

Peer Review File

What Compels Future Tropical Cyclone Genesis Pattern and Frequency



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Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

23rd Nov 21

Dear Dr Murakami,

Your manuscript titled "What Compels Future Tropical Cyclone Genesis Pattern and Frequency" has now been seen by three reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Specifically, for publication in Communications Earth & Environment to be appropriate, we will need you to

- * present novel insights on future patterns and mechanisms of tropical cyclone genesis, based on an analysis of up-to-date climate model simulations.
- * discuss your findings more deeply in the context of the existing literature (we endorse reviewer 3's suggestion to start with the relevant parts of the IPCC report, but please go beyond that, and include studies from the Southern Hemisphere where relevant and appropriate)
- * provide details on your methodology to a level that will allow an independent research to replicate your results.

Should additional work allow you to meet these editorial thresholds, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

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

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

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

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

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

[link redacted]

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

Best regards,

Heike Langenberg, PhD

Chief Editor
Communications Earth and Environment

On Twitter: @CommsEarth

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript “What Compels Future Tropical Cyclone Genesis Pattern and Frequency” uses a newly developed tropical cyclone genesis (TCG) index, named DGPI, to investigate TCG response to global warming. The results are compared with the traditional index ENGPI and improvements have been found particularly in the warming climate. The study highlights the importance of mid-level upward motion, low-level cyclonic vorticity and vertical wind shear while downplays the role of maximum potential intensity, which is a crucial factor in ENGPI. It is an interesting study and can help us better understand the control factors of TCG.

Major comments:

1. To compare DGPI and ENGPI in the current climate, the authors show global climatology maps and correlation of Interannual variability in Fig. 1. I'm also curious about the performance of annual cycle over each basin. A recent study of Yang et al. 2021 shows that the annual cycle of TCG over individual basins can be better understood by combining both TC genesis probability and TC seeds. I'm wondering if the TCG annual cycle is also better described by DGPI than ENGPI.

2. Regarding the fractional future changes (Fig. 3a and b), it would be great to also show the scatter plot of DGPI vs. TCG as well as ENGPI vs. TCG. If DGPI is really a good index for TCG, we expect to see some apparent linear relationship in the scatter plot of DGPI vs. TCG. This will provide another piece of convincing evidence of the superiority of DGPI over ENGPI.

3. While it is good to build a better TCG index to help understand TCG variability, it is also of great value to learn what the mechanic processes are behind. For example, why is vertical motion an important factor in controlling TCG? Is it because vertical velocity is crucial for pre-TC seeds as proposed in Hsieh et al. 2020? I'd like to hear more such discussion on the possible physics behind the choice of variables in DGPI.

4. Besides ENGPI, another widely used TCG index is TCGI (Tippett et al. 2011). How does DGPI perform compared with TCGI?

References:

Hsieh, T.L., Vecchi, G.A., Yang, W. et al. Large-scale control on the frequency of tropical cyclones and seeds: a consistent relationship across a hierarchy of global atmospheric models. *Clim Dyn* 55, 3177–3196 (2020). <https://doi.org/10.1007/s00382-020-05446-5>

Tippett, M. K., Camargo, S. J., & Sobel, A. H. (2011). A Poisson Regression Index for Tropical Cyclone Genesis and the Role of Large-Scale Vorticity in Genesis, *Journal of Climate*, 24(9), 2335-2357.

Yang, W., Hsieh, T.-L., and Vecchi, G. A. (2021): Hurricane annual cycle controlled by both seeds and genesis probability, *Proceedings of the National Academy of Sciences*, 118, <https://doi.org/10.1073/pnas.2108397118>.

Reviewer #2 (Remarks to the Author):

Recommendations: Major revision

The topic of this study is very interesting, which would provide a way to understand future changes in TC genesis frequency and distribution. However, I have a few comments on this manuscript.

Major comments

1) Please authors clarified which season was used to analyze. If it is the annual mean, the results of this study will be subject to large uncertainty and remain confused in terms of significant distinct differences for different seasons. If it is the peak TC season, please specify it.

2) I am confused that the relative importance of each large-scale environmental factor included in the two GPIs is inferred by their pattern correlations. It is because the pattern correlations do not

consider their amplitudes, just simply reflect the consistency for the spatial distribution.

3) As the authors said, the DGPI is better to project future changes of TC genesis and inter-annual variations of TC genesis on the global scale from observations than ENGPI. However, the authors did not show the inter-annual correlation between two GPIs and tracked TC genesis frequency over the globe and various basins for these climate models. How is the representation of tracked TC genesis in all GCMs for these two GPIs?

4) This study just presents the results of TC genesis distribution from only one GCM model (MRI-GCM). I think that authors should show the results of TC genesis distribution from the ensemble-mean of all the considered GCM models at least.

5) Authors suggested that an unchanged or a moderate decrease of TC genesis frequency over the global scale based upon the DGPI and tracked TC genesis in a warming climate, significantly different from a significant increase of TC genesis frequency using the ENGPI. In fact, from the analyses, the DGPI is mainly dominated by the changes of vertical wind shear and 500hPa omega, while the ENGPI is largely controlled by the changes of vertical wind shear and 600hPa relative humidity. Also mentioned in this study, the 500hPa omega is highly correlated with the 600hPa relative humidity (~ 0.84), how to cause distinct different future changes of TC genesis frequency on the global scale using these two GPIs. Why? Additionally, I guess that the significant difference between these two GPIs may be due to the distinct difference of TC genesis frequency over the specific basin.

