nature portfolio

Peer Review File

Recent changes in ENSO's impacts on the summertime circumglobal teleconnection and mid-latitude extremes

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
This study proposes that changes in the structure and variability of the circumglobal teleconnection (CGT) mode in the Northern Hemisphere around late 1970 can be attributed to a direct impact from ENSO. This is a shift from previous understanding, which emphasized the role of the Indian summer monsoon as an intermediary. The results of this study indicate that the weakening of the ENSO-monsoon relationship allows a direct interaction between ENSO and CGT and changes experienced by the CGT can be explained by the westward shift of ENSO activity from the eastern Pacific and accompanying teleconnections to the midlatitudes. The study also concludes that these changes are due to natural climate variability rather than anthropogenic forcing. The analysis is conducted using reanalysis, a low complexity baroclinic model and a coupled earth system model.

The study is of high interest and relevant for seasonal prediction of extreme events drawing their predictability from ENSO. The major concern I have about the study is the robustness of the findings. I suggest the authors apply the same analysis to other reanalysis products. For example, JRA-55 comparable period as ERA5. ORAS5 Global Ocean Analysis can be used instead of ERSSTv5.

My other concerns are related to gaps in the presentation and discussion. They include:

L54: ‘positive phase CGT’ - please describe the overall characteristics of each phase.

L61: ‘The enhanced CGT’ - please describe what this means

L63-65: ‘A better understanding of ...’ This statement does not follow from the previous discussion.

L67-68: The statement in line 54 argues that CGT promotes suppressed convection. Here the argument is reversed. In line 61 enhanced CGT is associated with La Nina and here La Nina enhances monsoon rainfall. These arguments need to be reconciled.

L74-76: The implied seesaw pattern forced by ENSO needs to be introduced.

L89-90: In the previous paragraph the change in the ENSO-CGT after 1970 is discussed as a hypothesis. In this sentence the change is introduced as an accepted fact.

L159-160: Most previous studies suggest a weakening of the Walker circulation after the late 70s and the change is attributed to climate change. Please comment on this.

L291-293: This needs to be further expanded. Describe what model improvements can be achieved by understanding the ENSO-CGT relationship and how future changes in mid-latitude waves can be better projected.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)
Overview

The authors report that the boreal summer circumglobal teleconnection (CGT) has shifted westward by a half-wavelength since the late 1970s, intensifying heatwaves and droughts in East Europe, East Asia, and southwestern America. They attribute this shift to changes in the dynamics of the El Niño-Southern Oscillation (ENSO) and support their assertion with both empirical and modeling evidence. The subject is relevant and would be of interest to the climate research community. However, I have concerns regarding the originality and novelty of the study, especially considering the level of Nature Communications. Much of the content appears to have been covered in previously published work, including Wang et al., 2012 https://doi.org/10.1029/2012GL052371) and Ding et al., 2011 (https://doi.org/10.1175/2011JCLI3621.1), and is not necessarily breaking new ground. But of course, I stand to be corrected. I have provided specific comments below.

Specific Comments

1. The observed change in the summer CGT pattern appears to align with the two CGT types (i.e., the traditional type and the Western Pacific–North America type) or “flavors” identified by Ding et al. (2011) in a similar analysis spanning 1948–2009. We may all agree it is one thing to say there exist two distinct modes (i.e., with distinct underlying mechanisms, spatial patterns, statistical characteristics, etc.) varying on both interannual and interdecadal timescales versus a single mode with temporal changes over time (i.e., consistent spatial pattern, mechanisms, similar statistical signature but with temporal changes). It appears the authors are referring to a singular mode, given that either half of the PC(2) time series is used to represent the CGT patterns in the two epochs. For two naturally occurring interannual modes of CGT variability, I would expect to see two separate time series from independent EOF modes, as demonstrated in Ding et al. (2011).

As the authors rightly note, Wang et al. (2012) later identified an interdecadal change in the CGT occurring around the late 1970s, which is effectively the same shift being reported in the present study. Given these similarities, how do the authors reconcile or discriminate their results from those reported in the aforementioned studies? What new insights or contributions does this study provide beyond confirming the previously identified patterns and/or shifts? I think this is an important point to clarify.

2. For clarity, what happens if you regress Z200 onto the principal component time series of the dominant EOF modes and analyze changes over the entire observation period, i.e., 1951-2020? Alternatively, what if the EOF is applied by epoch – will we see the two CGT “types” reported by Ding et al. in each epoch? If yes, wouldn't it suggest that the shift that is being reported here could better be characterized as either a predominance of the Western Pacific–North America type in the latter epoch or a weakening of the “traditional CGT” accompanied by the strengthening of the “Western Pacific–North America type”?

3. The authors have provided a clearer analysis of the linkages between ENSO diversity and the apparent CGT shift which represents an addition to the literature. However, Ding et al. (2011) also related the two CGT “types” they identified to ENSO. Therefore, a discussion contrasting these findings with those of the present study seems warranted to, fill the knowledge gap, if any, and to facilitate the progression of the science.

4. Generally, I would recommend that the authors employ the maximum covariance analysis (MCA) method to more accurately define these patterns, as demonstrated in Wang et al. (2012) and Ding et al. (2011), rather than relying on EOFs. The use of EOF to define CGT has significant limitations, especially in GCMs, where the “traditional CGT” and the “Western Pacific–North America” type can appear as any of the first four EOF modes.

5. Extended Data Figure 1 shows that the relationship between Nino3.4 and ISMR, as well as between ISMR and CGT, becomes weaker after 1980. However, during this latter epoch, the CGT still shows a connection to ENSO (Nino 3.4). If we are observing two modes, they should have distinct underlying mechanisms. For instance, some studies, such as Lin et al. (2023), suggest that the "traditional CGT type" is more directly related to Indo-Western Pacific SST variability but is indirectly related to ENSO, hence the significant correlation with ENSO as well (https://doi.org/10.3389/feart.2023.1033789).

6. I would interpret Figs 6a and 6b as showing differences in amplitude and not necessarily distinct pattern changes or shifts. Both look like Fig. 1b, although the expectation is that one would look like Fig. 1a and the other, Fig. 1b. Is this model inability to simulate the “traditional CGT pattern” (as in Fig. 1a)? Or does this suggest, there is no direct linkage between this CGT type and ENSO but perhaps an indirect link as suggested Lin et al. 2023?

7. Fig 6: is there a rationale for using regression to construct Figs 6a and 6b but composites in Figs 6d and 6e?

8. The designed experiments are an important addition. However, I find the placement of the relative vorticity forcings in the baroclinic model quite confusing. Based the baroclinic structures presented in Figs 4c, 4d, and Fig. 7, the CGT pattern in

recent decades is induced over the TWNP. In that case, shouldn't the location of the relative vorticity be in the vicinity of 10°-20°N or the tropical region? If not, how, then, can we directly attribute causality in this context? The experimental setup is not described particularly well so I guess I am a little confused.

9. The authors utilized the NOAA PREC 2.5-degree data. So, I was surprised to see the level of detail depicted in Extended Fig. 5 and out of curiosity I attempted to replot (see figure below) and got something different, especially the difference plot. What could I be missing? Since the location of the suppressed convection and rainfall over the TWNP is crucial to driving the tropical-extratropical teleconnection, it would be great if the authors would test different precipitation datasets for consistency. Alternatively, using velocity potential fields may better illustrate the convection anomalies over the TWNP. What's more, my attempts to replicate Extended Fig. 5 showed convection anomalies rather than suppression over the TWNP.

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)
This paper delves into the evolution of circumglobal teleconnection (CGT) in summer over the decades. Specifically, it analyses the changes that have occurred when comparing the years before and after the 1970s. The authors analyse the reasons for the change, studying the relationships with El Niño Southern-Oscillation (ENSO) and the westerly jet. In addition, model simulations are used to verify observational hypotheses.

In my opinion the manuscript is very well organized, and the methodologies used are appropriate. In addition, the results are relevant and robust, making the paper very interesting. Therefore, I consider its publication in this journal valid.

Even so, here are some thoughts and doubts that I would like to be considered before publication.

MAYORS
General
• Numerous figures show the differences in variables between periods (before and after the 1970s). In my opinion these differences should show significance to detect the areas in which these changes are statistically significant. Please include the significance in the figures: Fig.1c, Fig. 3c and f, Fig. 5a, Fig. 6c and f, Extended Data: Fig.4c and f, Fig. 5c and f, Fig. 6c and f.

• Although there is a section in the manuscript that analyses the role of the jet in the spread of CGT and concludes that the placement of the jet changes between before and after the 1970s and that this may be key in the placement of the centres of action of CGT over North America and western Europe, this conclusion seems to lose relevance since it does not appear in the introduction or in the conclusions.

INTRODUCTION
• L77-88. In this paragraph the authors explain the changes in the CGT between before and after the 1970s. As I suggested at the beginning of my comments, I am missing at this point a paragraph or some sentence that introduces the changes in the westerly jet.

RESULTS
Role of the ENSO
• L128-144 and Fig. 2. The manuscript describes Fig. 2 and the different action centres associated with ISMR and ENSO before and after the 1970s. Black boxes indicate the different centres (I supposed; see figures comments below). I have doubts regarding the North American action centre. In Fig. 2b the box is placed in the northeast, while the area of significance is placed further to the southwest. In the period after the 1970s, in Fig 2e, and even in Fig 2d, following the shading significance, this centre is in the southwest, in a place similar to where it is indicated in Fig 2f. Nevertheless, the black boxes are plotted in the northeast of North America. In my opinion, the authors should go more deeply into the location of these centres of action because they will condition the final results of the study.

Role of the westerly jet
• L220-223. The manuscript explains that there is a promoted centre over Ural Mountain, indicating to look at Fig. 5a. I cannot see this centre in the mentioned figure panel.

Numerical experiments validation
• L237-240. In my opinion, outside the North Pacific, the rest of the centres do not fit the observations as well. Please indicate it in the manuscript. Although I agree that the similarity between model simulations and observational results is

acceptable.
• L251. "… enhanced centres over southwestern America …." and L269 "… eastern Canada to southwestern America…". In my opinion it is more correct to write "…southwestern North America…".
• L249-252. "Compared…(Fig. 6d vs. e)". I see no major differences between panels (d) and (e). The significant regions are similar and correspond to the same action centres. On the other hand, it is true what the authors comment regarding the intensification of the centres over the North Pacific, North America and the North Atlantic in response to central Pacific-type ENSO. Calculating the statistical significance of the differences presented in panel (f) could help in this interpretation.

CONCLUSION AND DISCUSSION
• L289. "… westerly jet waveguide (Fig. 7b)". As I suggested at the beginning of my comments, I am missing at this point any sentence regarding the westerly jet changes and their effect on the downstream CGT centres of action.
• L301. Extended Data Fig. 10. Anything is mentioned about panels (c) and (d). Black boxes are plotted indicating (I supposed) the action centres, but same of them do not correspond to the shading, specially the one over North America in panel (c) and the one over eastern Europe in panel (d). Please clarify it. Significance could be computed to better justify these centres.

METHODS
Observation data
• Although it is indicated in the main text, I think it would be appropriate to repeat in this section the years and months (summertime) used in the study.
• L323. "…climatological mean…". Clarify please which period is considered to calculate this mean.

