

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I feel that the paper has been significantly improved and is now ready for publication as is.

**Thank you for your valuable feedback.**

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Reviewer #2 (Remarks to the Author):

Dear Editor,

The manuscript has been updated significantly to improve following reviewers' suggestions. Especially, they focus on the mechanism, the main topic in this research, instead of comparing it with others without significant implications. The study aims to reveal fundamental aspects of large-scale atmospheric dynamics, not judging who is right or what is more important. I think the current version of the manuscript suggests a balance in their analyses and discussions. The influence of global warming can have various perspectives that simple and linear statistics cannot reveal. This should be approached and studied by many methods and their combinations. Furthermore, the impact of a certain process might be understood through internal dynamics and thermodynamics. There are numerous research simply focusing on the linear sensitivity or teleconnection via wave propagation to determine whether a certain factor is more influential than others. This could reflect a small portion of influence. I hope to focus more on fundamental processes and their mechanisms to find out how global warming could change our climate and weather.

This research seems to take the right path to suggest a deeper understanding of a fundamental process. I have no objection to publish this paper.

**Thank you for your valuable feedback.**

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Reviewer #3 (Remarks to the Author):

I thank the authors for their clear and diligent responses to my previous review comments.

My concerns have been satisfactorily addressed. I think the revised manuscript provides a clearer narrative; the approach is better justified; the results are presented in a more balanced way; and their implications have been discussed in greater depth.

I enjoyed reading the paper and I think it will be a valuable addition to the literature on this topic. I recommend publication.

**Thank you for your valuable feedback.**

We expanded our review of the literature on this topic and included the following references in the Introduction and Discussion sections of the manuscript. The results are used as evidence of waviness trends, but it is not a comprehensive review of the literature. The reviewer's comment is correct. We repeated the text here for the reviewer's convenience.

We also updated the references in the Introduction and Discussion sections of the manuscript. The reviewer's comment is correct. We repeated the text here for the reviewer's convenience.

Editorial Policy: <https://www.nature.com/documents/editorial-policy-checklist.pdf> (Downloaded 12/02/24). For Manuscripts that fall into the following fields:

- Behavioural Science
- Ecology
- Life Sciences

An updated and completed version of our Reporting Summary must be uploaded with the revised manuscript. You can download the form here: <https://www.nature.com/documents/reporting-summary-form.pdf>

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developed, focusing on a north-south gradient of saturated specific humidity (the so-called moist-get-moister idea in Eq. 5 of Shaw and Miyawaki 2024). That paper is actually easier to understand since the dynamics were clearly presented. In the current study, the same framework is extended to emphasize meridional wind changes and east-west gradients of some related fields.

[illegible][illegible]

However, the main issues may lie in planetary scale motions, which have a longer time scale. When I read the paper, I could not follow the logic easily. Especially, Figure 5 presents a unit suggesting a  $\text{m s}^{-1}$  term associated with the  $\text{m s}^{-2}$  term. I would like to see a more detailed explanation of the physical meaning of the terms. The authors just use one or two sentences to quickly go through this critical part. More effort should be devoted to fully develop this thought to facilitate a good understanding for readers.

[illegible]

**lead to unethical practices that could be detrimental to all, including the individuals involved and the rest of the world.**

clear and appropriate way, distinct from their own paper.

second, we can either decrease the  $\lambda$  value, get faster dependence on all original  $\lambda$  values, thus sparse be

Yes! The fast gradient method for fast gradient descent and also the gradient method for the fast gradient descent.

In addition, in the abstract, the authors argue that some parameter trends in water patterns also occur in the SH in

reanalysis (Figure 1). We note that this is not the case in the fast gradient descent response

and how to use the moist-get-moister framework to explain this feature in the SH should be fully discussed. Based on the main

content of the paper, how to understand that the SH witnesses this feature earlier than the NH needs to be better clarified

since the moist-get-moister has appeared to be more significant in the NH, I understand Figure 2 is not directly

statements in the paper. I think the authors should be more explicit in the text to clarify the change (see lines

50-60).

denominator (see lines 151-159). We added an analysis of LWA which shows some

**single entities with the product**  
Reviewer #2 (Remarks to the Author):

The research examines the extreme waviness of large-scale atmospheric flow in the upper atmosphere under global warming. Using a simple metric to represent the degree of waviness, the authors concluded that this waviness in the upper levels will increase due to the rise in moisture associated with global warming. Specifically, the increase in moisture provides a mechanism for the amplification of the atmospheric circulation. The authors find that the increase in moisture leads to a decrease in the degree of waviness in the upper levels, rather than an increase. Therefore, the positive feedback from moisture is identified as the primary physical process causing the upper-level jets to become wavier under global warming.