6) Personally, I want to know how to calculation of the DGPI in detail. One of the researchers from my group attempted to use this DGPI to reproduce the TC genesis as shown in this study, but it is failed. Does have any special processes for its calculation?

Reviewer #3 (Remarks to the Author):

This is a very brief paper exploring tropical cyclone genesis pattern and frequency in future decades, using an ensemble of multiple model outputs. In principle it makes a contribution to knowledge, particularly through presenting the outputs of the range of models and comparisons thereof.

Given the long-standing and recently growing output on tropical cyclone dynamics and model outputs, greater emphasis needs to be placed on justifying the necessity and novelty of this study.

The study does not engage sufficiently with the wider range of literature on tropical cyclones and particularly tropical cyclone genesis and conditions for formation. There are model outputs from the global South (eg. the work of Reason, Abiodun, Malherbe, Engelbrecht) which have been neglected, and work on tropical cyclone dynamics from historical data. While papers to this journal are shorter in format, this engagement with the literature is important in justifying the importance of this study and critically engaging with what has been done.

Engagement with the IPCC and their emissions scenarios should be to the 6th Assessment Report (2021) and not to the 2007 report - each IPCC report replaces the one before it, and they have moved to quite different emissions scenarios. Indeed engagement with the IPCC 6th AR discussions

on tropical cyclones is much needed.

It would be good to see more on the verification of model outputs against historical tropical cyclone records - from the databases such as IBTraCs rather than the proportionally larger focus on the comparison between model outputs.

This paper requires language editing, and given the generalist audience of this journal, a strong move away from such extensive use of jargon and acronyms.

Response to comments by Reviewer 1:

We thank the reviewer for his/her insightful and encouraging comments and suggestions. We have carefully revised the manuscript according to the reviewer's suggestions and comments, as detailed below in our point-by-point response. Note that we replaced the CMIP3 A1B scenario by the MRI-AGCM3.2S with the CMIP5 RCP8.5 scenario. We also included CMIP6 model results in the supplement to show consistency of the results among the future emission scenarios.

Response to major comment:

1. *To compare DGPI and ENGPI in the current climate, the authors show global climatology maps and correlation of Interannual variability in Fig. 1. I'm also curious about the performance of annual cycle over each basin. A recent study of Yang et al. 2021 shows that the annual cycle of TCG over individual basins can be better understood by combining both TC genesis probability and TC seeds. I'm wondering if the TCG annual cycle is also better described by DGPI than ENGPI.*

Thank you for this comment. We also compared the TCG annual cycle between the two GPIs (Fig. R1). We found that the two GPIs show comparable and reasonable skill in reproducing the observed annual cycle of TCG for most of the ocean basins except for the North Indian Ocean. We have added this figure in the revised manuscript along with a description in the main text as follows.

Lines 107–109: “For the annual cycle of TCG frequency in each ocean basin, the two GPIs show comparable and reasonable skill except for the North Indian Ocean.”

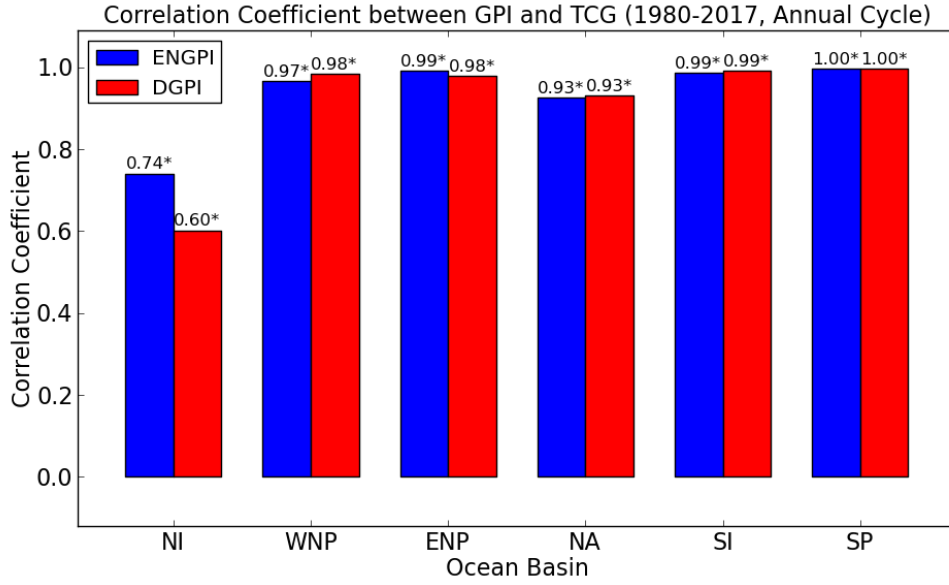


Fig. R1. Correlation coefficients between the observed TCG number and the TCG potential using ENGPI (blue) and DGPI (red) computed for the 12-month climatological monthly values in the six ocean basins: North Indian Ocean (NI); western North Pacific Ocean (WNP); eastern North Pacific Ocean (ENP); North Atlantic Ocean (NA); South Indian Ocean (SI); and South Pacific Ocean (SP).

2. *Regarding the fractional future changes (Fig. 3a and b), it would be great to also show the scatter plot of DGPI vs. TCG as well as ENGPI vs. TCG. If DGPI is really a good index for TCG, we expect to see some apparent linear relationship in the scatter plot of DGPI vs. TCG. This will provide another piece of convincing evidence of the superiority of DGPI over ENGPI.*

Thank you for the comment. The scatter plots are shown in the Fig R2. The correlation coefficient between ENGPI and TCG is -0.47 , indicating that ENGPI tends to show opposite sign of the future changes relative to the TCG changes. Indeed, among the 30 simulations, there are only 7 (23%) of the simulations in which ENGPI shows the same sign of the projected future changes relative to the TCG changes. Although the correlation coefficient

between DGPI and TCG is low (+0.04), there are 17 (57%) simulations show the same sign of the changes between DGPI and TCG. Unfortunately, DGPI still fails to capture the quantitative changes in TCGs (i.e., a simulation with a larger decrease (or increase) of DGPI does not always show a larger decrease (or increase) in TCG.)

