these modeled values directly in our calculations of LST extremes. We have clarified this methodology in the revised manuscript (lines 347–349).

The impact of this approach is evident in Supplementary Figure 4, particularly for months 2 and 3 in Lake Superior. During the first ~40 years of the dataset, the distribution of DLST was centered around zero, reflecting persistent ice cover. Over time, as ice cover declined, higher temperatures became more frequent, leading to changes in the DLST distribution.

R2-2 Although the authors provide a general overview of LST trends across the five Great Lakes, further elaboration on the spatial differences among the lakes and their underlying causes would add depth to the analysis.

One of the primary drivers of spatial variability in LST extremes is the reduction in ice cover, which differs across the lakes due to variations in their physical and climatic conditions. We have expanded the discussion to emphasize these spatial differences and their implications. Please refer to the revised manuscript (lines 262–270).

R2-3 The manuscript does not include sufficient validation of the model outputs against observational datasets, such as in situ measurements or satellite-derived LST data. Adding a section to compare the modeled results with observational data would enhance the study's robustness and credibility.

The model used in this study has been extensively validated in previous research. To reinforce its credibility, we have added a paragraph in the Methods section (lines 342–359) referencing these validation studies and summarizing the validation results.

R2-4 The methods section would benefit from a more explicit discussion of the assumptions underlying the hydrodynamic model, as well as potential uncertainties these assumptions might introduce into the findings. Similarly, the discussion section could include an evaluation of how these uncertainties might influence the interpretation of results.

The hydrodynamic model used in this study has undergone extensive validation and testing, as documented in previous studies. These studies provide a comprehensive assessment of the model's limitations and biases. We have added a paragraph in the Methods section (lines 342–359) referencing these works.

Generally, model errors and biases in LST were found to be relatively small and consistent across the lakes, RMSE ranged from 0.89 to 1.58°C, with a mean bias between -0.51 and 0.32°C. Additionally, by detrending LST before calculating extremes, we mitigate systematic biases, reducing their potential impact on our findings. Therefore, we believe that these uncertainties have a minimal influence on the interpretation of our results.

R2-5 The seasonal variability of LST trends is highlighted, but the causes of the observed differences between months are not adequately explored. A discussion of why certain months show greater variability and extremes would enhance the interpretative value of the results.

Seasonal changes in LST trends are largely driven by variations in ice cover, which influence both the magnitude and frequency of extremes. This was discussed in the original manuscript, but we have now expanded the discussion to better highlight the spatiotemporal variability of trends and their underlying causes. Please refer to the revised manuscript (lines 252-270).

R2-6 The conclusion could be expanded to discuss the practical implications of the study, including its relevance for ecosystem management, climate adaptation strategies, and water resource planning. This would underscore the broader significance of the findings.

We have revised the conclusion to better emphasize the broader implications of our findings. Please refer to the revised manuscript (lines 303–314).

R2-7 The observed coherence between LST extremes and teleconnection patterns (e.g., AO, SOI, PDO) is an intriguing result. The manuscript would benefit from a deeper exploration of the physical mechanisms driving these relationships.

We briefly discussed the physical mechanisms linking teleconnection patterns to LST extremes in the original manuscript. However, we have now expanded this discussion to provide a more detailed analysis of these relationships. Please refer to the revised manuscript (lines 252-270 and 271 to 292).

R2-8 In summary, the manuscript presents a thorough and innovative investigation of LST extremes and their evolution under climate change. Addressing the above points would significantly strengthen the manuscript and ensure its impact on the scientific community and stakeholders focused on climate adaptation and ecological management.

Thank you for the positive feedback!

Reviewer #3

In this manuscript, Abdelhady et al. analyze extreme lake surface water temperature (LSWT) changes over the past 80 years in the Laurentian Great Lakes using a hydrodynamic-ice model driven by ERA5 climate data. The study examines temperature extremes by analyzing changes in the 10th and 90th percentiles, as well as the occurrence, duration, and intensity of heatwaves and cold spells. The results are interpreted in relation to teleconnection indices.

The manuscript is interesting and well-written. However, there are some aspects that require clarification before it can be considered for publication.

Major Comments:

R3-1 The authors use a 3D model calibrated only with LSWT data. Moreover, they rely solely on lake-averaged values for model validation, thus not fully leveraging the advantages of 3D modeling. From a methodological perspective, this is an oversimplification, as a 3D model requires thorough validation. Given this approach, a 1D model might be preferable for two reasons: (1) using a 3D model while performing spatial averaging does not take full advantage of the benefits of 3D modeling, and (2) if a 3D model is used, it must be properly validated. The authors should clarify the advantages of using a 3D model over a 1D model if only mean values are considered, and they should provide model validation details (e.g., at least comparing simulated vs observed vertical temperature profiles). In this regard, lines 83-84 do not clearly specify whether daily or temporally aggregated data are used to calculate RMSE.