The recently published article by Yamamoto & Martineau (2024) provides a good review. Moreover, the positive feedback operating to provide more waviness is quite interesting because it emphasizes the role of moisture in large-scale dynamics.

**The Supreme Court has ruled that the North Carolina law controlling it**

1. Implications of Waviness in Upper Levels;

clarified since the moist-get-moister idea appears to be more significant in the atmospheric dynamics, that is perfectly valid. However, this question is not directly relevant to the primary aims of this research. The issue regarding the waviness of jets initially arose from the expectation that jet stream waviness is strongly related to extreme weather in mid-latitudes. Over time, many researchers became intensely focused on trends in the waviness of jet streams. However, the metric used in the present study is significantly different from the one used in the previous work reading the weakening of the mid-latitude jet stream (e.g., Solomon et al., 2006; Solomon and Solomon, 2002; Solomon et al., 2000, 2002) over a time interval of half a century. The present study is not concerned with the fact that this index has been used in the past, but rather with the fact that it is not a good metric to study trends in the waviness of jet streams. The present study is concerned with the fact that the index used in the present study is significantly different from the one used in the previous work reading the weakening of the mid-latitude jet stream (e.g., Solomon et al., 2006; Solomon and Solomon, 2002; Solomon et al., 2000, 2002) over a time interval of half a century. The present study is not concerned with the fact that this index has been used in the past, but rather with the fact that it is not a good metric to study trends in the waviness of jet streams.

What I mean is that the waviness of the jet stream could be one factor influencing extreme weather, but it is not the only factor. Therefore, I question whether the extreme cases examined in this research are truly related to extreme weather at lower levels, where people live.

2. Time-scale:

positive trend (Fig. 4c) in the near future. This could be a direct consequence of significant ( $p < 0.05$ ) in the dynamics under Arctic Amplification.

[illegible]

timesphere and statistically significant the fast gets low the 8 rather than is a they don't  
ld the positive feedback also affect planetary-scale eddies?

the principal defendant in the trial. Mr. Tamm reaffirmed a trial of this type to the justices on June 2, 1935.

**774 (1992-2014) at-tarigef warthirey (upper-level/tropical warming and polar amplified**

current research presents an interesting dynamic coupling with thermodynamics. Despite this intriguing dynamic thermodynamic coupling, there appears to be one more thing the dry ice is doing with the convection. Deep in the boundary layer, the dominant flow is the fast get faster response, not the slow get faster response. The large boundary conditions for the primitive equations. Given the non-local characteristics of the Navier-Stokes equations, the boundary conditions can influence solutions throughout the entire domain. Our question should be how global is the boundary condition? (Supplementory Fig. A11b) A diffusion consistent with the fast get-

fastest response of upper-level wave response emerging given the historical distribution and a lack of moisture in upper levels is an internal thermal forcing to the primitive equations. The internal forcing acting on a certain criterion can have very interesting aspects for large-scale atmospheric dynamics. However, I wonder if temperature and, for change in temperature gradient, the line [Fig. 3a](#) and discuss these points in the revised manuscript as [lines 204-215](#) of this.

**We agree that all significant observations based on the fast-get-slower response behavior level**  
**lowering (I thank you, the Author)**  
**various. The previous model pretation when we analyzed well known levels is as follows:**

Chuang and coauthors presents an interesting analysis of the two major mechanisms of meridional (north-south) wind trends in a study of climate model simulations and in observations. A key aspect of the analysis is considering the dependence of the trends off amplitude on the authors report that the North-South winds in the upper troposphere have little strength in a high-latitude but a horizontal warming of the tropics is a plausible cause of the observed trends. The authors also find that the vertical dependence from the perspective of latitudinal density gradient changes. They suggest that trends are detectable in the observed record in the Southern Hemisphere but not in the Northern Hemisphere.