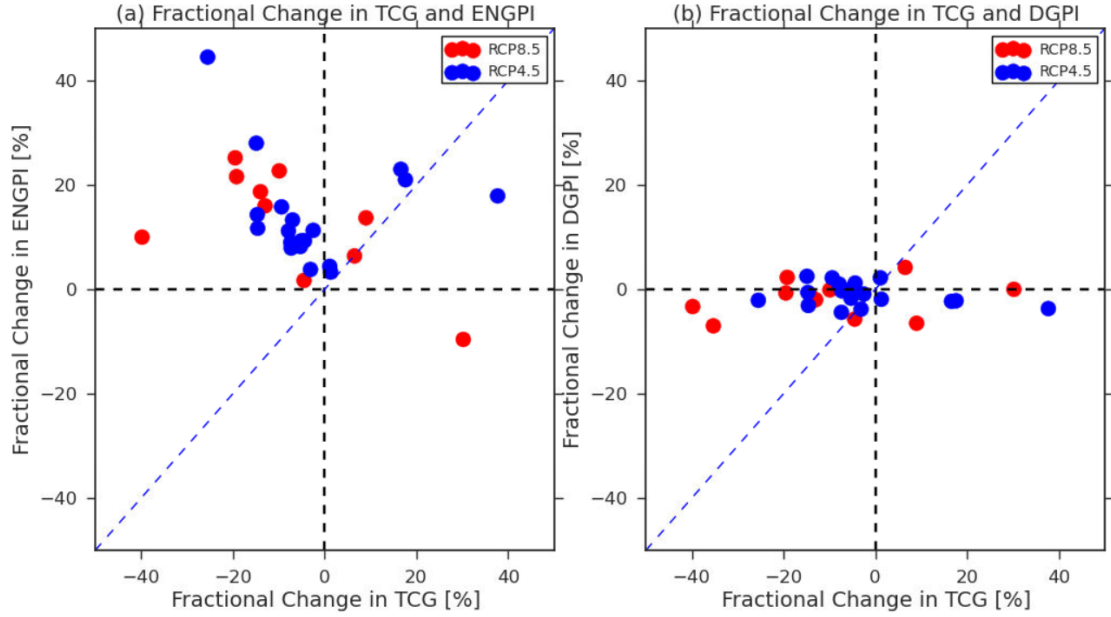


Fig. R2. Scatter plots between TCG changes and GPI changes. (a) ENGPI vs TCG, (b) DGPI vs TCG.

We believe that the original Figs. 3a and b had already captured the general feature discussed above so that we keep the original Figs. 3a and b in the revised manuscript.

3. *While it is good to build a better TCG index to help understand TCG variability, it is also of great value to learn what the mechanic processes are behind. For example, why is vertical motion an important factor in controlling TCG? Is it because vertical velocity is crucial for pre-TC seeds as proposed in Hsieh et al. 2020? I'd like to hear more such discussion on the possible physics behind the choice of variables in DGPI.*

The importance of vertical motion is in line with a hypothesis proposed by the previous studies (e.g, Bengtsson et al. 1996, Sugi et al. 2002; Held and Soden 2006) in which projected weakening of the tropical overturning circulation (e.g., weakening of Hadley or Walker circulations) should be the main reason for the projected decreases in TCG. In general, mean upward motion is important for TC genesis because the boundary layer flows converge and the upward transfer of moisture increases the mid-level relative humidity. Both the dynamic and thermodynamic conditions are also conducive to initiation of organized convection or incipient cyclonic circulation (i.e., the “seeds”). We added above discussions in the revised manuscript as follows.

Lines 227–233: “The importance of the mid-tropospheric vertical motion is in line with a hypothesis proposed by the previous studies^{10–11} in which projected weakening of the tropical overturning circulation (e.g., weakening of Hadley or Walker circulations) should be the main reason for the projected decreases in TCG. In general, mean upward motion is important for TC genesis because the boundary layer flows converge and the upward transfer of moisture increases the mid-level relative humidity. Both the dynamic and thermodynamic conditions are also conducive to initiation of organized convection or incipient cyclonic circulation (i.e., the “seeds”).”

4. *Besides ENGPI, another widely used TCG index is TCGI (Tippett et al. 2011). How does DGPI perform compared with TCGI?*

We appreciate this comment. The GPI developed by Tippett et al. (2011) (TCGPI) is mostly similar to ENGPI regarding the interannual variation (Fig. R3a) and annual cycle (Fig. R3b). Regarding the projected future changes in the global total TCGPI, ensemble mean of RCP4.5, RCP8.5, SSP2-4.5, and SSP5-8.5 future projections show +4.3%, -1.1%, +6.2%, and +8.7%, respectively (Supplementary Fig. S9). When these values are compared with ENGPI (+13.3%, +15.0%, +16.2%, and +26.5%) and DGPI (-0.3%, -1.2%,

+0.2%, and +1.9%), the projected changes in TCGPI are in between ENGPI and DGPI. TCGPI also tend to show projected future increases. We added the following sentence in the revised manuscript:

Lines 245–248: “Finally, we preliminarily investigated another GPI formula proposed by Tippet et al.³⁶. We find that this GPI shows similar skill to ENGPI for the interannual variation and annual cycle in the present climate (Supplementary Fig. S8). The projected future changes in Tippet et al.’s GPI tend to show increases in the total global values (Supplementary Fig. S9).”

