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Peer Review File

Anthropogenic Climate Change Leads to a Pronounced Reorganisation of North Atlantic Atmospheric Circulation Regimes

Corresponding Author: Professor Christian Franzke

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

Version 0:

Decision Letter:

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Dear Professor Franzke,

Your manuscript titled "Anthropogenic Climate Change Leads to a Pronounced Reorganisation of North Atlantic Atmospheric Circulation Regimes" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

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

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

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

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

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

Best regards,

Chao He
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REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

Review of:
Anthropogenic Climate Change Leads to a Pronounced Reorganisation of North Atlantic Atmospheric Circulation Regimes

The authors apply SVD to study the changes in the North Atlantic atmospheric circulation using the CESM-LE. They separately study the internal variability (minus the ensemble mean) and the full flow field of the 500hPa streamfunction and surface temperature. They apply changepoint analysis, identifying different behaviour before and after 1995, where after the forcing of global warming starts to impact the circulation.

For the period before and after 1995 they apply Gaussian Mixture Modelling to identify circulation regimes. For the full flow field they find that the number of regimes stays the same, although their patterns change. For the internal variability they find a reduction in the number of regimes from 4 to 3, with one regime disappearing. The regime analysis is followed by one on the NAO, for which a small increasing trend is found. This is linked back to the regime dynamics and through that to changes in the jet stream strength and location.

Overall, I find the study well executed and it shows important results that provide a detailed view of the atmospheric response to climate change. However, the writing and structure of many paragraphs at times is confusing and needs substantial rewriting to ensure clarity. I suggest the authors revisit their text and carefully assess the storyline, while aiming to make it more concise to get the main results across better.

Below I first provide my main concerns, followed by a list of minor comments and some specific points

Main comments:
1. In many places throughout the results section it is not clear whether you are discussing the results of the full flow field or those of internal variability, or only after reading ahead. I suggest introducing abbreviations for both (FF and IV maybe), which you can then use throughout to ensure it is clear which of the results you are discussing. Furthermore, the discussion can be made a lot more concise and concrete. E.g. throughout the discussion of the results there are sentence like in line 230, 242. These sentences are repeated often, do not add much to the discussion and disrupt from the story. I suggest removing them to make the story more concise. Instead focus on making the influence specific, what are the actual changes you find and how do they link to the literature. Doing this throughout would make the results section easier to read.
2. The arguing for SVD in favour of EOFs is unclear to me. The way I understand it, it is because SVD analysis also allows to identify the forced global warming signal, whereas using EOFs this is less straightforward, which indeed is valuable. However, the paragraph (lines 111-132) in which this is discussed lacks a clear storyline. I suggest restructuring this paragraph, starting with a one sentence summary of why you use SVD over EOFs, followed by an elaboration going into some of the details and advantages. Also, it would be valuable to clarify that for SVD the sign of the pattern does matter.
3. One of my main concerns is the assumption of piece-wise stationarity of the GMM. For the post-1995, when the climate is warming and the PC's all are increasing this is not a valid assumption, which is also shown in the regime results with a shift over time. While I appreciate this assumption is required to allow a thorough analysis, it does require better arguing and improved discussion. Furthermore, would it be an option to first detrend the data and then do the analysis?
4. The structure with first considering the SVD and regimes and only then looking at the NAO-index could also be switched around. At the moment the discussion on the NAO results comes out of the blue and at first does not appear to fit an overarching story. I suggest thinking through the storyline of the results and the best order in which to discuss them, as well as better linking the different sections.
5. In the discussion section the results are not places inside the wider literature on e.g. changes in the NAO in a changing climate or the link with the ocean cold blob. This needs to be done. The discussion could also benefit of some discussion on the physical mechanisms of the observed changes as well. It is currently written it is a conclusion and summary, but not as much a discussion.