large by a messy response that gets lower paper (see lines presence) of significant polar climate change (Arctic sea ice loss and Arctic amplification) in accepted climate models (ventral publication). But not yet, I think the manuscript fails to fully deliver on its aims and to sufficiently support its claims. And it neglected sea ice loss experiments has discussed on lines 254-262. We use the authors' aims to "clarify recent controversy regarding the impact of Arctic climate change on waviness"; however, in my opinion, the published physics-based model the oceanic wind, explain the increase (see lines 269-269) note appears in current form muddles the waters further. I'll try to explain why I think that in what follows, and make some added a physical interpretation related to changes in the meridional temperature contrast across the tropics. The band cloud difference contrast (see lines 269-274) due to surface weather – but the authors

[illegible]

second point - related to the first but more technical - is about the choice of metrics. The authors present some arguments using the meridional wind as an admittedly simple measure of waviness. They also present some arguments for not using other metrics. I don't find any of these arguments especially convincing, in the sense that one could (and indeed other authors have) make equally strong (or weak!) arguments for using other metrics. The fact some of the metrics are not directly output from CMIP6 is a poor justification, as they could still be calculated from available variables. The authors acknowledge



and [Bjornsson \(2013\)](#) note that the observed weather extremes (2016-2019) may be a result of the fact that the results are consistent with a "geometric" approach to defining waininess (see related discussion in [Geen et al., \(2023\)](#)). **As a consequence of the nonlinear relationship between weather and consequential**

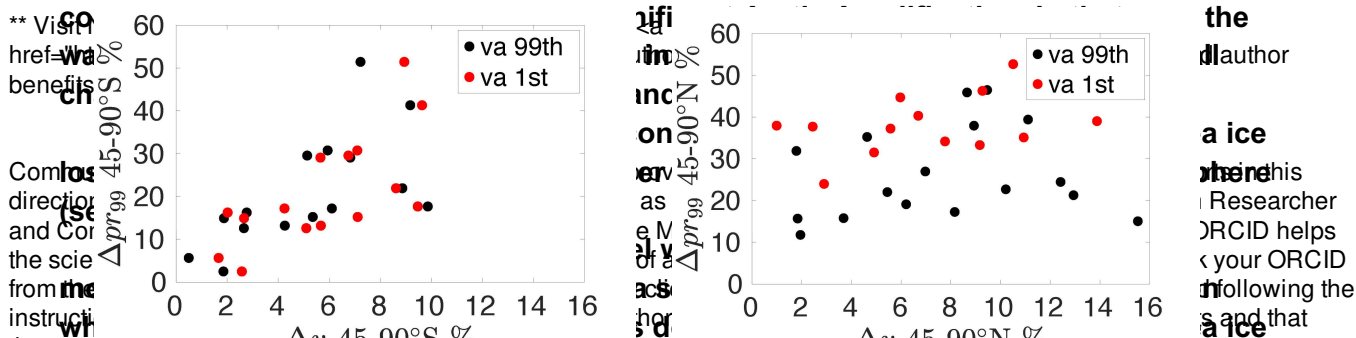
Thank you for your feedback. We received three responses from the scientific community of something rather obvious. Am I right in surmising that interpretation is that the meridional density gradient increases in the upper troposphere and decreases in the middle troposphere and this explains the increased and decreased winds, respectively at these levels, or features like storming forces that trigger and/or dampen the density waves? A larger amount of detail can be used to explain the decrease in the space response of a gas with the stretching of a wave from the troposphere to the stratosphere, but I think that's a little more than I intended. It's not that simple, or I am missing something here? If it's essentially that simple, I would try to bring the message out more clearly, as currently it gets a bit lost.

Another related with precipitation extremes is the Southern Hemisphere extratropical region, but not much interest is the mooted link to surface weather extremes. What does it mean for surface weather to have differing 'waviness' trends at different levels? It is generally unclear how different metrics of 'waviness' relate to surface weather – but the authors have been able to infer a deepening of synoptic-scale weather systems in the Tropics, with the implications of this should be considered for different conditions to investigate the effect of a global warming on large-scale weather. I also agree with the authors' conclusion that the different trends in surface weather are not clear exactly what this means; is it that there would be mooted changes in surface weather because of the opposing 'waviness' trends? Some further discussion on implications would be helpful – especially if the aim of the paper is to provide clarity to the discipline. At the moment, this is left for further work. I am sympathetic to limitations of scope but would like to see some more work on this, that has been left by the authors. I think it is important to have a better understanding of the links between the different trends in surface weather and the different trends in the different layers of the atmosphere. I think it is important to have a better understanding of the links between the different trends in surface weather and the different trends in the different layers of the atmosphere. I think it is important to have a better understanding of the links between the different trends in surface weather and the different trends in the different layers of the atmosphere.