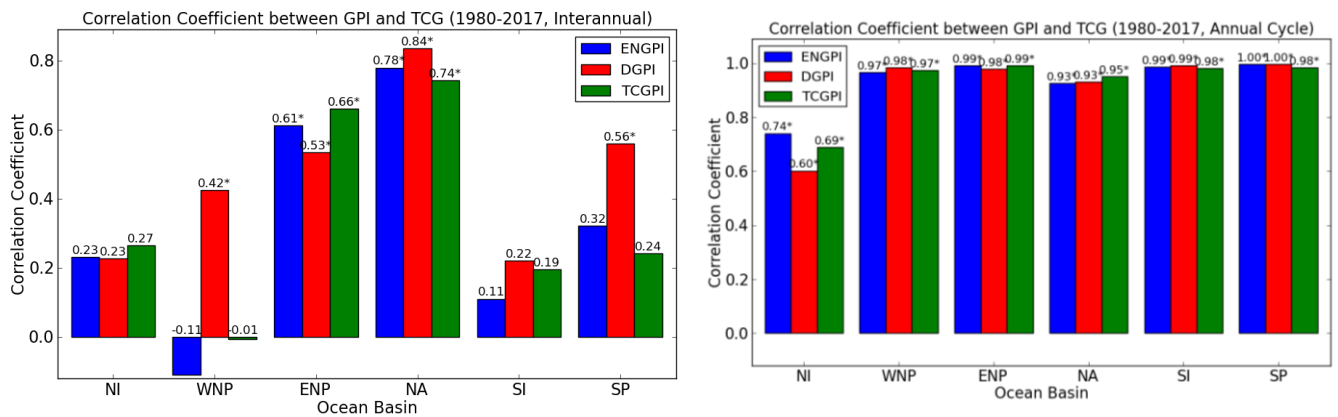


Fig. R3. (a, b) As in Figs. 1c, d, except for including Tippet et al. (2011)’ GPI (green).

Response to comments by Reviewer 2:

We thank the reviewer for his/her insightful and encouraging comments and suggestions. We have carefully revised the manuscript according to the reviewer's suggestions and comments, as detailed below in our point-by-point response. Note that we replaced the CMIP3 A1B scenario by the MRI-AGCM3.2S with the CMIP5 RCP8.5 scenario. We also included CMIP6 model results in the supplement to show consistency of the results among the future emission scenarios.

Response to major comment:

- 1. Please authors clarified which season was used to analyze. If it is the annual mean, the results of this study will be subject to large uncertainty and remain confused in terms of significant distinct differences for different seasons. If it is the peak TC season, please specify it.*

Thank you for this comment. We analyzed all seasons (i.e., January–December). Our target is global so that we believe that it is better to analyze all seasons for the following reasons. 1) TC seasons vary from basin to basin. For example, TC season is April–June & October–December for North Indian Ocean, whereas it is June–November for North Atlantic Ocean. TCs generate all seasons in the western North Pacific. Therefore, it is better to analyze all seasons because our target is global ocean. 2) It is uncertain if the TC season would change in the future. 3) Most of the studies reported projected future changes in TCs for all seasons (see Knutson et al. 2021). It is preferable to analyze all seasons to compare with the past studies.

Meanwhile, we also preliminarily analysed the peak storm season (May–October for the Northern Hemisphere and November–April for the Southern Hemisphere), revealing the consistent results as in all seasons (Fig. R1). Therefore, we keep the results for all seasons in the revised manuscript. We added following sentence in the discussion.

Lines 248–252: “In addition, we also confirmed that the consistent results are obtained when the analysis is applied to the peak storm season (May–October for the Northern Hemisphere and November–April for the Southern Hemisphere) instead of all seasons (January–December) (See Supplementary Fig. S10 for example).”