Minor comments:
• Your results are only valid for winter, so please specify that in the title.
• I suggest rewriting the abstract to clarify you separate the forced signal and internal variability and that the results differ, specifying how many regimes you find and rephrasing lines 10-12 as that sentence is hard to follow and it is unclear to me what you mean by "fingerprint". Also, the last sentence refers to "important implications", which you don't mention or refer to in the main text.
• Can you explain why you decided to use the streamfunction instead of geopotential height?
• What are the bounds of your region and how did you decide on those? How sensitive are your results to the chosen region?
• For the CESM-LE matching with ERA-5 data. Can you show the PDFs, both for the streamfunction, surface temperature and NAO index, as the variance of ERA-5 appears smaller. Also, can you show similar figures to Fig. S1 for the N. Atl. region you consider, to see how well it matches there? (I assume Fig. S1 is global, but it does not say this in the caption, so please check that and add to the caption) It would also be valuable to know how much the spatial patterns of the variability deviate, e.g. how does the NAO compare? Do you expect an impact of this on your results? I would think it could primarily affect the internal variability signal.
• You refer to Fig. 1, which shows pre- and post-1995 results, before introducing the changepoint detection. This is confusing and I suggest restructuring.
• Can you explain why you decided on using the changepoint of PC2 of the full flow field? And not PC1 or the internal variability ones? And in line with this, what would be possible physical reasons for the changepoints being found where they are? Is it changes in the ocean circulation, is it a threshold of sorts?
• Currently the results on the regimes come very out of the blue and to me do not naturally follow from the discussion before. I suggest restructuring and starting a paragraph with the main question you want to answer by studying the regimes. In your initial reasoning it is unclear to me how Fig. 1 fits into that discussion.
• Have you tried applying GMM to more of the PC's? E.g. up to PC5. And if not, why did you decide to only use the first two

PC’s?
• The order of the regimes is not aligned between the pre- and post-1995 data. It would significantly help readability if they would be aligned (with the same color).
• The abbreviation FRS is confusing to me, as it is the most frequent regime state, whereas frequent on its own does not make that sufficiently clear.
• The “gradual shift” (line 206) is hard to see from Fig. 2. Can you make this more clear in the plot, also that there is a time component to this? And how does this align with the stationarity assumption?
• In line 211-212 you refer to results you will discuss later, without making this specific. This is done multiple times in the results section and makes reading confusing at times. I suggest specifically saying you will discuss this later in Sec. ….
• Discussion in Lines 203-224. In the first paragraph you call the full flow field regimes steady, to at the end of the second paragraph say they are changing. This does not align. While I appreciate your likely referring to the patterns, you have not discussed those yet. Also, in the last sentence you say “true physical shift”. How do you assess whether this is the case or not?
• Throughout the discussion of the results there are sentence like in line 230, 242. These sentences are repeated often, do not add much to the discussion and disrupt from the story. I suggest removing them to make the story more concise. Instead focus on making the influence specific, what are the actual changes you find. Doing this throughout would make the results section easier to read.
• The “southward displacement” (line 234) is hard to see, maybe plot overlapping contours? Also, what do you mean with “conventional”?
• For the internal variability after 1995 you only find 3 regimes. In line 257 you mention the corresponding patterns have a reduced loading. To me this is to be expected as less regimes, means more data per regime and thus a centroid that considers more data, automatically weakening the pattern. Or am I wrong?
• I do not see how you arrive at the conclusion in line 260.
• When discussing the changes in regime patterns, you mention structural similarity. Can you make this concrete, by e.g. computing the distance or pattern correlation between the regime patterns. And what then would be the physical interpretation of these changes?
• The discussion in lines 272-280 feels out of place after already having drawn a conclusion on the changes, I suggest re-ordering.
• I do not see how you can draw the conclusion in line 279-280 based on the discussion before.
• In line 297 you say “revealing a clear physical circulation behaviour” linked to the opposite polarity. What sort of behaviour? This is unclear.
• I do not understand how you arrive at the conclusion that “the potential disappearance of Cluster 14 is due to physical suppression and is not a statistical artefact” (lines 300-301) from the discussion before?
• On the NAO index. As currently written it is unclear whether you compute the PC for each ensemble member separately, or for the full ensemble in one go. Can you clarify this?
• You look at the distribution of the NAO phases pre- and post-1995. Since the regime patterns change, wouldn’t you also expect the NAO to change, i.e. the pattern to change with the regime change? Can you comment on this.
• The statement on the “consistent shift of regime states towards a more positive NAO phase” (lines 362-363) is hard to see from the figure. Can you make this clearer by e.g. indicating the maximum of the other distributions in the figure?
• Around line 424, it took me a while to realise you use the regimes to look at the impact of the jet stream on the NAO. It would be good to mention this earlier.
• You mention a decline of the westerly wind strength (line 441), but looking at the figure I also see shifts. Is that correct and can you comment on that?
• Can you clarify what you mean by “transitional NAO phases” in line 447?
• In line 462, what do you mean by “the regime states intensify”? And how can I see tis from the discussion in the results section?
• The discussion is the first place you mention extreme NAO events (line 464). Please mention this in the results section, linking it to the figures.
• You refer to the “recurrent characteristics” of the regimes (line 466). Can you also comment on their dynamics, i.e. how does the circulation transition from one regime to another and how persistent are they?
• Can you make the statement “reveal distinct shifts in regime characteristics” (line 474) more concrete?
• In line 489 you mention “previous studies”. Please give those (also see my main comment on the discussion).
• How dependent do you think your result on the specific warming scenario you use? Can you discuss this in the discussion.
• In Data and Methods, in the SVD section, it would aid if you in one or two lines discussed the physical interpretation of SVD compared to EOFs. How should people think about this?
• In the section on the GMM, you need to mention x is a data point, as is z? And what i and k represent in (2). Also, should the | in eqn. (5) be there, and if so why?
• Can you expand the discussion on the BIC? As it is not that straightforward to compute, e.g. what are the dimensions you use?
• Would it be an option to make the code publicly available on e.g. Zotero? And if not, why?
• Figures: The location of the streamfunction patterns in Fig. 1 is confusing, as it appears one is about the streamfunction and the other about the temperature, while they are the streamfunction pre- and post-1995. In line with one of my minor comments, it would help to align the colors in Fig. 2, 5, and 7. Please check there are units at each plot! E.g. Fig. 3 is missing them. Also, please make the fontsize larger, especially for the colorbars and donut charts, but also for some labels such as the changepoint years or the Full flow field/Internal variability in Fig. 6. What do you mean by “transitional behaviour” in Fig. 7?