- 1) We acknowledge that using a spatially averaged 3D model does not fully utilize its capabilities. However, 1D models have been shown to be less accurate for the Great Lakes due to their large spatial extent and depth, often yielding poorer results than a spatially averaged 3D model. References [1, 2, 3, 4], listed below, discuss the accuracy differences between 1D and 3D models for the Great Lakes. Additionally, the model outputs used in this study contribute to other research projects by co-authors and collaborators.
- 2) The model has undergone extensive validation and testing, as documented in previous studies. We have added a paragraph in the Methods section (lines 342–359) referencing these validation efforts.
- 3) Daily data were used for model validation. We have revised the manuscript to clarify this point (line 84).

R3-2 The methodology is often unclear, particularly regarding the temporal resolution considered at different processing steps. For instance, line 333 mentions the "entire time series," but its meaning is unclear. The same issue arises on line 363. The spectral analysis is only briefly described. For example, it is unclear what is meant by "red noise spectrum based on a lag-one autocorrelation coefficient ($\alpha=0.5$).\" More details should also be provided regarding "the 95% confidence levels from the red noise spectrum using the F-statistic, with 2 degrees of freedom.\" Similarly, "performed spectral coherence analysis\" lacks sufficient explanation. These details should be explicitly included in the Methods section rather than inferred from reading the Results. Additionally, Figure 8 lacks axis labels, and the interpretation of the red noise fitting and 95% confidence interval are unclear.

We have revised the Methods section to clarify these points. Please refer to the updated Methods section in the modified manuscript.

Additionally, we have corrected Figure 8 to include appropriate axis labels. Regarding the interpretation of the red noise spectrum, we have clarified in the text that a peak exceeding the 95% red noise threshold indicates statistical significance, meaning we can reject the null hypothesis that it arises from random noise. These clarifications are included in the revised Methods section (lines 417-445).

R3-3 The analysis could be more detailed and sometimes remains at a superficial level. For instance, the double peak in Lake Superior in May and June is worth discussing: could it be related to early stratification dynamics? While I agree that lakes have lower thermal inertia than the ocean (L235-239), in Lake Superior, thermal inertia plays a crucial role (see <https://doi.org/10.1002/2014WR016555>, <https://doi.org/10.1002/lno.10331>, <https://www.nature.com/articles/s41598-017-04058-0>). The effect of stratification should be investigated.

Thank you for pointing this out. The emerging peak in May and June has become more pronounced in recent years (as seen in the yellow PDFs in the figure), likely due to earlier ice-off and the onset of stratification occurring earlier in the year. We have highlighted this in the Discussion section (lines 265–270).

We agree that Lake Superior exhibits significant thermal inertia. However, in this context, we were comparing lakes to the ocean. The phrasing was intended to emphasize the difference in thermal response between the two systems, and we believe it remains appropriate in this comparison.

R3-4 L118-119: "All lakes showed a significant increase in annual low temperature extremes (10th percentile)." Do the authors mean that the temperature associated with the 10th percentile increased or decreased? Looking at the cdfs in the figures, I would assume the latter, but in the text the authors use the term "increase", which may be misleading.

The 10th percentile values were normalized relative to a reference period (1940–1970), meaning the reported increases refer to relative changes rather than absolute temperature shifts. Since the 10th percentile DLST values are negative, normalization results in a positive trend. To clarify this, we have revised the text to explicitly state that we are referring to normalized annual low-temperature extremes (line 121).

R3-5 L219: The Atlantic Multidecadal Oscillation (AMO) has not been investigated. I assume the authors are referring to the Arctic Oscillation (AO).

AMO was not analyzed for coherence. Here, we were referring to the nonlinear trend extracted from the original time series and used to detrend the LST data before extreme value analysis. We have revised the text to clarify this distinction (lines 220–227).

R3-6 I think that some literature on Great Lakes thermal/climate patterns is missing. The authors may consider adding the following studies: <https://www.mdpi.com/2072-4292/15/17/4253>, <https://doi.org/10.1080/20442041.2021.1873698>, <https://doi.org/10.3389/frwa.2021.782265>, <https://doi.org/10.1016/j.jhydrol.2022.128655>, <https://doi.org/10.1007/s00382-018-4481-z>, <https://doi.org/10.1023/A:1022140604052>, <https://iopscience.iop.org/article/10.1088/1748-9326/ad8ee0/meta>

Thank you for these suggestions. We have incorporated the most relevant studies where appropriate throughout the manuscript.

Minor Comments

R3-7 In the Abstract, the term "vital" could be replaced with a better term. Additionally, the authors refer to "tipping points" but this concept is not further investigated in the manuscript.

We have replaced "vital" with "significant" in the Abstract.

The concept of tipping points is addressed in the Discussion section (lines 271-302).

R3-8 L38: The cited works likely include the Great Lakes.