MINORS
• L143. Please indicate the b panel of the Fig.1, Fig.1b.
FIGURES
• In several figures "P1" and "P2" are used to refer to the period before and after the 1970s, respectively. Although it is quite intuitive, I think it would be convenient to indicate in the figures captions to which these acronyms correspond. Fig. 1c, Fig. 3c and f, Fig. 6c, Extended Data: Fig. 4c and f, Fig. 5c and f, Fig. 6c and f.
• Please write in the figure captions what the black boxes indicate. Fig.1a and b, Fig.2, Fig. 4c and d, Fig.6a-b and d-e, Extended Data: Fig. 9a-b, Fig. 10c-d.
• Please indicate the statistical significance of the fields plotted with arrows. Fig. 3d-f, Fig. 4a-b, Fig. 5b-c, Extended Data: Fig. 5d-f.
• Fig. 4. L196-199. In the caption is written "(c) averaged over 170ºE-150ºW….. (d) averaged over 130ºE-170ºE…." It means zonal averaged is not it?
• Fig. 4e-f. L201. In the caption is written "during the 1951–1979 (solid line) and 1980–2020 (dotted line)" but in the corresponding panel there is only one dotted line, no one solid. Perhaps a line is missing in each panel? Please clarify.
• Extended Data Fig. 7. If it is not too much trouble, I think it would be nice to see these same fields but for the period after the 70s.
• Fig. 5. In panel (a) a green line is used to represent the jet axis. But in that figure the differences in the jet between periods are shown (before and after the 70s). To which period does this axis correspond, or is it an average? Please clarify.

TYPOS
• L224. Fig. 5. The title of the (a) panel "U200 & UV200". In the caption it is said that only the zonal wind (U200) is plotted.
• L437. Reference 17. "t1emperature extremes".

Suggested bibliography

Jong et al. (2020 and 2021) analyse the different teleconnection patterns in response to La Niña in summer, when La Niña is transitioning from El Niño and when it is a persisting La Niña, concluding that in the latter the extratropical teleconnection is insignificant. Martija-Díez et al. (2021 and 2023) analyse the differences on ENSO impacts over Europe in summer along the decades and they conclude that westerly jet characteristics are fundamental.

Jong, B. T., Ting, M., Seager, R., & Anderson, W. B. (2020). ENSO teleconnections and impacts on US summertime temperature during a multiyear La Niña life cycle. Journal of Climate, 33(14), 6009-6024. https://doi.org/10.1175/JCLI-D-19-0701.1
Jong, B. T., Ting, M., & Seager, R. (2021). Assessing ENSO summer teleconnections, impacts, and predictability in North America. Journal of Climate, 34(9), 3629-3643. https://doi.org/10.1175/JCLI-D-20-0761.1
Martija-Díez, M., Rodríguez-Fonseca, B., & López-Parages, J. (2021). ENSO Influence on Western European summer and fall Temperatures. Journal of Climate, 34(19), 8013-8031. https://doi.org/10.1175/JCLI-D-20-0808.1
Martija-Díez, M., López-Parages, J., Rodríguez-Fonseca, B., & Losada, T. (2023). The stationarity of the ENSO teleconnection in European summer rainfall. Climate Dynamics, 61(1), 489-506. https://doi.org/10.1007/s00382-022-06596-4

(Remarks on code availability)
As indicated by the authors, all calculations in this study have been produced using NCAR Command Language (NCL) version 6.4.0, and the source codes can be obtained upon request to the corresponding authors.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have satisfactorily addressed my main concerns.

(Remarks on code availability)
The code is not available.

Reviewer #2

(Remarks to the Author)
I appreciate the authors' efforts in thoroughly addressing my concerns. The revisions have significantly improved the clarity of the article. I have no further comments.

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)
I would like to thank the authors for following my suggestions and kindly answering my questions. I have detected a couple of errors that need to be corrected before the article is published and I would also like to make one last suggestion.

1. It was a great success to use the green dotted and black solid boxes to refer to the locations of the observed action centres of CGT in both periods. I suggest if it is not too much trouble, to use this same criterion in the Figure 1

2. The title of the Fig 5a, is still wrong. Correct it please.

3. Thank you for considering my bibliographical suggestion. However, I think there was some mistake. I cannot find any references to Martija-Díez et al. (2021, 2023) in the bibliography and I think that Jong et al. (2020, 2021) are not entered in the correct bibliography number. Please correct this.

(Remarks on code availability)

Response to Reviewer 1

Summary:

This study proposes that changes in the structure and variability of the circumglobal teleconnection (CGT) mode in the Northern Hemisphere around late 1970 can be attributed to a direct impact from ENSO. This is a shift from previous understanding, which emphasized the role of the Indian summer monsoon as an intermediary. The results of this study indicate that the weakening of the ENSO-monsoon relationship allows a direct interaction between ENSO and CGT and changes experienced by the CGT can be explained by the westward shift of ENSO activity from the eastern Pacific and accompanying teleconnections to the midlatitudes. The study also concludes that these changes are due to natural climate variability rather than anthropogenic forcing. The analysis is conducted using reanalysis, a low complexity baroclinic model and a coupled earth system model.

The study is of high interest and relevant for seasonal prediction of extreme events drawing their predictability from ENSO. The major concern I have about the study is the robustness of the findings. I suggest the authors apply the same analysis to other reanalysis products. For example, JRA-55 comparable period as ERA5. ORAS5 Global Ocean Analysis can be used instead of ERSSTv5. My other concerns are related to gaps in the presentation and discussion.

Reply: We would like to thank the anonymous reviewer for spending valuable time reading the manuscript and providing constructive and valuable feedbacks. Per your comments, we have tried to revise the manuscript and address your concerns.

To address your concern about robustness, we have repeated the results by using JRA-55 (beginning from 1958) and HadISST (part of ORAS5), and the main conclusions remain unchanged (Fig. R1.1, 1.2). We have also clarified this in the Data: "*The robustness of this work has been confirmed by repeating with other alternative datasets such as JRA-55 and HadISST*" (See Lines 373–376).

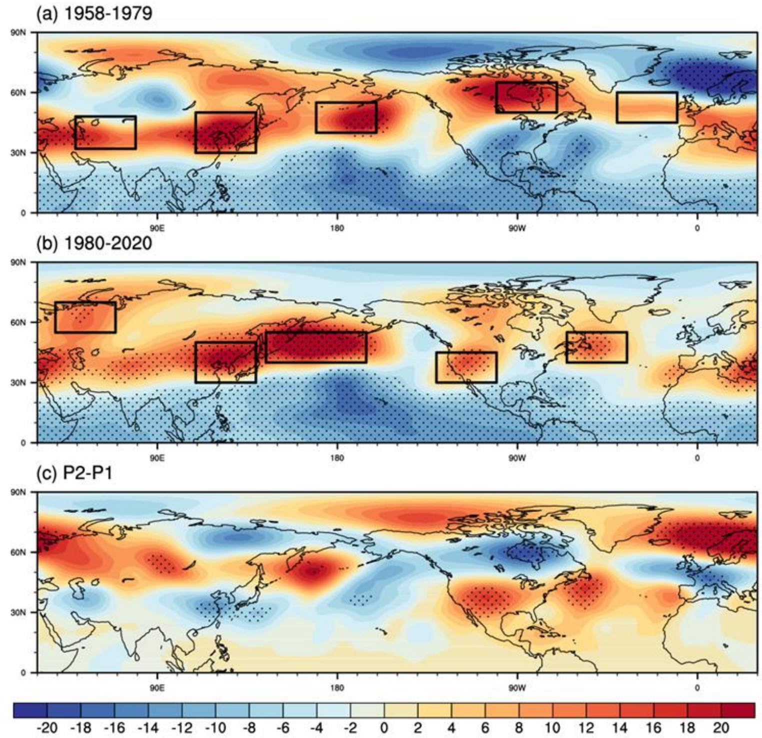


Fig. R1.1: Same as Fig. 1, but for the JRA-55.

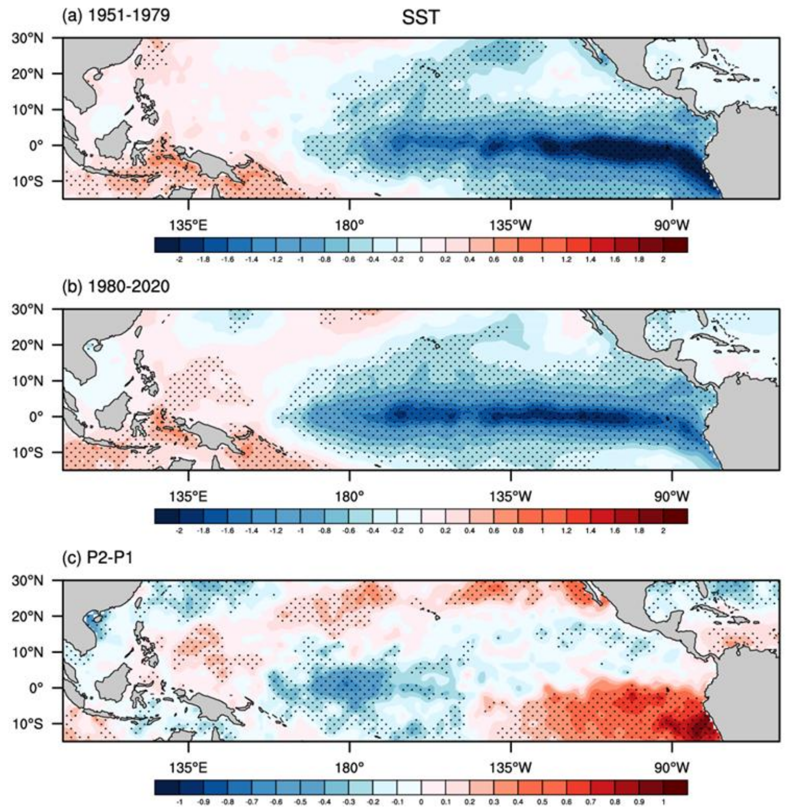


Fig. R1.2: Same as Fig. 3a-c, but for the HadISST.

Comments:

1. L54: ‘positive phase CGT’ - please describe the overall characteristics of each phase.

Reply: Thank you for the comment. Following your suggestion, we have rewritten this sentence: “*The positive-phase CGT is featured as a zonal 5-wavenumber structure with geographically phase-locked centers of action over the mid-latitude West Asia, East Asia, North Pacific, North America and West Europe*[16]*, promoting the descending motion and suppresses the cloud cover and rainfall along the wave train and thereby inducing heatwaves and droughts over the mid-latitude NH*[17,18] (vice versa).” (See Lines 52–56).

2. L61: ‘The enhanced CGT’ - please describe what this means

Reply: Thank you for the comment. The enhanced CGT means its positive phase here. To avoid misunderstanding, we have rewritten this sentence: “*Specifically, the positive(negative)-phase CGT is significantly modulated by an enhanced (suppressed) Indian summer monsoon rainfall (ISMR) and a La Niña (El Niño) developing phase*[16,21].” (See Lines 61–64).

3. L63-65: ‘A better understanding of ...’ This statement does not follow from the previous discussion.

Reply: Exactly, we have deleted this statement here and removed it to the Conclusion and Discussion in the revised manuscript: “*This work may be helpful for future model improvements in predictions and projections. Despite the highly-predictable interannual intensity of CGT sourced from ENSO, the CGT’s centers of action simulations are still facing challenges*[22,46]*. A deeper understanding of the recent CGT structure change should help improve the seasonal predictions of the mid-latitude NH heatwaves and droughts associated with the ENSO. Moreover, a better understanding of the ENSO–CGT linkage may provide some references for constraining extensive ensemble simulations to improve the projection accuracy of future climate change over the mid-latitude NH.*” (See Lines 331–338).

4. L67-68: The statement in line 54 argues that CGT promotes suppressed convection. Here the argument is reversed. In line 61 enhanced CGT is associated with La Nina and here La Nina enhances monsoon rainfall. These arguments need to be reconciled.

Reply: Thank you for raising the concern. The suppressed convection induced by CGT above is located over the mid-latitude NH along the wave train, but the enhanced convection here is located over the lower-latitude Indian Peninsula, southeast of the wave train.