Specific feedback:
Line 10 – Decline in the number of regime states.

Line 18 – Greenhouse gas -> greenhouse gasses
Line 21 – It is unclear to me what you mean by "shaping climate responses". Please clarify.
Line 86 – It is unclear where "this region" refers to. Please clarify.
Line 106 – The sentence starting here is unclear to me. Please rephrase.
Line 139 – The word "shift" to me suggests a change in time, which is not the case here. Please rephrase.
Line 142 – As for "shift", but here with "rearrangements".
Line 205 – You say the regime states are "steady", while their patterns to appear to shift. Only their number is steady. Please rephrase.
Line 254 – Please avoid terminology like "loadings of SVD patterns", as it is unclear to a non-specialised audience. Same for "polarity" in line 296.
Line 292-293 – This sentence is unclear to me. Please rephrase.
Line 479-282 – These two sentences make pretty much the same statement. Please remove one for conciseness.
Caption Fig. 1 – Lines 830-831 repeat the one before. Remove for clarity. It would help to name the other changepoints in the caption.

Reviewer #3 (Remarks to the Author):

Review of "Anthropogenic Climate Change Leads to a Pronounced Reorganisation of North Atlantic Atmospheric Circulation Regimes" by Satpathy et al.

Recommendation: Major revision

In this manuscript, the authors have investigated the response of North Atlantic atmospheric circulation regimes to anthropogenic global warming. By decomposing the circulation into a forced response and internal climate variability, the authors suggest that anthropogenic global warming will reorganize the North Atlantic atmospheric circulation regimes. The manuscript is well written and is worthy of publication. I think it can be further improved by considering following suggestions.

Major points:
1.Why does the ensemble mean represent the forced response, which means that the internal variability (e.g., NAO) have been fully eliminated? This issue is the basis of this study. I suggest the authors give a clear explanation.