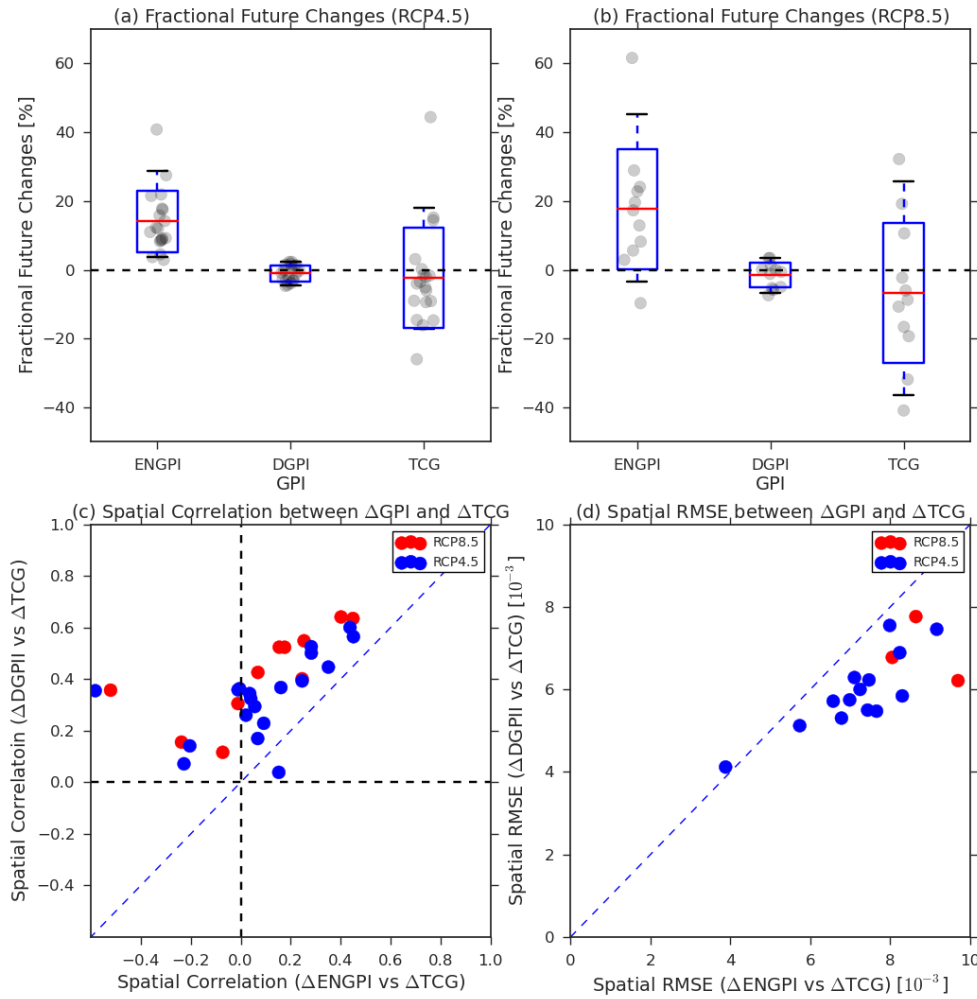


Fig. R1. As in Fig. 3 in the main text, but for the peak storm season (May–October for the Northern Hemisphere and November–April for the Southern Hemisphere).

2. I am confused that the relative importance of each large-scale

environmental factor included in the two GPIs is inferred by their pattern correlations. It is because the pattern correlations do not consider their amplitudes, just simply reflect the consistency for the spatial distribution.

We agree that spatial pattern correlation is not always necessary reflecting each term contribution to the total TCG change. However, we believe that the spatial correlation is also an important metric to measure the spatial consistency between the projected future changes in each variable and the TCG changes. In the revised manuscript, we separately discuss each term contribution and spatial consistency. The former is discussed in terms of the fractional ratio to the total change, whereas the latter is discussed by means of spatial correlation.

3. As the authors said, the DGPI is better to project future changes of TC genesis and inter-annual variations of TC genesis on the global scale from observations than ENGPI. However, the authors did not show the inter-annual correlation between two GPIs and tracked TC genesis frequency over the globe and various basins for these climate models. How is the representation of tracked TC genesis in all GCMs for these two GPIs?

We appreciate this comment. We evaluated the correlation coefficients between the simulated GPIs and tracked TC genesis frequency for the CMIP5 (Figs. R2a, b) and CMIP6 models (Figs. R2c, d) in terms of the interannual variation and annual cycle. Overall, DGPI obtained comparable or higher skill in reproducing the variability of TCG relative to ENGPI and Tippet et al. GPI. In general, the differences between ENGPI and DGPI are mostly consistent with Figs. 1d, e in the main text in which the high-resolution 20-km mesh MRI-AGCM was analyzed.