To avoid misunderstanding, we have rewritten these statements: "*Specifically, the positive(negative)-phase CGT is always accompanied by a La Niña (El Niño) developing phase and an enhanced (reduced) Indian summer monsoon rainfall (ISMR)*[15,20]" (See Lines 61–64), and "*In turn, the upper-level anticyclone over the West Asia center of CGT helps maintain the ISMR by enhancing convection and easterly vertical shear to its southeast over the Indian Peninsula.*" (See Lines 73–75).

5. L74-76: The implied seesaw pattern forced by ENSO needs to be introduced.

Reply: Thank you for the suggestion. We have introduced it in the revised manuscript: "*Except for these indirect pathways, the zonally symmetric seesaw pattern forced by ENSO is another reason for the robust ENSO–CGT linkage: The summertime development of La Niña can induce a zonally symmetric low in tropics and thereby favors a consistent high in mid-latitudes due to meridional compensation*[21]*.*" (See Lines 79–83).

6. L89-90: In the previous paragraph the change in the ENSO-CGT after 1970 is discussed as a hypothesis. In this sentence the change is introduced as an accepted fact.

Reply: Thank you for pointing out this mistake. According to your comment, this sentence has been rewritten in the revised manuscript as *"Above all, better understanding the recent ENSO's impacts on CGT motivates us to conduct this study."* (See Lines 104–105).

7. L159-160: Most previous studies suggest a weakening of the Walker circulation after the late 70s and the change is attributed to climate change. Please comment on this.

Reply: Thank you for the comment. Here we mainly focused on the displacement of the Walker circulation anomaly caused by the westward shift of ENSO at the interannual timescale, without considering its intensity change in response to climate change.

8. L291-293: This needs to be further expanded. Describe what model improvements can be achieved by understanding the ENSO-CGT relationship and how future changes in mid-latitude waves can be better projected.

Reply: Thank you for this valuable suggestion. According to your comment, we have highlighted it in the Discussion in the revised manuscript: "*This work may be helpful for future model improvements in predictions and projections. Despite the highly-predictable interannual intensity of CGT sourced from ENSO, the CGT's centers of action simulations are still facing challenges*[22,46]*. A deeper understanding of the recent CGT structure change should help improve the seasonal predictions of the mid-latitude NH heatwaves and droughts associated with the ENSO. Moreover, a better understanding of the ENSO–CGT linkage may provide some references for constraining extensive ensemble simulations to improve the projection accuracy of future climate change over the mid-latitude NH.*" (See Lines 331–338).

Response to Reviewer 2

Summary:

The authors report that the boreal summer circumglobal teleconnection (CGT) has shifted westward by a half-wavelength since the late 1970s, intensifying heatwaves and droughts in East Europe, East Asia, and southwestern America. They attribute this shift to changes in the dynamics of the El Niño-Southern Oscillation (ENSO) and support their assertion with both empirical and modeling evidence. The subject is relevant and would be of interest to the climate research community. However, I have concerns regarding the originality and novelty of the study, especially considering the level of Nature Communications. Much of the content appears to have been covered in previously published work, including Wang et al., 2012 https://doi.org/10.1029/2012GL052371) and Ding et al., 2011 (https://doi.org/10.1175/2011JCLI3621.1), and is not necessarily breaking new ground. But of course, I stand to be corrected. I have provided specific comments below.

Reply: We would like to thank the anonymous reviewer for spending valuable time reading the manuscript and providing constructive and valuable feedbacks. Per your comments, we have tried to revise the manuscript and address your concerns.

Comments:

1. The observed change in the summer CGT pattern appears to align with the two CGT types (i.e., the traditional type and the Western Pacific–North America type) or "flavors" identified by Ding et al. (2011) in a similar analysis spanning 1948–2009. We may all agree it is one thing to say there exist two distinct modes (i.e., with distinct underlying mechanisms, spatial patterns, statistical characteristics, etc.) varying on both interannual and interdecadal timescales versus a single mode with temporal changes over time (i.e., consistent spatial pattern, mechanisms, similar statistical signature but with temporal changes). It appears the authors are referring to a singular mode, given that either half of the PC2 time series is used to represent the CGT patterns in the two epochs. For two naturally occurring interannual modes of CGT variability, I would expect to see two separate time series from independent EOF modes, as demonstrated in Ding et al. (2011).

As the authors rightly note, Wang et al. (2012) later identified an interdecadal change in the CGT occurring around the late 1970s, which is effectively the same shift being reported in the present study. Given these similarities, how do the authors reconcile or discriminate their results from those reported in the aforementioned studies? What new insights or contributions does this study provide beyond confirming the previously identified patterns and/or shifts? I think this is an important point to clarify.

Reply: Thank you for the comment. Let us first address your concern about the analysis methodology stated in the first paragraph.

Indeed, we examined a single CGT mode rather than two. This is a popular method to investigate the interdecadal structure change of a pattern defined by the EOF mode, such as Pacific–Japan pattern (Xu et al., 2018, https://doi.org/10.1175/JCLI-D-18-0123.1), without justification.

The two EOF modes for the entire period are shown in Fig. R2.1, as you requested. The EOF1 represents a uniform negative anomaly superposed with the WP-NA pattern, and EOF2 shows the CGT pattern. These two modes explain 23.9% and 11.9% of the total variance. They are statistically significantly separated, representing two orthogonal patterns preferably occurring in the decaying (EOF1) and developing (EOF2) years of ENSO and associated with the Philippines heat source (EOF1) and Indian heat source (EOF2), respectively (Ding et al. 2011).

When the EOF analysis is conducted in each period independently, similar structure change is shown between two EOF2 modes during each period (Fig. R2.2), and the main conclusions remain unchanged.

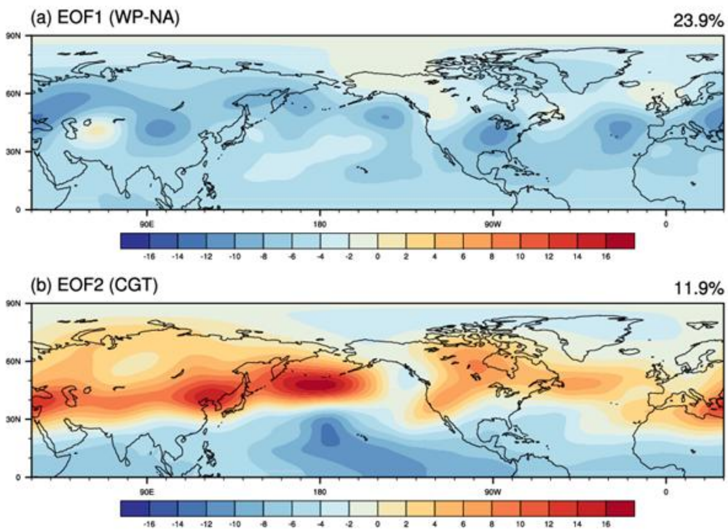


Fig. R2.1: The Western Pacific–North America and CGT patterns throughout the period. (a) EOF1 and (b) EOF2 modes of the 200-hPa geopotential height anomalies over the Northern Hemisphere.

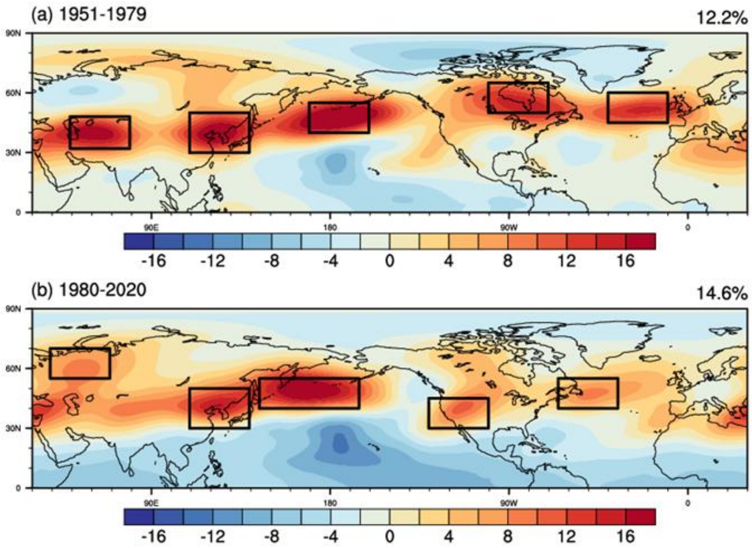


Fig. R2.2: Same as Fig. 1, but for the EOF2 mode during each period.

To avoid misunderstanding, we have clarified how to distinguish the CGT structure change and explained the reasons in the revised manuscript: "*We carried out an empirical orthogonal function (EOF) analysis of the summertime 200-hPa geopotential height anomalies over the NH during the whole period. The second EOF mode (EOF2) and principal component (PC2) are applied to define the CGT*[16]*. Consistent with the previous studies*[27,28]*, we divide the 70 years of the 1951–2020 period into 1951–1979 and 1980–2020 in this study, and 200-hPa geopotential height anomalies are respectively regressed onto the separated PC2 during these two sub-periods to represent the structure change of CGT, as shown in Fig. 1. This is a common method to investigate the interdecadal structure change of a pattern defined by an EOF mode*[36]*. Note that the main results and conclusions are similar when the EOF analysis is conducted independently during each period.*" (See Lines 112–121).

Next, we address your concern about the novelty expressed in the second paragraph. Wang et al. (2012) highlighted that the CGT structure experienced an interdecadal weakening of the major CGT centers after 1979, mainly attributed to the decreased ISMR variability. The study did not notice the robust structure change of CGT with enhanced North Pacific center and half-wavelength westward shift of centers downstream. Second, the weakened ISMR failed to explain the enhanced

ENSO–CGT linkage and the recently increased CGT variability. Therefore, our study provides new insights, that is, the recently changing ENSO effects on CGT, which explains the recent CGT structure change with enhanced North Pacific center and half-wavelength westward shift of centers downstream. This is beyond the traditional view that the ENSO effects on CGT are indirect, mainly through modulating the ISMR.

According to your comment, we have rewritten new insights in the Introduction: "*It has been noted that the CGT has experienced an interdecadal change in structure and variability after the late 1970s*[27,28,29]*. The pronounced change of CGT was observed over the North Atlantic, Europe, and Central Asia due to the enhanced coupling to North African monsoon rainfall and southward shift of upper-level westerly jet*[27,28]*. More importantly, the weakening of major CGT centers, as a response to the decreased ISMR variability*[26]*, was attributed to the much weakened ENSO–ISMR linkage during the recent period*[30,31] *(Extended Data Fig. 1; Extended Data Fig. 2a). However, it is surprised that the explained variance and predictability of CGT are even higher after the late 1970s, which implies an enhanced linkage between the ENSO development and CGT*[28,29]*. Note that the enhanced ENSO–CGT linkage cannot be explained by weakened ENSO–ISMR linkage; other processes must be explored to understand the intensity changes.*" (See Lines 84–95).

2. For clarity, what happens if you regress Z200 onto the principal component time series of the dominant EOF modes and analyze changes over the entire observation period, i.e., 1951-2020? Alternatively, what if the EOF is applied by epoch – will we see the two CGT "types" reported by Ding et al. in each epoch? If yes, wouldn't it suggest that the shift that is being reported here could better be characterized as either a predominance of the Western Pacific–North America type in the latter epoch or a weakening of the "traditional CGT" accompanied by the strengthening of the "Western Pacific–North America type"?

Reply: Thank you for the comment. As our reply to your comment 1 shows, the CGT pattern is regarded as the EOF2 mode in both periods, with the explained variability increasing from 12.2% to 14.6% (Fig. R2.2). Meanwhile, the EOF1 mode featured a consistent low over the Northern Hemisphere with the Western Pacific–North America pattern. The latter is distinct from the CGT pattern, and its explained variability changes from 20.5% to 21.7%. The Western Pacific–North America and CGT patterns can be well separated by the EOF analysis because the explained variance of the former is much larger than the latter during the whole period or any sub-periods.