2.It would be best to explain the differences between the NAO-like pattern obtained by SVD analysis and the NAO obtained by EOF analysis.

3.Some conclusions in the main text are obtained based on the figures in the supplementary materials (e.g., Fig. S2, Fig. S5, Fig. S6, Fig. S8, Fig. S9). It's best to place them in the main text.

Minor points:
L13-14:"These changes in regimes have important implications for extreme weather, energy, water, and food security." The important implications of the authors' finding have not been explanation in the discussion.

L255:Here, the authors seem to describe Fig. 3g not Fig. 3 (e and h).

L259:"However, the change in spatial structure of the SVD patterns indicates that external forcing reorganises the centres of circulation over the North Atlantic". Not understand.

L331:"This comparison demonstrates that CESM2-LE effectively captures the observed NAO phase distributions". What are the correlation coefficients between the NAO indices in the CESM2-LE and the observed NAO index?

L411-423:Why does Fig. S9 mean "This highlights the imprint of the global warming signal in modulating the variability of the NAO index in the future"? Not understand. The decline in the variance of the NAO index in Fig. S9b is the "internal variability", which is not related to the global warming.

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Version 1:

Decision Letter:

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Dear Professor Franzke,

Your manuscript titled "Anthropogenic Climate Change Leads to a Pronounced Reorganisation of North Atlantic Atmospheric Circulation Regimes" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

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

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

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

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

Best regards,

Alice Drinkwater, PhD

Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

I thank the reviewers for carefully taking into account my previous comments and feel the manuscript has substantially improved in clarity. I do still have some minor points for clarification, which are given below.

Minor and technical comments:

L8-10 – Please specify the first part of the sentence refers to the forced signal.

L83-85 – You already say something like this in the paragraph before.

L111 – the framework -> a framework

L199-201 – This is more or less what you already said, I suggest either removing or adding e.g. “In summary”

L216, 227 – It would be valuable to note how often the MFR occurs.

L249 – This is for FF?

L252-255 – In what way does the similarity in patterns indicate that the circulation patterns remain caused by external forcing? Also, it is confusing to bring intensity and frequency in there, as the spatial pattern correlation does not quantify changes in these. Also, 0.62 is not too high, what causes this change? And looking at the surface temperature there is quite a difference in some regions.

L260 – Increased frequency does not directly mean an intensified regime, so I do not see how you can make this claim.

L274 – Clarify what “This decline” refers to. In general, these sentences read confusing.

L282 – Remove “potential” (and check this sentence).

L286 – What do you mean with evolution? The change pre- and post-1995 I think, but suggest phrasing this differently. Also, what do you mean by “dynamical patterns”?

L292-294 – Joint dominance doesn’t indicate the circulation becomes more dynamically stable, what you mention as evidence in the second part of the sentence does. Please rephrase.

L296 – What do you mean by “further clarifying the move towards “regime locking””?

L299 – FR -> MFR

L302 – “or the … periods” -> between the pre- and post-1995 periods (I think, or I misunderstood the sentence, in that case, please rephrase to clarify)

L303-304 – What do you mean by “internal connectivity”?

L306-307 – What do you mean by “recurrent counterpart”?

L311-312 – With “greater tendency”, do you consider the expected changes when one of the regimes disappears? That is, is it proportionally still greater?

L315-316 – This better fits after the first sentence of the next paragraph, but do then check for consistency.

L348 and further – S5 -> S8

L364 – What do you mean by “the total range of variability remains dominant”?

L394 – Add FF for clarity.

L410 – but only -> and

L420 – the stationarity -> stationarity

L433 – Can you elaborate a bit on why you specifically look at low-amplitude NAO- events, and not all NAO- events?

L446 – I would say there is a complex interplay between the NAO and the regimes, and not necessarily one responding to the other. I suggest phrasing as “are connected”, as you don’t do a causal analysis on this.

L451 – Specify FF.

L479 – the two -> two

L485-486 – The part “With the … century“ repeats what is said in the first sentence, so I suggest removing.