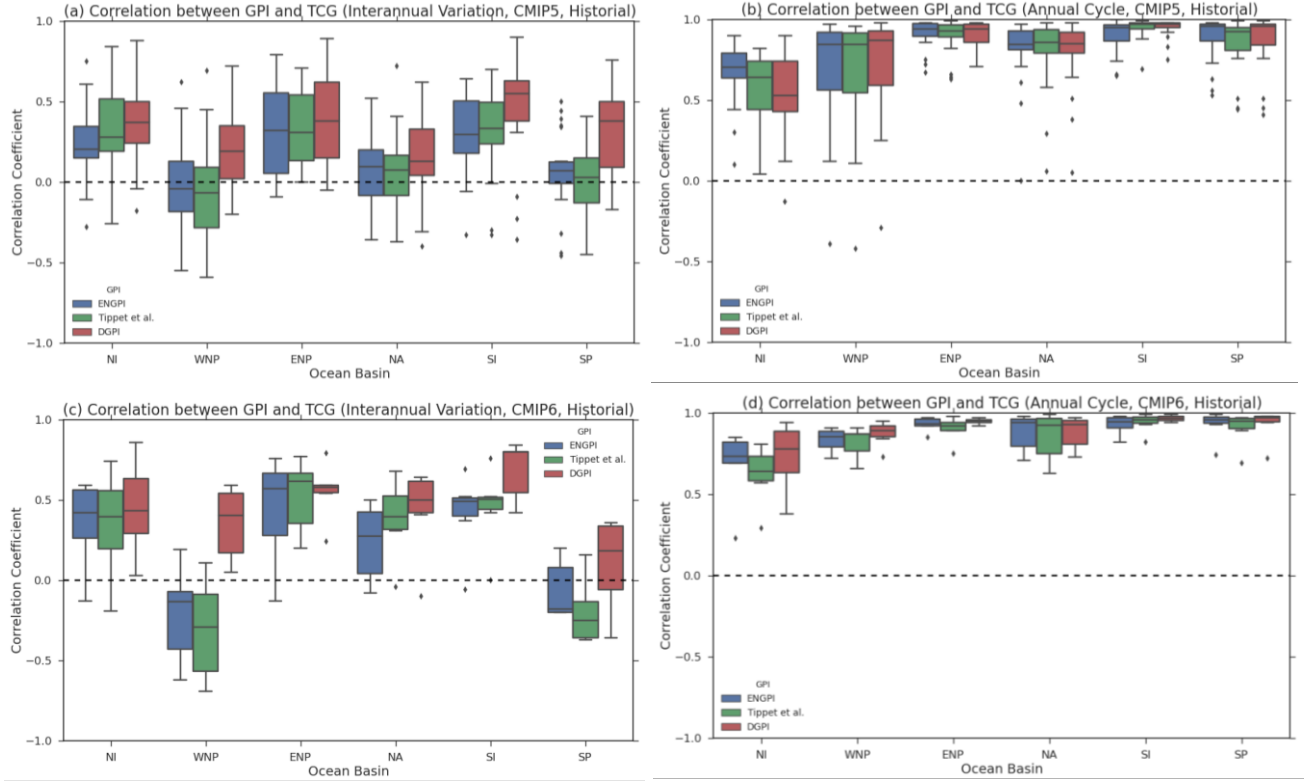


Fig R2. Correlation coefficients between GPI and TCG in terms of interannual variation (a, c) and annual cyclone (b, d) for each basin. The GPIs compared were ENGPI (blue), Tippet et al.'s GPI (green), and DGPI (red). The ocean basins are North Indian Ocean (NI); western North Pacific Ocean (WNP); eastern North Pacific Ocean (ENP); North Atlantic Ocean (NA); South Indian Ocean (SI); and South Pacific Ocean (SP). The boxes represent the range of the 25th and 75th percentile of the intermodal spread. The horizontal black lines show the median values, and the dashed bars show the minimum and maximum values. Diamond marks indicate outliers.

4. *This study just presents the results of TC genesis distribution from only one GCM model (MRI-GCM). I think that authors should show the results of TC genesis distribution from the ensemble-mean of all the*

considered GCM models at least.

We appreciate this comment. In the revised manuscript, we show the ensemble-mean plots for the CMIP5 and CMIP6 models in Supplementary Figs. S1–3, revealing consistent results as in MRI-AGCM.

5. Authors suggested that an unchanged or a moderate decrease of TC genesis frequency over the global scale based upon the DGPI and tracked TC genesis in a warming climate, significantly different from a significant increase of TC genesis frequency using the ENGPI. In fact, from the analyses, the DGPI is mainly dominated by the changes of vertical wind shear and 500hPa omega, while the ENGPI is largely controlled by the changes of vertical wind shear and 600hPa relative humidity. Also mentioned in this study, the 500hPa omega is highly correlated with the 600hPa relative humidity (~ 0.84), how to cause distinct different future changes of TC genesis frequency on the global scale using these two GPIs. Why? Additionally, I guess that the significant difference between these two GPIs may be due to the distinct difference of TC genesis frequency over the specific basin.

We appreciate this comment. However, the reviewer might have misunderstood the projected future changes. The 500-hPa omega (ω_{500}) in DGPI is the dominant factor to contribute to decreasing TCG potential, whereas maximum potential intensity (MPI) in ENGPI is the dominant factor to contribute to increases in TCG potential. As can be seen in Figs. 4b and I, 600-hPa relative humidity (RH600) in ENGPI actually contributes to decreasing TCG potential, which is consistent with ω_{500} .

6. Personally, I want to know how to calculation of the DGPI in detail. One of the researchers from my group attempted to use this DGPI to reproduce the TC genesis as shown in this study, but it is failed. Does

have any special processes for its calculation?

We appreciate this comment. We added one more paragraph in the Methodology (Method d) to describe how to compute DGPI as follows.

Lines 294–302: “The DGPI formula used in this study (i.e., Eq. 4) is a modified version of the DGPI that were originally proposed by Wang and Murakami²⁶. The modified DGPI values were computed over $5^\circ \times 5^\circ$ latitude-longitude grids in this study, although the DGPI can be also applied to any grid spacing depending on the reanalysis and/or model outputs. In this study, we first computed DGPI values for each month then averaged them for computing the climatological mean values. Wang and Murakami²⁶ showed that there is no TC genesis occurred over the domain of 5°S – 5°N and in the region where relative SST anomaly (RSST_a) is lower than zero in both observations and the model simulations. Therefore, following Wang and Murakami²⁶, we replaced the computed DGPI values with zero over the domains of 5°S – 5°N or $\text{RSST}_a < 0$. Here RSST_a is defined as the anomaly of local SST relative to the tropical (30°S – 30°N) mean SST.”

Response to comments by Reviewer 3:

We thank the reviewer for his/her insightful and encouraging comments and suggestions. We have carefully revised the manuscript according to the reviewer's suggestions and comments, as detailed below in our point-by-point response. Note that we replaced the CMIP3 A1B scenario by the MRI-AGCM3.2S with the CMIP5 RCP8.5 scenario. We also included CMIP6 model results in the supplement to show consistency of the results among the future emission scenarios.

Response to major comment:

1. *This is a very brief paper exploring tropical cyclone genesis pattern and frequency in future decades, using an ensemble of multiple model outputs. In principle it makes a contribution to knowledge, particularly through presenting the outputs of the range of models and comparisons thereof.*

We appreciate the reviewer's positive comment on our manuscript.