3. The authors have provided a clearer analysis of the linkages between ENSO diversity and the apparent CGT shift which represents an addition to the literature. However, Ding et al. (2011) also related the two CGT “types” they identified to ENSO. Therefore, a discussion contrasting these findings with those of the present study seems warranted to, fill the knowledge gap, if any, and to facilitate the progression of the science.

Reply: Thank you for the comment. As you pointed out, Ding et al. (2011) clarified that the western Pacific–North America pattern tends to occur in summers following ENSO peak phases, while the CGT pattern appears in summers preceding the peak phases of ENSO, in association with Indian monsoon rainfall anomalies. Although Ding et al. (2011) mentioned that a zonally symmetric seesaw pattern across the Northern Hemisphere may appear even in cases with average Indian monsoon rainfall, they did not discuss the possibility that ENSO directly triggers the CGT.

Following your suggestion, we have rewritten the new findings with this study in the Introduction: “*Previous works has systematically analyzed several summertime extratropical teleconnections triggered by ENSO*[34,35]*. However, although Ding et al. (2011)*[21] *have mentioned the occurrence of the zonally symmetric seesaw pattern across the Northern Hemisphere even under the cases with normal ISMR, few study has discussed the possibility that ENSO directly triggers the CGT. Also, previous studies have indicated that the ENSO itself has also exhibited an interdecadal change in the spatial pattern, asymmetry, amplitude, and periodicity*[32,33]*. The possible role of ENSO’s change in the recent enhanced ENSO–CGT linkage needs to be answered.*” (See Lines 96–103).

4. Generally, I would recommend that the authors employ the maximum covariance analysis (MCA) method to more accurately define these patterns, as demonstrated in Wang et al. (2012) and Ding et al. (2011), rather than relying on EOFs. The use of EOF to define CGT has significant limitations, especially in GCMs, where the

“traditional CGT” and the “Western Pacific–North America” type can appear as any of the first four EOF modes.

Reply: Thank you for the suggestion. According to your comment, we have replotted the figures using the MCA method in the revised manuscript (Fig. R2.3), and the main conclusions are almost unchanged. The correlation coefficient of the CGT intensity defined by the EOF and MCA analysis is +0.70, exceeding 99.9% confidence level during the 1951–2020. Considering that the MCA analysis is more involved in rainfall factors, the CGT seems to appear as the EOF2 mode in both observations and GCMs stably (Lee and Ha, *JC*, 2019, https://doi.org/10.1175/JCLI-D-15-0154.1), here we remain to use EOF to define CGT for consistency.

To address your concern, we have discussed the CGT definition in the Methods: “*Commonly, the CGT can be defined as the second EOF mode (EOF2) of the summertime Northern Hemisphere (NH) 200-hPa geopotential height or the first maximum covariance analysis (MCA) mode of the tropical rainfall and NH 200-hPa geopotential height*[16]*. In this study, the EOF analysis is applied to identify the CGT, and the main conclusions remain unchanged when using the MCA analysis.*” (See Lines 419–424).

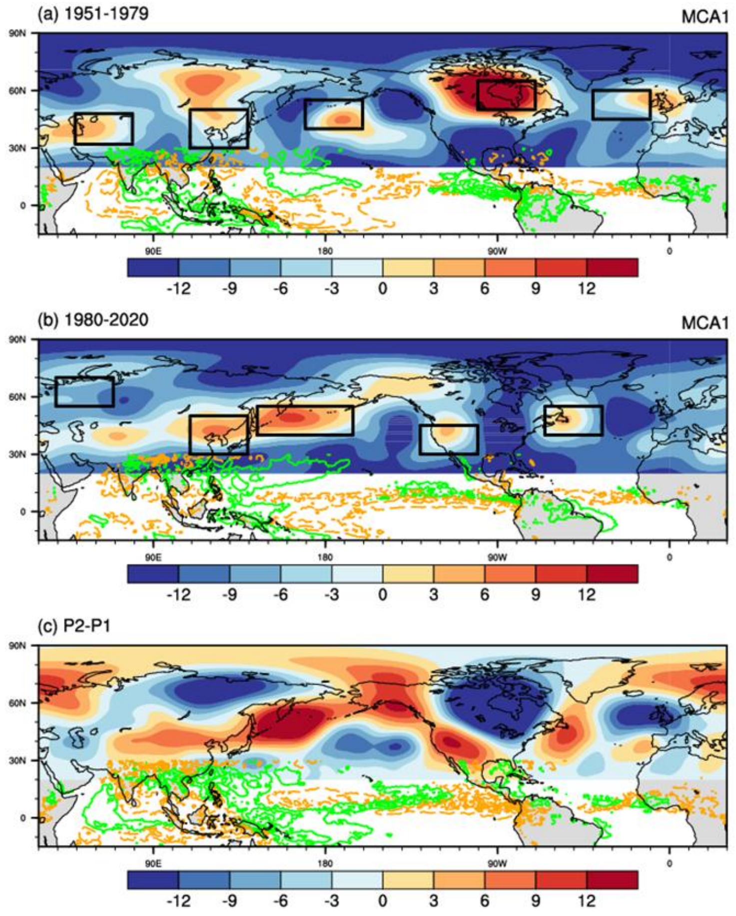


Fig. R2.3: Same as Fig. 1, but for the CGT and tropical rainfall defined by the MCA1 mode during each period.

5. Extended Data Figure 1 shows that the relationship between Nino3.4 and ISMR, as well as between ISMR and CGT, became weaker after 1980. However, during this latter epoch, the CGT still shows a connection to ENSO (Nino 3.4). If we are observing two modes, they should have distinct underlying mechanisms. For instance, some studies, such as Lin et al. (2023), suggest that the "traditional CGT type" is more directly related to Indo-Western Pacific SST variability but is indirectly related to ENSO, hence the significant correlation with ENSO as well (https://doi.org/10.3389/feart.2023.1033789).

Reply: Thank you for the improvement. According to your comment, we agree that the traditional CGT is more directly related to Indo-Western Pacific SST variability than ENSO, and thus cite this reference in the revised manuscript: "*Moreover, pacemaker simulations from the Community Earth System Model (CESM1) revealed that the CGT pattern is more directly related to sea surface* *temperature (SST) variability in the Indo-Western Pacific than the eastern tropical Pacific, suggesting another indirect pathway of the ENSO-CGT linkage*[26]." (See Lines 75–79).

6. I would interpret Figs 6a and 6b as showing differences in amplitude and not necessarily distinct pattern changes or shifts. Both look like Fig. 1b, although the expectation is that one would look like Fig. 1a and the other, Fig. 1b. Is this model inability to simulate the "traditional CGT pattern" (as in Fig. 1a)? Or does this suggest, there is no direct linkage between this CGT type and ENSO but perhaps an indirect link as suggested Lin et al. 2023?

Reply: Thank you for the comment. As you pointed out, the numerical experiments primarily represent an enhanced amplitude, and the indirect linkage between ENSO and CGT during the former period fails to be reproduced due to the model limitations. In fact, both the CESM2 pacemaker and LE experiment can reproduce the "traditional CGT pattern" in response to the ISMR (Fig. R2.4). Thus, the model can simulate the CGT's direct linkage to ISMR rather than the indirect linkage to ENSO.

Following your suggestion, we have rewritten this part in the revised manuscript: "*In comparison, the forced patterns show much weaker amplitude in both the pacemaker experiment before the late 1970s (Fig. 6a vs. b) and the eastern Pacific type experiment (Fig. 6d vs. e). Although the traditional CGT pattern during the former period fails to be reproduced, the models can simulate it when regressed onto the ISMR in both experiments (Extended Data Fig. 9). The above results support our conclusion that the linkage between ENSO and CGT during the former period is indirect.*" (See Lines 282–288).

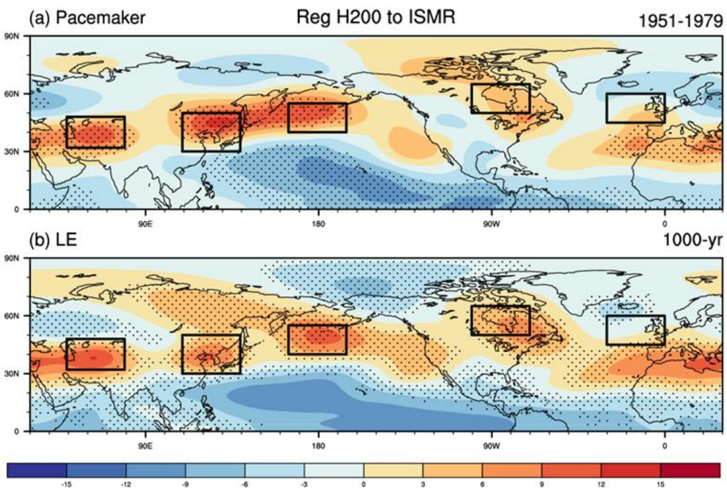


Fig. R2.4 (Extended Data Fig. 9 in the revised manuscript): Numerical experimental results in response to ISMR.

200-hPa geopotential height (shading; gpm) anomalies regressed onto the ISMR in the CESM2 (a) Pacific pacemaker experiment during the 1951–1979 period and (b) LE experiment during 1000 years. Stippling represents the shading values exceeding the 95% confidence level. The boxes refer to the locations of the observed action centers of CGT during the 1951–1979 period.

7. Fig 6: is there a rationale for using regression to construct Figs 6a and 6b but composites in Figs 6d and 6e?

Reply: Thank you for raising the concern. The two clusters of ENSO are much consistent with the leading two EOF modes of the tropical Pacific SST in 1000-year LE experiment, respectively (Fig. R2.5). However, the EOF modes seem to overestimate and underestimate the observed westward-shifted and eastward-shifted ENSO, respectively. Instead, the two clusters of ENSO can represent the Eastern Pacific type and Central Pacific type well via the composite analysis, which should be more sound than the regressed analysis, especially at a large sample of 1000 years.

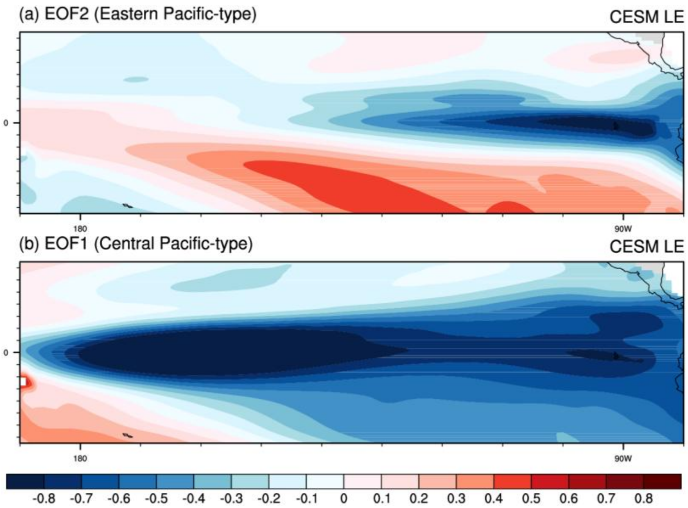


Fig. R2.5: ENSO classification in the pre-industry LE experiment.

(a) The EOF2 modes of tropical Pacific SST anomalies (Eastern Pacific type). (b) The EOF1 modes of tropical Pacific SST anomalies (Central Pacific type).

8. The designed experiments are an important addition. However, I find the placement of the relative vorticity forcings in the baroclinic model quite confusing. Based the baroclinic structures presented in Figs 4c, 4d, and Fig. 7, the CGT pattern in recent decades is induced over the TWNP. In that case, shouldn't the location of the relative vorticity be in the vicinity of 10°-20°N or the tropical region? If not, how, then, can we directly attribute causality in this context? The experimental setup is not described particularly well so I guess I am a little confused.