The **modeling** of climate change effects (Gleckler et al., 2006) will become more complex as we move beyond first-order approximations. The ‘fast-gets-faster’ response is better and arguably the results presented here are just an extension of the ‘fast-gets-faster’ response introduced by some of the same authors in a previous paper. The author themselves note the fast-gets-faster should apply in the north-south direction too.

The projected decrease in mid-tropospheric 'wingspan' in the Northern Hemisphere, which is suggested to be sensitive to Arctic amplification is contrary to the well-known assertions in the Francis and Vavrus papers. The submission is, perhaps deliberately, quite loose in its description of conflicting past results. But given the prominence of the Francis and Vavrus hypothesis in the report, it is surprising that the change is attributed to the 20th rather than to the poleward advection of the Francis and Vavrus hypothesis. Again, this suggestion is in the spirit of a narrative that adds clarity to the debate.

other regions and seasons presented so far at this point the impacts of changes in the boundary conditions: sea surface temperature warming vs Arctic sea ice loss. In particular, the AMIP and AQUA simulations involve + 4K sea surface temperature warming boundary



https://www.scribd.com/document/385646466/Response-to-Comments-2020-2100-minus-historical-1980-2000-99th-percentile (see lines 334-335)

precipitation versus 99th or 1st percentile of 250 hPa meridional wind for the

**extratropical (45-90 degrees) (a) Southern and (b) Northern Hemispheres for individual**

**coupled models. Correlation coefficient is (a)  $R = 0.76$  and (b)  $R = 0.08$ .**

Version 1:

Decision Letter:

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

Dear Professor Shaw,

Your manuscript titled "Fast-get-faster explains wavier upper-level jet stream under climate change" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

The 'wavy-gets-wavier' moniker feels a bit of a gimmick to me. As already noted, I don't think we therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work. **here are just an extension of the 'fast-gets-faster' response introduced by some of the same authors in a previous paper. The author themselves note the fast-gets-faster should apply in the north-south direction too.**

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table". **The reviewer is correct that ultimately there is a broader concept at play here which is**

\*\*\*\* Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests can lead to delays. **the extreme get more extreme behavior that follows from the multiplicative increase of CC. Following the reviewer's suggestion we revised the manuscript to focus on**

**investigating the implication of the fast-get-faster mechanism for meridional wind. We revised the introduction (see lines 36-64) and changed the title to "Fast-get-faster explains wavier upper-level jet stream under climate change".**

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

The projected decrease in mid-tropospheric 'waviness' in the Northern Hemisphere, which is suggested to be sensitive to Arctic amplification is contrary to the well-known assertions in the

Francis and Vavrus papers. This is discussed in its own right in the Editorial Request Table. The list of required files is also available at <https://www.nature.com/documents/commsi-file-checklist.pdf>

description of conflicting past results. But given the prominence of the Francis and Vavrus hypothesis, I think it is necessary to make a clearer (more direct) statement about how these

results support, or not, the Francis and Vavrus hypothesis. Again, this suggestion is in the spirit of a narrative that adds clarity to the debate. Communications Earth & Environment is a fully open access journal. Articles are made freely accessible on publication. For further information about article processing charges, open access funding, and advice and support from Nature Research, please visit <https://www.nature.com/commsenv/open-access>

**In the submitted manuscript we discussed the comparison with the previous hypothesis of increased mid-level waviness due to Arctic climate change. We expanded this discussion to include references to the robustness of the mid-level waviness weakening across different metrics (meridional wind distribution, MCI, sinuosity and LWA) (see lines**

**65-75). We also now specifically state that our results do not support the hypothesis that Arctic Sea ice loss and Arctic Amplification lead to increased waviness (see lines 342-345).**

Please use the following link to submit the above items.

Link: [\[Link\]](#)

\*\* This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first \*\*

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

Best regards,

Alireza Bahadori, PhD  
Associate Editor  
Communications Earth & Environment

Kyung-Sook Yun, PhD  
Editorial Board Member  
Communications Earth & Environment  
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