2. *. Given the long-standing and recently growing output on tropical cyclone dynamics and model outputs, greater emphasis needs to be placed on justifying the necessity and novelty of this study.*

We appreciate this comment. We first added a review of the efforts in the past, remaining issues, necessity on the tropical cyclone climate studies, and novelty of this study in the introduction as follows:

Lines 24–29: “Previous studies proposed potential reasons for the projected future changes in the TCG frequency such as changes in tropical overturning circulation associated with increased atmospheric static stability^{10–11}, entropy deficit¹², ventilation index¹³, and frequency of cyclone seeds⁸. Although consensus has not been reached regarding a theory determining global TCG frequency and the projected future change, it has been widely accepted that TC genesis change depends on the changes in the large-scale

environmental conditions of the atmosphere and ocean.”

Lines 47–49: “Specifically, it is necessary to reconstruct a new GPI that can represent both the present-day and future TCG climate to elucidate the potential physical mechanisms behind the future changes in TCG frequency.”

Lines 65–66: “This study fills a gap of the inconsistency between projected future changes in TCG and GPI and facilitates interpretation of the physical mechanisms behind the TCG changes.”

3. The study does not engage sufficiently with the wider range of literature on tropical cyclones and particularly tropical cyclone genesis and conditions for formation. There are model outputs from the global South (eg. the work of Reason, Abiodun, Malherbe, Engelbrecht) which have been neglected, and work on tropical cyclone dynamics from historical data. While papers to this journal are shorter in format, this engagement with the literature is important in justifying the importance of this study and critically engaging with what has been done.

We included more literature on the tropical cyclone genesis and conditions for formation from the studies for the ocean basins in the Southern Hemisphere as follows.

Lines 43–45: “Some studies also show that the projected future changes in dynamic properties could be the main causes for the projected future changes in TCG frequency in Southwest Indian Ocean²⁸ and Australian region²⁹.”

The work on historical data associated with the use of GPI is also cited as follows.

Lines 30–33: “Thus, the empirical genesis potential index (GPI) depicting

such dynamic and thermodynamic conditions, such as the GPI developed by Emanuel and Nolan (hereafter ENGPI)¹⁴, has been widely used to diagnose the physical processes behind the projected future changes in TCG^{15–20} as well as the observed intraseasonal, interannual and interdecadal variability of TCG^{21–24}.”

4. Engagement with the IPCC and their emissions scenarios should be to the 6th Assessment Report (2021) and not to the 2007 report - each IPCC report replaces the one before it, and they have moved to quite different emissions scenarios. Indeed engagement with the IPCC 6th AR discussions on tropical cyclones is much needed.

We appreciate this comment. We added some results using the CMIP6 models, revealing consistent results with the CMIP5 models. We replaced the results under the CMIP3 A1B scenario with those under the CMIP5 RCP8.5 scenario for the TC-permitting MRI-AGCM model because the A1B scenario is relatively out of date. But the results are generally consistent regardless of the differences in the emission scenarios.

5. It would be good to see more on the verification of model outputs against historical tropical cyclone records - from the databases such as IBTrACs rather than the proportionally larger focus on the comparison between model outputs.

We appreciate this comment. Figure 1 in the main text is actually for comparisons between IBTrACS and a model. Because our preliminary study (Wang and Murakami 2020) had already focused on the verification of the model output in a historical simulation compared with the IBTrACS and the main scope of this study is for the projected future changes, we only presented Fig. 1 for the verification of the model using the historical observed dataset.

6. *This paper requires language editing, and given the generalist audience of this journal, a strong move away from such extensive use of jargon and acronyms.*

We appreciate this comment. We carefully revised the manuscript..

7th Feb 22

Dear Dr Murakami,

Your manuscript titled "What Compels Future Tropical Cyclone Genesis Pattern and Frequency" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. In particular, we would like to encourage you to follow reviewer 3's request to focus on the CMIP6 results in the main paper, and to delineate clearly the advance over earlier work.

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,

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed most concerns from the reviewer. The only suggestion from the reviewer is to add Fig. R2 at least to the supplementary file with some discussion in the main text since it contains key information on the predict power of DGPI on TCG that is not delivered in Fig. 3a and b. Although it is not something we expect if DGPI is a good predictor of TCG, it is what it is and worth letting readers know what needs to be improved in future research.

Reviewer #2 (Remarks to the Author):

All authors have well addressed all my comments. I have no more comments.

Reviewer #3 (Remarks to the Author):

The authors have addressed the majority of the substantive comments raised on the first version of this manuscript. I would, however, like to see further work on the following points:

1) While the authors have now included for comparison AR6 pathways, they retain focus on the RCPs from AR5. AR6 has been out for sufficiently long that this focus on AR5 is outdated. You can retain the comparisons, but change the focus from one of AR5 with a comparison to AR6, to a focus on AR6 with a comparison to AR5 for interest.

2) The authors have included a few more references, but still fail to engage sufficiently on southern Hemisphere tropical cyclone literature. As this is a global oversight, this is not only a large oversight, but problematic to discount the work conducted in the South Indian and South Pacific Oceans. While the word count is limited, the literature covered should be comprehensive.

3) The manuscript still requires a thorough language edit. The authors indicate that this has carefully been done, but there are still many issues in this regard. As one example, the opening sentence 'Future change of tropical cyclones (TCs) genesis frequency and distribution is of societal apprehension and a highly debating scientific issue.' - there should be an apostrophe on tropical cyclones, or the sentence rearranged to avoid possession placed on the cyclone. Societal apprehension is strange wording. And 'a highly debated scientific issue' not 'debating'. This is just one sentence with three errors. I would suggest that if the authors had thoroughly proofread this before resubmission that it would be prudent at this stage to employ the services of a language editor.

4) Please note that there are comments in the margin on the track change version that I cannot read, and therefore cannot assess, as they are not in English.

5) Finally, it has transpired through the review response comments and in the revised manuscript that this follows closely from prior work by the same authors. I think it is important that it is clearly delineated where that study ends and this begins as there are concerns of double dipping.