Reply: Thank you for raising the concern. We designed this experiment to prove that the half-wavelength westward movement of the disturbance should result in the half-wavelength westward movement of CGT centers downstream of the North Pacific during the recent epoch. Thus, we placed the negative relative vorticity forcings of the disturbances over the former and recent locations of the North Pacific center for comparisons.

Following your suggestion, we have already tried adding negative heating forcings over the TWNP in LBM to represent the baroclinic structure (Fig. R2.6b). As expected, a high is triggered to its north at the exit region of the East Asian westerly jet. For the former period, positive heating forcings over India representing the ISMR also can trigger the West Asia and East Asia high in LBM (Fig. R2.6a). However, the Rossby wave response to these disturbances in both experiments has large biases, because the LBM is a highly linearized simple model unable to reflect the impacts of the nonlinear eddy. Therefore, we use the complicated CESM2 to verify the mid-latitude teleconnection in response to tropical forcing.

To address your concern, we have briefly explained it in Methods: "*The LBM is an ideal model based on the primitive equations linearized at a given state. Two dry experiments are designed to verify the impact of the position changes of upstream disturbances on the downstream propagation of the CGT structure. The EXP-WP experiment is forced by an anticyclonic vorticity anomaly over mid-latitude western Pacific (Extended Data Fig. 10b), representing the disturbances on jets associated with westward-shifted ENSO during the recent period. In comparison, the EXP-CP experiment is forced by the same prescribed relative vorticity, but over the central Pacific consistent with the North Pacific CGT center during the former period (Extended Data Fig. 10a).*" (See Lines 392–400).

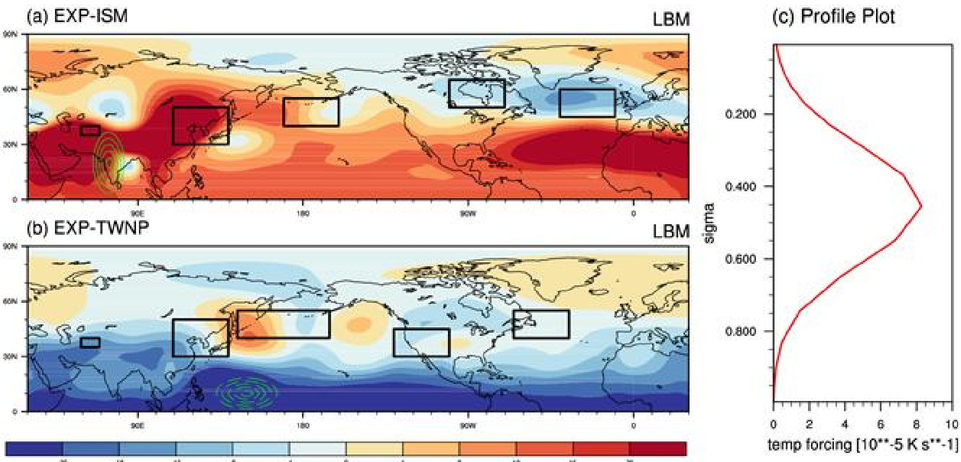


Fig. R2.6 (Extended Data Fig. 10 in the revised manuscript): The LBM experiment forced by anomalous heating.

200-hPa geopotential height (shading; gpm) anomalies in response to the prescribed heating and its horizontal distribution (green contours with the solid lines positive and dotted lines negative; 10^{-5} K s^{-1}) in (a) EXP-ISM and (b) EXP-TWNP. (c) The vertical profile of the prescribed relative vorticity (10^{-5} K s^{-1}) at the maximum center. The boxes refer to the locations of the observed action centers of CGT.

9. The authors utilized the NOAA PREC 2.5-degree data. So, I was surprised to see the level of detail depicted in Extended Fig. 5 and out of curiosity I attempted to replot (see figure below) and got something different, especially the difference plot. What could I be missing? Since the location of the suppressed convection and rainfall over the TWNP is crucial to driving the tropical-extratropical teleconnection, it would

be great if the authors would test different precipitation datasets for consistency. Alternatively, using velocity potential fields may better illustrate the convection anomalies over the TWNP. What's more, my attempts to replicate Extended Fig. 5 showed convection anomalies rather than suppression over the TWNP.

Reply: Thank you for pointing out our mistake. In fact, NOAA PREC data is used to calculate the ISMR consistent with previous works (e.g. Ding et al. 2011), while the global rainfall data in Fig. 3 and Extended Fig. 5 is based on ERA5 (Fig. R2.7), which was not clarified clearly in the original manuscript.

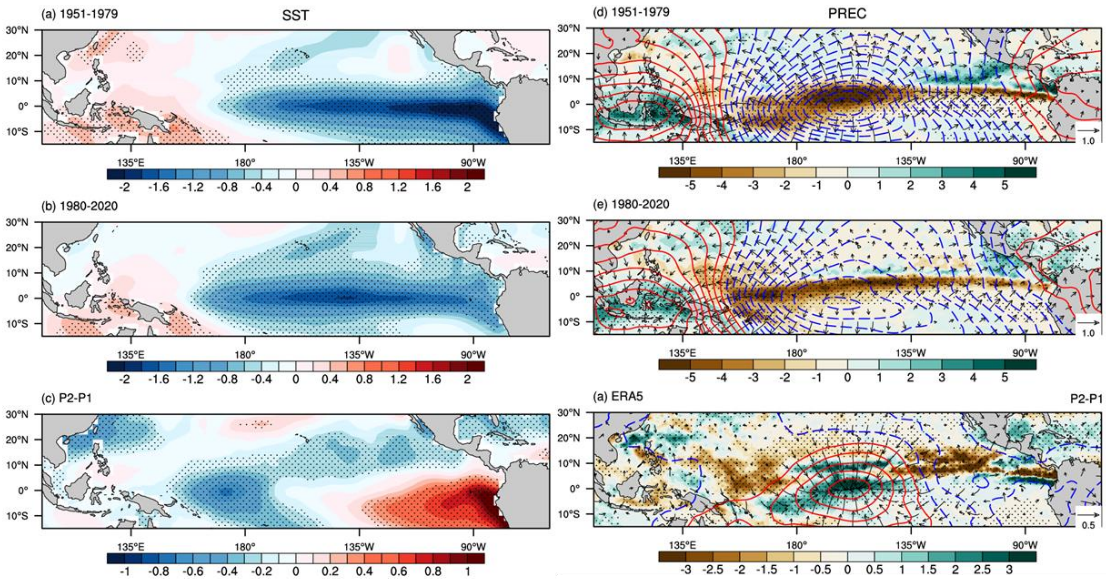


Fig. R2.7 (Fig. 3 in the revised manuscript): The recent change in the summertime ENSO.

Composite differences of SST (shading; K) anomalies between the summertime La Niña (Niño 3.4 indices < –0.5) and El Niño (Niño 3.4 indices > 0.5) years during the (a) 1951–1979 period (P1), (b) 1980–2020 period (P2), and (c) their differences (P2–P1). (d-f) Same as (a-c), but for the rainfall (shading; mm day^{-1}), 850-hPa velocity potential (contours at the interval of $2*10^{-5}$ m^2 s^{-1}; red solid contours are positive and blue dotted contours are negative), and 850-hPa divergence wind (vectors; m s^{-1}). Stippling represents the shading values exceeding the 95% confidence level.

When using the NOAA PREC, we get a similar result with your plot. Due to lack of reliable rainfall data over the ocean before the late 1970s, we have replotted 1980–2020 period by using ERA5, NOAA PREC and GPCP to compare their accuracy over the TWNP (Fig. R2.8). We find that ERA5 is more consistent with GPCP during 1980–2020 period (Fig. R2.8c). In NOAA PREC, the dry anomaly over the TWNP is located south of 10°N, inconsistent with the anomalous TWNP anticyclone around 10°N–20°N (Fig. R2.8b). Moreover, the variability of the enhanced rainfall over the Maritime Continent and the suppressed rainfall over the equatorial Pacific is too weak in NOAA PREC. In the plot of P2 minus P1, the circulation anomalies are also inconsistent with rainfall anomalies, because the enhanced rainfall shouldn't occur with the abnormal divergence over the TWNP around 10°N–20°N (Fig. R2.9b). These results may explain the bias of the TWNP convection anomalies in NOAA PREC in P2 minus P1. When repeated by NCEP/NCAR reanalysis beginning from 1948, the difference between the two periods also shows a suppressed rainfall over the TWNP (Fig. R2.9c). Thus, we argued that the ERA5 data with a higher resolution should be more reliable for the tropical Pacific rainfall covering the 1951–2020 period.

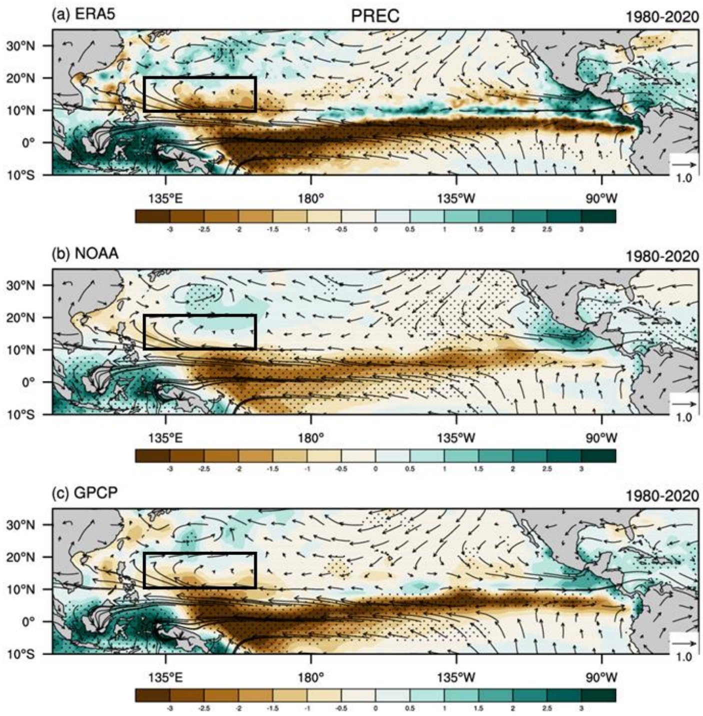


Fig. R2.8 (Extended Data Fig. 13 in the revised manuscript): Same as Fig. R2.7e, but for 850-hPa wind in ERA5 and rainfall in (a) ERA5, (b) NOAA PREC and (c) GPCP during the 1980–2020 period (P2).

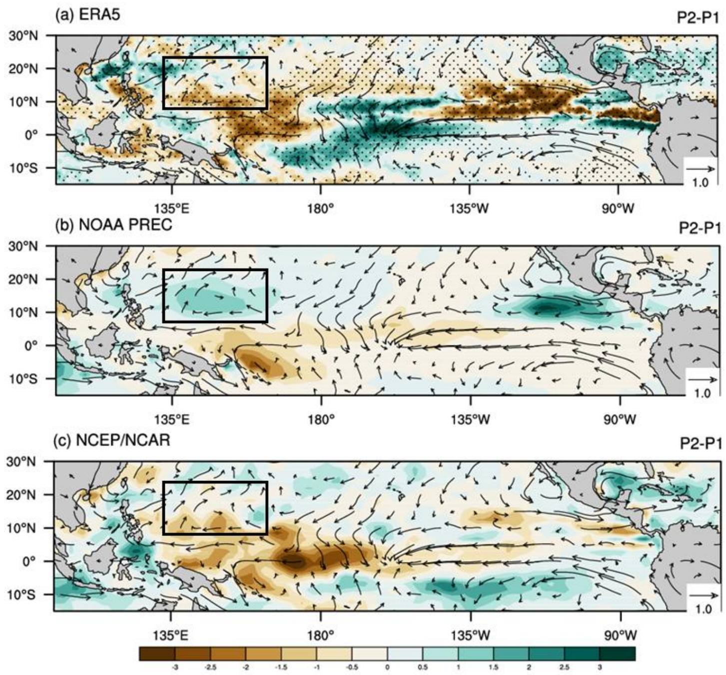


Fig. R2.9: Same as Fig. R2.7f, but for 850-hPa wind in ERA5 and rainfall in (a) ERA5, (b) NOAA PREC and (c) GPCP in 1980–2020 period (P2) minus 1951–1979 period (P1).