Thank you for pointing this out. We have revised this section to clarify that these studies either did not include the Great Lakes or treated them separately for testing and validation. Please refer to the updated manuscript (lines 37–40).

R3-9 L323: "The" → "the".

Fixed.

R3-10 L331-332: Use "LST" instead of "STL".

Fixed.

R3-11 L340: "ΔLST" should be introduced at L332.

Fixed

R3-12 L391: Double "the".

Fixed

R3-13 L396: "The" → "the".

Fixed

R3-14 Figure 1: The lettering of subplots is missing. In the caption, the use of parentheses is inconsistent, as is capitalization.

Fixed

R3-14 Figure 1: The first plot should be commented and referenced in the text.

Fixed

R3-15 Typically, Lake Surface Water Temperature (LSWT) is used instead of Lake Surface Temperature (LST) to avoid confusion with Land Surface Temperature (also abbreviated as LST). The authors may consider adopting LSWT for clarity and consistency.

Thank you for the suggestion. While LSWT and LST are often used interchangeably, we have chosen to retain LST for consistency with previous work by the co-authors and to ensure alignment with terminology used elsewhere in the manuscript.

References

- [1] Xue, Pengfei, et al. "Enhancing Winter Climate Simulations of the Great Lakes: Insights from a New Coupled Lake-Ice-Atmosphere (CLIAv1) Model on the Importance of Integrating 3D Hydrodynamics with a Regional Climate Model." *Geoscientific Model Development Discussions* 2024 (2024): 1-41.
- [2] Notaro, Michael, et al. "Cold season performance of the nu-wrf regional climate model in the Great Lakes region." *Journal of Hydrometeorology* 22.9 (2021): 2423-2454.
- [3] Xiao, Chuliang, et al. "Improving the lake scheme within a coupled WRF-lake model in the Laurentian Great Lakes." *Journal of Advances in Modeling Earth Systems* 8.4 (2016): 1969-1985.
- [4] Cannon, David, et al. "Investigating Multidecadal Trends in Ice Cover and Subsurface Temperatures in the Laurentian Great Lakes Using a Coupled Hydrodynamic–Ice Model." *Journal of Climate* 37.4 (2024): 1249-1276.

Response to the Reviewer's Comments

We thank reviewer #3 for the valuable comments and suggestions. We have revised the manuscript to address all comments and implement suggestions. Please, see below for our detailed response to comments and explanation of manuscript edits. The reviewer's comments are in black, italic font, with our response in regular blue font. All line numbers refer to the clean revised manuscript unless stated otherwise.

Reviewer #3

The authors have responded positively to the previous comments. Overall, I appreciate the authors' revisions, but I still have some concerns that need clarification:

L 37-40: Referring back to comment R3-8 in the previous review round, I still do not agree that the Great Lakes are excluded or included with special validation in global studies (e.g., references 3 and 23). In many cases, they are used for validation due to the availability of long and good quality data records. To prevent any confusion, this sentence was removed.

L 122: Instead of "normalized," I suggest using "relative to the reference period 1941-1970" for consistency, as the manuscript refers to "relative" and does not use "normalized" elsewhere. Additionally, the authors' response in the rebuttal letter ("Since the 10th percentile DLST values are negative, normalization results in a positive trend") clarified that an increasing relative 10th percentile indicates colder low temperatures. Explicitly stating this in the manuscript would improve clarity.

The wording was changed to improve clarity. Please refer to the modified manuscript lines 116-119.

L 221-225: This section remains somewhat unclear. Why has AMO been considered separately from NAO, AO, SOI, PDO, and ONI? AMO is not mentioned in the methods section, and no figures are provided to support its discussion. Finally, the statement that "the Atlantic Multidecadal Oscillation (AMO) was not explicitly used to interpret the periodicity of heatwaves and cold spells, since the long-term nonlinear trend was already removed" needs further clarification: why is this an issue for AMO but not for the other teleconnection indices?

The Atlantic Multidecadal Oscillation (AMO) has a period of approximately 60–90 years, meaning that removing the long-term nonlinear trend effectively eliminates AMO variability, as the two are highly correlated. Furthermore, from a statistical standpoint, an 80-year record is insufficient to robustly identify a signal with a period spanning more than half of the dataset's length. In contrast, the other teleconnection indices considered (NAO, AO, SOI, PDO, and ONI) have much shorter periods, allowing their influence to be analyzed without such constraints.

The text was modified to clarify this point. Please refer to the modified manuscript lines 214-225.

R3-1: A brief comment in the manuscript explaining why a 3D model was chosen over a 1D model would be beneficial, as many readers may wonder about this decision. The authors provided a clear explanation

in their response in the rebuttal letter, and including it in the text would strengthen the justification for using a 3D model.

The test was modified to justify the use of a 3D model instead of a 1D. Please refer to the modified manuscript, lines 320-323.