Response to comments by Reviewer 1:

We thank the reviewer. We revised the manuscript following the useful comments to improve the manuscript.

Response to minor comment:

1. *The authors have addressed most concerns from the reviewer. The only suggestion from the reviewer is to add Fig. R2 at least to the supplementary file with some discussion in the main text since it contains key information on the predict power of DGPI on TCG that is not delivered in Fig. 3a and b. Although it is not something we expect if DGPI is a good predictor of TCG, it is what it is and worth letting readers know what needs to be improved in future research.*

Thank you for this comment. We added this figure in the revised manuscript along with a discussion in the revised manuscript as follows.

Lines 157–161: “To reveal the consistency between the GPI and TCG changes, scatter plots of DGPI versus TCG as well as ENGPI versus TCG are presented in Fig. S1. The correlation coefficient between the ENGPI and TCG changes is -0.47 , revealing that the future changes in ENGPI tend to be opposite in sign to those in TCG. Indeed, among the 40 simulations, only 8 (20%) of them show the projected future changes in ENGPI as having the same sign as those in TCG. Meanwhile, the correlation coefficient between the DGPI and TCG changes is low (-0.12), indicating that DGPI still fails to capture the changes in TCGs in quantitative terms. This means that a simulation with a larger decrease (or increase) in DGPI does not always show a larger decrease (or increase) in TCG, although there are 22 (55%) simulations that show the same sign of the change in DGPI and TCG.”

Response to comments by Reviewer 3:

We have incorporated the reviewer's suggestion in the revised manuscript as follows.

Response to the comments:

1. *While the authors have now included for comparison AR6 pathways, they retain focus on the RCPs from AR5. AR6 has been out for sufficiently long that this focus on AR5 is outdated. You can retain the comparisons, but change the focus from one of AR5 with a comparison to AR6, to a focus on AR6 with a comparison to AR5 for interest.*

We moved the CMIP6 results to the main text in the revised manuscript as suggested. However, given that only 6 models available from the CMIP6 models, discussion on the robustness and the confidence interval among the models is doubtful. Therefore, please understand our strategy to mainly discuss the CMIP5 models and show CMIP6 models to support the CMIP5 models' results.

2. *The authors have included a few more references, but still fail to engage sufficiently on southern Hemisphere tropical cyclone literature. As this is a global oversight, this is not only a large oversight, but problematic to discount the work conducted in the South Indian and South Pacific Oceans. While the word count is limited, the literature covered should be comprehensive.*

Thank you for this comment. The relevant papers that should be cited are the studies analysed the relationship between projected future changes in TCG frequency and large-scale parameters via the use of GPI. Although there are a number of climate models showing projected future changes in TCG frequency over the Southern Hemisphere, there are few studies on the topic except for the papers cited (Melherbe et al 2013; Lavender et al. 2011; Cattiaux et al. 2020). Meanwhile, we understand the reviewer's concern on the less reference on the projected future changes in TCs over the Southern

Hemisphere. There are many dynamical models addressed projected future changes in TCG over the global ocean basins including the ocean basins in the Southern Hemisphere (IPCC 2013; Knutson et al. 2020). We cited these review papers in the revised manuscript and specifically described the projected future changes in TCs in the Southern Hemisphere as follows.

Lines 18–22: “Most previous studies project decreases in global TCG frequency^{1–5}. Specifically, the projected decreases are more significant over the Southern Hemisphere, including the South Indian Ocean and South Pacific Ocean, than the Northern Hemisphere (see Fig. 5 and Table ES1 in Knutson et al.³).”

Also, we specifically highlighted the work by Cattiaux et al. (2020) who showed inconsistent changes in GPI and TCG frequency in the South Indian Ocean. They also showed the consistent results with our study that the changes in dynamical parameters such as wind shear are more consistent with the changes in TCG frequency than the changes in thermodynamic parameters. We cited this relevant study in the revised manuscript as follows.

Lines 53–55: “Specifically, a study on the projected future changes in TCG frequency in the South Indian Ocean showed future changes in ENGPI that were inconsistent with those in TCG frequency, but the dynamical elements in the ENGPI were better aligned than the thermodynamic elements with the projected changes in TCG frequency³⁰.”

3. *The manuscript still requires a thorough language edit. The authors indicate that this has carefully been done, but there are still many issues in this regard. As one example, the opening sentence 'Future change of tropical cyclones (TCs) genesis frequency and distribution is of societal apprehension and a highly debating scientific issue.' - there should be an apostrophe on tropical cyclones, or the sentence rearranged to avoid*

possession placed on the cyclone. Societal apprehension is strange wording. And 'a highly debated scientific issue' not 'debating'. This is just one sentence with three errors. I would suggest that if the authors had thoroughly proofread this before resubmission that it would be prudent at this stage to employ the services of a language editor.

Thank you for this comment. We went through an English editing service to improve the English in the manuscript.

4. Please note that there are comments in the margin on the track change version that I cannot read, and therefore cannot assess, as they are not in english.

The track change was created through the MS word track change. The file was deleted.

5. Finally, it has transpired through the review response comments and in the revised manuscript that this follows closely from prior work by the same authors. I think it is important that it is clearly delineated where that study ends and this begins as there are concerns of double dipping.

Thank you for this comment. We clearly described a relation with our prior work as follows.

Lines 75–77: “Our group’s previous study in which the newly developed DGPI was reported focused mainly on its skill in reproducing the TCG frequency in the present-day climate, but we have not yet published any results respect to future climate projections²⁷.”