Following your suggestion, we have also replaced 850-hPa winds with 850-hPa velocity potential and divergent winds in Fig. 3 (Fig. R2.8). Also, we have clarified the use of global rainfall based on ERA5 in Methods: "*The observed monthly rainfall data derived from the National Oceanic and Atmospheric Administration (NOAA) Precipitation Reconstruction (PREC) with a horizontal resolution of 2.5°×2.5°, which merges the observed rainfall over land, is used for the calculation of the ISMR. The global geopotential height, wind, rainfall and surface air temperature are given by the European Center for Medium-Range Weather Forecasts Reanalysis 5 (ERA5), with a 0.25°×0.25° horizontal resolution.The selection of global rainfall is due to the fact that ERA5 is more consistent with Global Precipitation Climatology Project (GPCP) than NOAA PERC during 1980–2020 period (Extended Data Fig. 13)*" (See Lines 358–366).

Response to Reviewer 3

Summary:

This paper delves into the evolution of circumglobal teleconnection (CGT) in summer over the decades. Specifically, it analyses the changes that have occurred when comparing the years before and after the 1970s. The authors analyze the reasons for the change, studying the relationships with El Niño Southern-Oscillation (ENSO) and the westerly jet. In addition, model simulations are used to verify observational hypotheses.

In my opinion the manuscript is very well organized, and the methodologies used are appropriate. In addition, the results are relevant and robust, making the paper very interesting. Therefore, I consider its publication in this journal valid.

Even so, here are some thoughts and doubts that I would like to be considered before publication.

Reply: We would like to thank the anonymous reviewer for spending valuable time reading the manuscript and providing constructive and valuable feedbacks. Per your comments, we have tried to revise the manuscript and address your concerns.

Comments:

GENERAL

1. Numerous figures show the differences in variables between periods (before and after the 1970s). In my opinion these differences should show significance to detect the areas in which these changes are statistically significant. Please include the significance in the figures: Fig.1c, Fig. 3c and f, Fig. 5a, Fig. 6c and f, Extended Data: Fig.4c and f, Fig. 5c and f, Fig. 6c and f.

Reply: Thank you for this valuable suggestion. Following your suggestion, we have shown the significance level of differences by using the Student’s t-test in the revised manuscript (See Fig. R3.1 as an example).

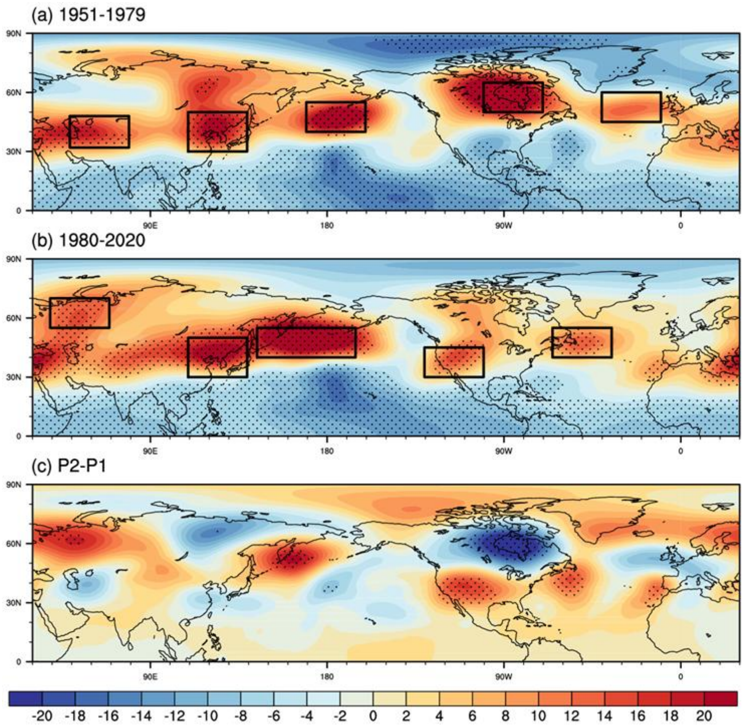


Fig. R3.1 (Fig. 1 in the revised manuscript): The recent change in the CGT structure.

200-hPa geopotential height (shading; gpm) anomalies regressed onto the PC2 of the summertime 200-hPa geopotential height over the Northern Hemisphere during the (a) 1951–1979 period (P1), (b) 1980–2020 period (P2), and (c) their differences (P2–P1). Stippling represents the shading values exceeding the 95% confidence level. The boxes refer to the locations of the observed action centers of CGT during each period.

2. Although there is a section in the manuscript that analyses the role of the jet in the spread of CGT and concludes that the placement of the jet changes between before and after the 1970s and that this may be key in the placement of the centers of action of CGT over North America and western Europe, this conclusion seems to lose relevance since it does not appear in the introduction or in the conclusions.

Reply: Thank you for this valuable suggestion. Following your suggestion, we have mentioned it in Introduction: "*The pronounced change of CGT was observed over the North Atlantic, Europe, and Central Asia, due to the enhanced coupling to North African monsoon rainfall and southward shift of upper-level westerly jet*[27,28]." (See Lines 85–88), and in Conclusion: "*Meanwhile, the interdecadal change of the subtropical westerly jet also matters: the recently enhanced East Asian westerly jet at its exit region favors a more robust Rossby response near the North Pacific center,* *whereas the southward shift of the westerly jet over North America and North Atlantic contributes to the southward movement of the CGT centers there.*" (See Lines 323–328).

INTRODUCTION

3. L77-88. In this paragraph the authors explain the changes in the CGT between before and after the 1970s. As I suggested at the beginning of my comments, I am missing at this point a paragraph or some sentence that introduces the changes in the westerly jet.

Reply: Thank you for this valuable suggestion. As we mentioned in our reply to your comment 1, we have rewritten this part in Introduction: "*The pronounced change of CGT was observed over the North Atlantic, Europe, and Central Asia, due to the enhanced coupling to North African monsoon rainfall and southward shift of upper-level westerly jet*[27,28]." (See Lines 85–88).

RESULTS

Role of the ENSO

4. L128-144 and Fig. 2. The manuscript describes Fig. 2 and the different action centres associated with ISMR and ENSO before and after the 1970s. Black boxes indicate the different centres (I supposed; see figures comments below). I have doubts regarding the North American action centre. In Fig. 2b the box is placed in the northeast, while the area of significance is placed further to the southwest. In the period after the 1970s, in Fig 2e, and even in Fig 2d, following the shading significance, this centre is in the southwest, in a place similar to where it is indicated in Fig 2f. Nevertheless, the black boxes are plotted in the northeast of North America. In my opinion, the authors should go more deeply into the location of these centres of action because they will condition the final results of the study.

Reply: Sorry for making you confused. In the original manuscript, boxes refer to the observed action centers of CGT during the 1951–1979 period (a-e) and 1980–2020 period (f) based on Fig. 1. These boxes are presented in order to make it convenient to show the individual contributions of ISMR and ENSO on the locations of CGT centers during each period. Thus, there is a possibility of center mismatch, because the ISMR exerts weak effect on the North America center in Fig. 2b, d and e.

To avoid misunderstanding, we use two types of boxes to present the CGT centers during the two period (Fig. R3.2).

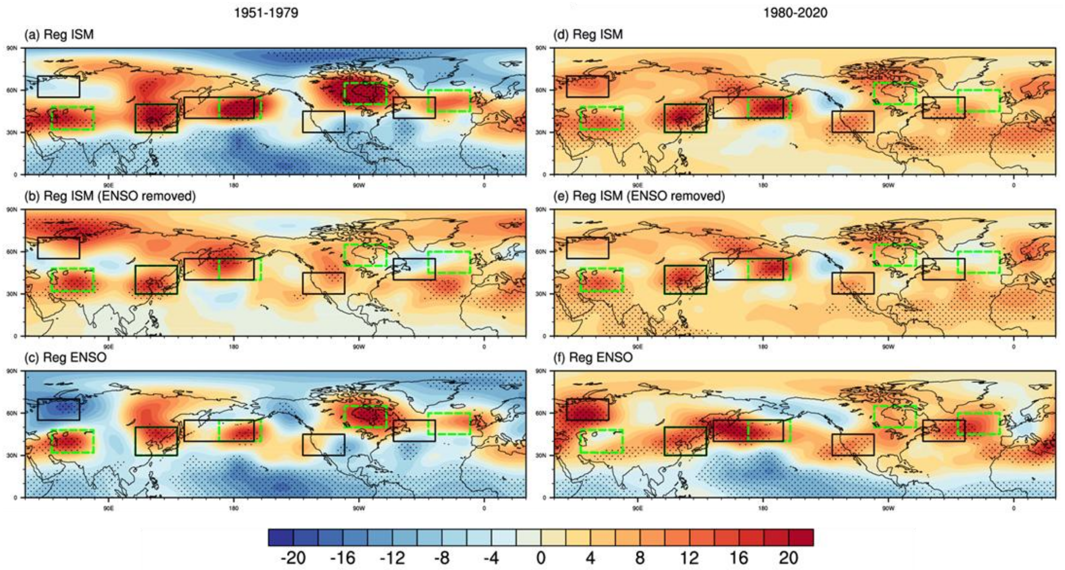


Fig. R3.2 (Fig. 2 in the revised manuscript): The recent change between ENSO and ISMR effects.

200-hPa geopotential height (shading; gpm) anomalies regressed onto the (a) ISMR, (b) ISMR with the effects of ENSO removed, and (c) inverted summertime Niño 3.4 index during the 1951–1979 period. (d–f) Same as (a–c), but for the 1980–2020 period. Stippling represents the shading values exceeding the 95% confidence level. The green dotted (black solid) boxes refer to the locations of the observed action centers of CGT during the 1951–1979 (1980–2020) period.

Role of the westerly jet

5. L220-223. The manuscript explains that there is a promoted centre over Ural Mountain, indicating to look at Fig. 5a. I cannot see this centre in the mentioned figure panel.

Reply: Thank you for pointing out this mistake. It should be Fig. 1c: "*The stronger westerly jet at the jet exit region over Europe (Fig. 5a) helps promote the occurrence of Ural Mountain center (Fig. 1c), due to the stronger barotropic instability and energy conversion there.*" (See Lines 252–254).

Numerical experiments validation

6. L237-240. In my opinion, outside the North Pacific, the rest of the centres do not fit the observations as well. Please indicate it in the manuscript. Although I agree that the similarity between model simulations and observational results is acceptable.

Reply: Thank you for the comment. We have rewritten this part: "*Similar to the observations, the half-wavelength westward movement of the North Pacific center and other centers downstream after the 1970s was successfully reproduced (Fig. 6b vs. Fig. 2f).*" (See Lines 270–272).

7. L251. "… enhanced centres over southwestern America …." and L269 "… eastern Canada to southwestern America…". In my opinion it is more correct to write "…southwestern North America…".

Reply: Revised. Thank you.