L490 – This sentence is confusing, please rephrase.

L492 – What “distinct transformations”?

L509-512 – Split this sentence somewhere, it is too long.

L519-521 – I think the novel part here is the change in internal variability, so I suggest focusing this sentence on that.

L537-540 – The last part of this sentence says something very similar to the first, so suggest removing.

L542-543 – What do you mean by “enhance seasonal climate variability”?

L613 – “applied to a single variable field” (added to a)

L655 – Say somewhere that determining the optimal cluster fit means finding the optimal number of clusters K. I would also use capital K in the notation here, to avoid confusion with the section before.

L683 – static -> statistic

L811 – 40. Check this reference.

L929 – 92. Add author(s).

Fig 2 – I suggest removing the axis labels of the inset figures to allow for larger, more readable tick labels.

Fig 4 – Hard to read in the dark red part, also please increase the fontsize in the transition matrices and add a label to the colorbar at the bottom. The locations of the circles do not align between top and bottom, i.e. the colors move around, can you ensure consistency there, so that you look at the same place in top and bottom fig. for e.g. transitions to the MFR. And a small point, I feel it’d be more insightful to use e.g. “13 <- 12” with “13 -> 12” instead of “12 -> 13”, so that the arrow is in the direction of the transition. (Same for S7, where the dark squares are pretty much unreadable)

Fig 5 – Check the percentages of explained variance for FF and IV. It seems unlikely that they are exactly the same. Also, please increase the fontsize in the c, d and e.

Fig 6 – In c,d,g,h Can you add a line representing the mean of the distribution, which you now refer to in the text? In c, can you put the yellow distribution in front of the green one, as it’s hard to see.

Fig 8 – Please increase the fontsize, especially of the small insets as they are near impossible to read.

Fig S1 – Please increase the fontsize in d and e and als that of the legends in c and f.

Fig S7 – See comment on Fig 4. And edit the caption Fig 5 -> Fig 4.

Fig S9 – p-value is hard to read.

Fig S11 – Please increase the fontsize in the insets.

Fig S12 – Can you plot the mean proportion of each regime?

Reviewer #3 (Remarks to the Author):

I appreciate the authors’ efforts in fully addressing all reviewers’ comments and suggestions and improving the manuscript. I suggest that the manuscript could be accepted for publication in current form.

Reply to the Reviewer #2 comments

At the outset, we would like to thank the reviewer for their constructive comments and suggestions to improve the manuscript. These suggestions and comments have really improved the structure and readability of the manuscript. Here we submit a point-by-point response to each of the points raised. We have submitted both a revised manuscript and a version with tracked changes, in which all modifications are highlighted in red and underlined for clarity. The original comments are in black *italic*, the responses are marked in blue, and the revised manuscript text is presented in blue *italic*.

The authors apply SVD to study the changes in the North Atlantic atmospheric circulation using the CESM-LE. They separately study the internal variability (minus the ensemble mean) and the full flow field of the 500hPa streamfunction and surface temperature. They apply changepoint analysis, identifying different behaviour before and after 1995, where after the forcing of global warming starts to impact the circulation.

For the period before and after 1995 they apply Gaussian Mixture Modelling to identify circulation regimes. For the full flow field they find that the number of regimes stays the same, although their patterns change. For the internal variability they find a reduction in the number of regimes from 4 to 3, with one regime disappearing. The regime analysis is followed by one on the NAO, for which a small increasing trend is found. This is linked back to the regime dynamics and through that to changes in the jet stream strength and location.

Overall, I find the study well executed and it shows important results that provide a detailed view of the atmospheric response to climate change. However, the writing and structure of many paragraphs at times is confusing and needs substantial rewriting to ensure clarity. I suggest the authors revisit their text and carefully assess the storyline, while aiming to make it more concise to get the main results across better.

Below I first provide my main concerns, followed by a list of minor comments and some specific points.

→ We are extremely grateful to the reviewer for taking the time to provide a thorough evaluation of our manuscript. Thank you for finding the study well executed and for providing constructive comments on our manuscript. Following the reviewer's suggestions, we have revised the manuscript to improve clarity and to ensure cohesiveness in the flow of the manuscript. Please find our point-by-point responses below.