8. L249-252. "Compared…(Fig. 6d vs. e)". I see no major differences between panels (d) and (e). The significant regions are similar and correspond to the same action centres. On the other hand, it is true what the authors comment regarding the intensification of the centres over the North Pacific, North America and the North Atlantic in response to central Pacific-type ENSO. Calculating the statistical significance of the differences presented in panel (f) could help in this interpretation.

Reply: Thank you for the comment. As replied above, we have calculated the statistical significance of the differences across the revised manuscript. Following your suggestion, we have rewritten this part to underline the intensification of CGT centres in response to central Pacific-type ENSO: "*In comparison, the forced patterns show much weaker amplitude in both the pacemaker experiment before the late 1970s (Fig. 6a vs. b) and the eastern Pacific type experiment (Fig. 6d vs. e). Although the traditional CGT pattern during the former period fails to be reproduced, the models can simulate it when regressed onto the ISMR in both experiments (Extended Data Fig. 9). The above results support our conclusion that the linkage between ENSO and CGT during the former period is indirect.*" (See Lines 282–288).

CONCLUSION AND DISCUSSION

9. L289. "… westerly jet waveguide (Fig. 7b)". As I suggested at the beginning of my comments, I am missing at this point any sentence regarding the westerly jet changes and their effect on the downstream CGT centres of action.

Reply: Thank you for this valuable suggestion. Following your suggestion, we have mentioned it in Conclusion: "*Meanwhile, the interdecadal change of the subtropical westerly jet also matters: the recently enhanced East Asian westerly jet at its exit region favors a more robust Rossby response near the North Pacific center, whereas the southward shift of the westerly jet over North America and North Atlantic contributes to the southward movement of the CGT centers there.*" (See Lines 323–328).

10. L301. Extended Data Fig. 10. Anything is mentioned about panels (c) and (d). Black boxes are plotted indicating (I supposed) the action centres, but same of them do not correspond to the shading, specially the one over North America in panel (c) and the one over eastern Europe in panel (d). Please clarify it. Significance could be computed to better justify these centres.

Reply: Thank you for the comment. In this figure, boxes refer to the observed CGT centers, and thus it is possible not to correspond to the shading of the CMIP6 simulations. We first calculate the regressed pattern in each model, and then composite the results of 24 models. Therefore, it is difficult to present the significance here.

According to your comment, we have clarified the information in caption: "*Here the regressed pattern in each model is calculated, and then the results of 24 models are composited. The boxes refer to the locations of the observed action centers of CGT.*" (See Extended Data Fig. 10 caption).

METHODS

Observation data

11. Although it is indicated in the main text, I think it would be appropriate to repeat in this section the years and months (summertime) used in the study.

Reply: Thank you for the suggestion. We have mentioned it here: "*The summertime results in this study refer to the average values of June, July and August (JJA).*" (See Lines 373–375).

12. L323. "…climatological mean…". Clarify please which period is considered to calculate this mean.

Reply: Thank you for the suggestion. We have added it here: "*The anomalies are calculated by the original values minus the climatological mean during the 1951–2020 period and detrended to exclude the potential effect of global warming.*" (See Lines 371–373).

MINORS

13. L143. Please indicate the b panel of the Fig.1, Fig.1b.

Reply: Revised. Thank you.

FIGURES

14. In several figures "P1" and "P2" are used to refer to the period before and after the 1970s, respectively. Although it is quite intuitive, I think it would be convenient to indicate in the figures captions to which these acronyms correspond. Fig. 1c, Fig. 3c and f, Fig. 6c, Extended Data: Fig. 4c and f, Fig. 5c and f, Fig. 6c and f.

Reply: Thank you for the suggestion. We have indicated the meaning of these acronyms in these figures captions, such as: "*200-hPa geopotential height (shading; gpm) anomalies regressed onto the PC2 of the summertime 200-hPa geopotential height over the Northern Hemisphere during the 1951–1979 period (P1), (b) 1980–2020 period (P2), and (c) their differences (P2–P1).*" (See Fig. 1 caption).

15. Please write in the figure captions what the black boxes indicate. Fig.1a and b, Fig.2, Fig. 4c and d, Fig.6a-b and d-e, Extended Data: Fig. 9a-b, Fig. 10c-d.

Reply: Thank you for the suggestion. We have indicated the meaning of these boxes in these figures captions, such as: "*The boxes refer to the locations of the observed action centers of CGT.*" (See Fig. 1 caption).

16. Please indicate the statistical significance of the fields plotted with arrows. Fig. 3d-f, Fig. 4a-b, Fig. 5b-c, Extended Data: Fig. 5d-f.

Reply: Thank you for the comment. We have replotted these figures in the revised manuscript. For Fig.3 and Extended Data Fig. 5, the arrows of 850-hPa wind are replaced by 850-hPa velocity potential according to the comments by Reviewer 2. For Fig. 4, the arrows of 200-hPa wind are of little significance, which has been deleted for simplicity. For Fig. 5, the arrows represent the climatological-mean 200-hPa wind without considering the significance level.

17. Fig. 4. L196-199. In the caption is written "(c) averaged over 170ºE-150ºW….. (d) averaged over 130ºE-170ºE…." It means zonal averaged is not it?

Reply: Exactly. We have added "zonally" here (See Fig. 4 caption).

18. Fig. 4e-f. L201. In the caption is written "during the 1951–1979 (solid line) and 1980–2020 (dotted line)" but in the corresponding panel there is only one dotted line, no one solid. Perhaps a line is missing in each panel? Please clarify.

Reply: Sorry for this mistake. We have rewritten it in the caption: "*Zonal wind of the basic flow (e) averaged over 5°N–20°N/170°E–140°W during the 1951–1979 period, and (f) averaged over 5°N–20°N/100°E–170°E during the 1980–2020 period.*" (See Fig. 4 caption).

19. Extended Data Fig. 7. If it is not too much trouble, I think it would be nice to see these same fields but for the period after the 70s.

Reply: Thank you for the suggestion. For the climatology, it is difficult to show obvious differences before and after the late 1970s (Fig. R3.3). Thus, here we present the climatological mean during the whole period to show the overall features of the basic flow.

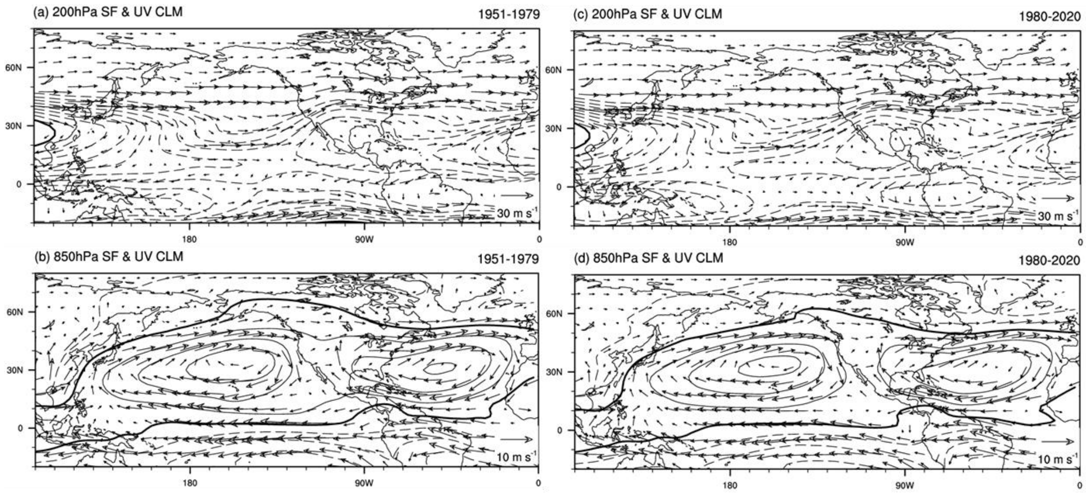


Fig. R3.3 The basic flow.

Same as Extended Data Fig. 7, but for the 1951–1979 period (left) and 1980–2020 period (right).

20. Fig. 5. In panel (a) a green line is used to represent the jet axis. But in that figure the differences in the jet between periods are shown (before and after the 70s). To which period does this axis correspond, or is it an average? Please clarify.

Reply: Thank you for the comment. We have clarified it in the caption: "*The green line represents the westerly jet axis, defined as the maximum value between 30°N–60°N of the climatological-mean 200-hPa zonal wind during the 1951–2020 period.*" (See Fig. 5 caption).

21. L224. Fig. 5. The title of the (a) panel "U200 & UV200". In the caption it is said that only the zonal wind (U200) is plotted.

Reply: Revised. Thank you for pointing out this mistake.

22. L437. Reference 17. "t1emperature extremes".

Reply: Revised. Thank you.

Suggested bibliography

23. Jong et al. (2020 and 2021) analyse the different teleconnection patterns in response to La Niña in summer, when La Niña is transitioning from El Niño and when it is a persisting La Niña, concluding that in the latter the extratropical teleconnection is insignificant. Martija-Díez et al. (2021 and 2023) analyse the differences on ENSO impacts over Europe in summer along the decades and they conclude that westerly jet characteristics are fundamental.

Jong, B. T., Ting, M., Seager, R., & Anderson, W. B. (2020). ENSO teleconnections and impacts on US summertime temperature during a multiyear La Niña life cycle. Journal of Climate, 33(14), 6009-6024. https://doi.org/10.1175/JCLI-D-19-0701.1

Jong, B. T., Ting, M., & Seager, R. (2021). Assessing ENSO summer teleconnections, impacts, and predictability in North America. Journal of Climate, 34(9), 3629-3643. https://doi.org/10.1175/JCLI-D-20-0761.1

Martija-Díez, M., Rodríguez-Fonseca, B., & López-Parages, J. (2021). ENSO Influence on Western European summer and fall Temperatures. Journal of Climate, 34(19), 8013-8031. https://doi.org/10.1175/JCLI-D-20-0808.1

Martija-Díez, M., López-Parages, J., Rodríguez-Fonseca, B., & Losada, T. (2023). The stationarity of the ENSO teleconnection in European summer rainfall. Climate Dynamics, 61(1), 489-506. https://doi.org/10.1007/s00382-022-06596-4

Reply: Thank you for the suggestion. We have carefully read these references and cited in the revised manuscript: "*The pronounced change of CGT was observed over the North Atlantic, Europe, and Central Asia, due to the enhanced coupling to North African monsoon rainfall and southward shift of upper-level westerly jet*[27,28]." (See Lines 85–88), and "*Previous works has systematically analyzed several summertime extratropical teleconnections triggered by ENSO*[34,35]*.*" (See Lines 96–97).

Code availability

24. As indicated by the authors, all calculations in this study have been produced using NCAR Command Language (NCL) version 6.4.0, and the source codes can be obtained upon request to the corresponding authors.

Reply: Revised. Thank you.

Response to Reviewer 1

Summary:

The authors have satisfactorily addressed my main concerns.

Reply: We would like to thank the anonymous reviewer for spending valuable time reading the manuscript and providing constructive and valuable feedbacks.

Remarks on code availability:

The code is not available.

Reply: Thank you for reminding us. We have uploaded the latest code on Zenodo according to the editorial requests: "*Code for the main results is available on Zenodo at https://doi.org/10.5281/zenodo.14467595.*" (See lines 384–385).

Response to Reviewer 2

Summary:

I appreciate the authors' efforts in thoroughly addressing my concerns. The revisions have significantly improved the clarity of the article. I have no further comments.

Reply: We would like to thank the anonymous reviewer for spending valuable time reading the manuscript and providing constructive and valuable feedbacks.

Response to Reviewer 3

Summary:

I would like to thank the authors for following my suggestions and kindly answering my questions. I have detected a couple of errors that need to be corrected before the article is published and I would also like to make one last suggestion.

Reply: We would like to thank the anonymous reviewer for spending valuable time reading the manuscript and providing constructive and valuable feedbacks.

Comments:

1. It was a great success to use the green dotted and black solid boxes to refer to the locations of the observed action centres of CGT in both periods. I suggest if it is not too much trouble, to use this same criterion in the Figure 1.

Reply: Thank you for this valuable suggestion. We have use this same criterion in the Figure 1 for consistency.

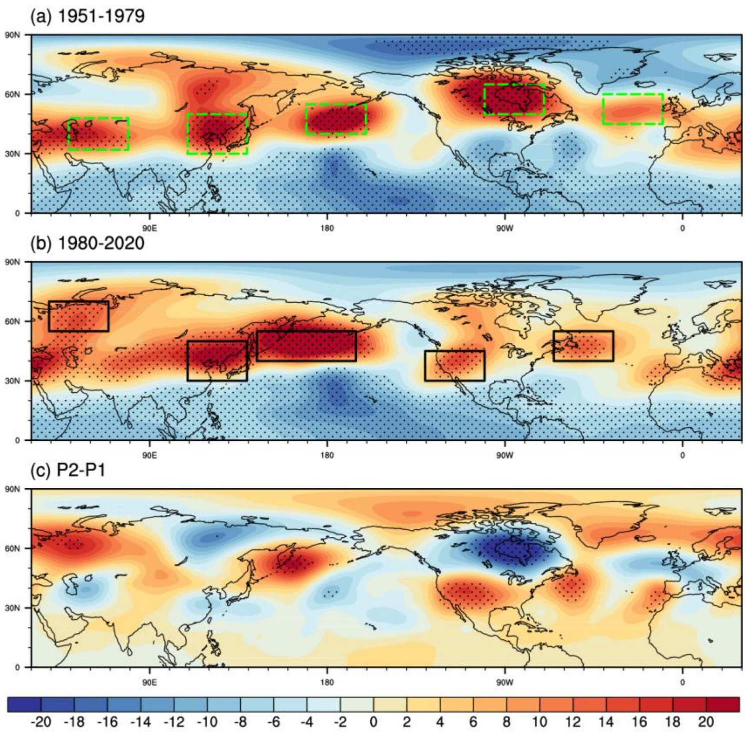


Fig. R3.1 (Fig. 1 in the revised manuscript): The recent change in the circumglobal teleconnection (CGT) structure.

200-hPa geopotential height (shading; gpm) anomalies regressed onto the PC2 of the summertime 200-hPa geopotential height over the Northern Hemisphere during the (a) 1951–1979 period (P1), (b) 1980–2020 period (P2), and (c) their differences (P2–P1). Stippling represents the shading values exceeding the 95% confidence level. The green dotted (black solid) boxes refer to the locations of the observed action centers of CGT during the 1951–1979 (1980–2020) period.

2. The title of the Fig 5a, is still wrong. Correct it please.

Reply: Revised. Thank you for pointing out this mistake.

3. Thank you for considering my bibliographical suggestion. However, I think there was some mistake. I cannot find any references to Martija-Díez et al. (2021, 2023) in the bibliography and I think that Jong et al. (2020, 2021) are not entered in the correct bibliography number. Please correct this.

Reply: Thank you for pointing out this mistake. We have rewritten these parts in the revised manuscript: "*Previous studies have extensively examined how the ENSO affects summertime extratropical teleconnections and their stability*[21,27,32,33]*, particularly during the transition from El Niño to La Niña, which significantly triggers Rossby wave propagation and warming anomalies over the central and western regions of North America*[34,35]" (See Lines 95–99).

Overview

The authors report that the boreal summer circumglobal teleconnection (CGT) has shifted westward by a half-wavelength since the late 1970s, intensifying heatwaves and droughts in East Europe, East Asia, and southwestern America. They attribute this shift to changes in the dynamics of the El Niño-Southern Oscillation (ENSO) and support their assertion with both empirical and modeling evidence. The subject is relevant and would be of interest to the climate research community. However, I have concerns regarding the originality and novelty of the study, especially considering the level of Nature Communications. Much of the content appears to have been covered in previously published work, including Wang et al., 2012 https://doi.org/10.1029/2012GL052371) and Ding et al., 2011 (https://doi.org/10.1175/2011JCLI3621.1), and is not necessarily breaking new ground. But of course, I stand to be corrected. I have provided specific comments below.

Specific Comments

1. The observed change in the summer CGT pattern appears to align with the two CGT types (i.e., the traditional type and the Western Pacific–North America type) or "flavors" identified by Ding et al. (2011) in a similar analysis spanning 1948–2009. We may all agree it is one thing to say there exist two distinct modes (i.e., with distinct underlying mechanisms, spatial patterns, statistical characteristics, etc.) varying on both interannual and interdecadal timescales versus a single mode with temporal changes over time (i.e., consistent spatial pattern, mechanisms, similar statistical signature but with temporal changes). It appears the authors are referring to a singular mode, given that either half of the PC(2) time series is used to represent the CGT patterns in the two epochs. For two naturally occurring interannual modes of CGT variability, I would expect to see two separate time series from independent EOF modes, as demonstrated in Ding et al. (2011).

   As the authors rightly note, Wang et al. (2012) later identified an interdecadal change in the CGT occurring around the late 1970s, which is effectively the same shift being reported in the present study. Given these similarities, how do the authors reconcile or discriminate their results from those reported in the aforementioned studies? What new insights or contributions does this study provide beyond confirming the previously identified patterns and/or shifts? I think this is an important point to clarify.

2. For clarity, what happens if you regress Z200 onto the principal component time series of the dominant EOF modes and analyze changes over the entire observation period, i.e., 1951-2020? Alternatively, what if the EOF is applied by epoch – will we see the two CGT "types" reported by Ding et al. in each epoch? If yes, wouldn't it suggest that the shift that is being reported here could better be characterized as either a predominance of the Western Pacific–North America type in the latter epoch or a weakening of the "traditional CGT" accompanied by the strengthening of the "Western Pacific–North America type"?

3. The authors have provided a clearer analysis of the linkages between ENSO diversity and the apparent CGT shift which represents an addition to the literature. However, Ding et al. (2011)

also related the two CGT “types” they identified to ENSO. Therefore, a discussion contrasting these findings with those of the present study seems warranted to, fill the knowledge gap, if any, and to facilitate the progression of the science.

4. Generally, I would recommend that the authors employ the maximum covariance analysis (MCA) method to more accurately define these patterns, as demonstrated in Wang et al. (2012) and Ding et al. (2011), rather than relying on EOFs. The use of EOF to define CGT has significant limitations, especially in GCMs, where the “traditional CGT” and the “Western Pacific–North America” type can appear as any of the first four EOF modes.

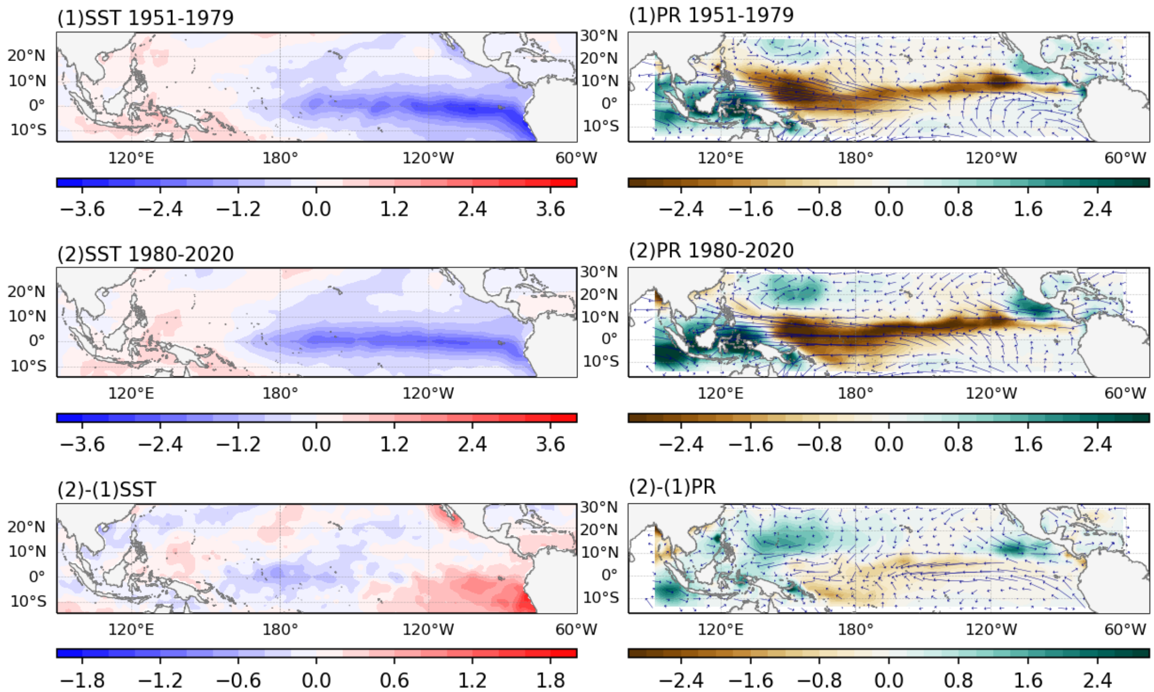
5. Extended Data Figure 1 shows that the relationship between Nino3.4 and ISMR, as well as between ISMR and CGT, becomes weaker after 1980. However, during this latter epoch, the CGT still shows a connection to ENSO (Nino3.4). If we are observing two modes, they should have distinct underlying mechanisms. For instance, some studies, such as Lin et al. (2023), suggest that the "traditional CGT type" is more directly related to Indo-Western Pacific SST variability but is indirectly related to ENSO, hence the significant correlation with ENSO as well (https://doi.org/10.3389/feart.2023.1033789).

6. I would interpret Figs 6a and 6b as showing differences in amplitude and not necessarily distinct pattern changes or shifts. Both look like Fig. 1b, although the expectation is that one would look like Fig. 1a and the other, Fig. 1b. Is this model inability to simulate the “traditional CGT pattern” (as in Fig. 1a)? Or does this suggest, there is no direct linkage between this CGT type and ENSO but perhaps an indirect link as suggested Lin et al. 2023?

7. Fig 6: is there a rationale for using regression to construct Figs 6a and 6b but composites in Figs 6d and 6e?

8. The designed experiments are an important addition. However, I find the placement of the relative vorticity forcings in the baroclinic model quite confusing. Based the baroclinic structures presented in Figs 4c, 4d, and Fig. 7, the CGT pattern in recent decades is induced over the TWNP. In that case, shouldn’t the location of the relative vorticity be in the vicinity of 10°-20°N or the tropical region? If not, how, then, can we directly attribute causality in this context? The experimental setup is not described particularly well so I guess I am a little confused.

9. The authors utilized the NOAA PREC 2.5-degree data. So, I was surprised to see the level of detail depicted in Extended Fig. 5 and out of curiosity I attempted to replot (*see figure below*) and got something different, especially the difference plot. What could I be missing? Since the location of the suppressed convection and rainfall over the TWNP is crucial to driving the tropical-extratropical teleconnection, it would be great if the authors would test different precipitation datasets for consistency. Alternatively, using velocity potential fields may better illustrate the convection anomalies over the TWNP. What’s more, my attempts to replicate Extended Fig. 5 showed convection anomalies rather than suppression over the TWNP.



(1)SST 1951-1979
(1)PR 1951-1979
(2)SST 1980-2020
(2)PR 1980-2020
(2)-(1)SST
(2)-(1)PR
30°N
20°N
10°N
0°
10°S
120°E
180°
120°W
60°W
−3.6 −2.4 −1.2 0.0 1.2 2.4 3.6
−2.4 −1.6 −0.8 0.0 0.8 1.6 2.4
−1.8 −1.2 −0.6 0.0 0.6 1.2 1.8