Main comments:

1. In many places throughout the results section it is not clear whether you are discussing the results of the full flow field or those of internal variability, or only after reading ahead. I suggest introducing abbreviations for both (FF and IV maybe), which you can then use throughout to ensure it is clear which of the results you are discussing. Furthermore, the discussion can be made a lot more concise and concrete. E.g. throughout the discussion of the results there are sentence like in line 230, 242. These sentences are repeated often, do not add much to the discussion and disrupt from the story. I suggest removing them to make the story more concise. Instead focus on making the influence specific, what are the actual changes you find and how do they link to the literature. Doing this throughout would make the results section easier to read.

→ We thank the reviewer for this constructive suggestion. We have now implemented abbreviations, i.e., FF (full flow field) and IV (Internal variability), throughout the manuscript to explicitly distinguish between these two analyses. This has greatly improved the clarity and interpretability of the results. Furthermore, following the reviewer's suggestions, we have completely removed the repetitive lines and less informative sentences in the "Results" section. These revisions have made the presentation more concise, thereby strengthening the overall narrative and readability of the results.

2. The arguing for SVD in favour of EOFs is unclear to me. The way I understand it, it is because SVD analysis also allows to identify the forced global warming signal, whereas using EOFs this is less straightforward, which indeed is valuable. However, the paragraph (lines 111-132) in which this is discussed lacks a clear storyline. I suggest restructuring this paragraph, starting with a one sentence summary of why you use SVD over EOFs, followed by an elaboration going into some of the details and advantages. Also, it would be valuable to clarify that for SVD the sign of the pattern does matter.

→ We thank the reviewer for this helpful comment. Following the suggestion, we have restructured the paragraph (see lines **108-117**) to clearly justify the use of Singular Value Decomposition (SVD) over Empirical Orthogonal Function (EOF) analysis. The revised paragraph now states the motivation for employing SVD and the advantages in capturing coupled variability between the mid-tropospheric circulation and surface temperature fields, as well as its ability to isolate the forced response signal. Further, we have also clarified the importance of the pattern sign in SVD interpretation in the "**Data and Methods**" section (see lines **586-588**). Additionally, detailed discussions on the first two SVD patterns for FF and IV have been moved to the Supplementary Information for improved readability and more focus on the main text.

Lines 108-117: "*We then employ the Singular Value Decomposition (SVD) instead of Empirical Orthogonal Function (EOF) analysis to better capture the coupled variability between the mid-tropospheric circulation and surface temperature (refer to "Data and Methods"). While EOFs identify the dominant modes of variability within a single field, the SVD provides the framework to examine the co-variability between two fields. This coupling from the SVD framework enables isolating the forced response signal, which might not emerge clearly in single-field EOF analysis. However, previous studies, like Corti et al. (1999)[34] use the EOF analysis to base their results on the manifestation of the climate change signal on the Northern hemisphere atmospheric circulation regimes. Here, we elucidate the advantage of the coupled SVD analysis, which provides a clear illustration of the response of surface warming to the mid-tropospheric circulation."*

Lines 586-588: "*Additionally, unlike Empirical Orthogonal Functions (EOFs), the signs of the SVD patterns carry physical meaning as they indicate the phase relationship between the coupled variables."*

3. One of my main concerns is the assumption of piece-wise stationarity of the GMM. For the post-1995, when the climate is warming and the PC's all are increasing this is not a valid assumption, which is also shown in the regime results with a shift over time. While I appreciate this assumption is required to allow a thorough analysis, it does require better arguing and improved discussion. Furthermore, would it be an option to first detrend the data and then do the analysis.

→ We thank the reviewer for raising this important point regarding the assumption of piece-wise stationarity in the Gaussian Mixture Model (GMM) clustering. We agree that this assumption is an approximation, particularly for the Post-1995 period when anthropogenic warming introduces non-stationary behaviour in the principal components. To address this, we have expanded the discussion to explicitly acknowledge the limitation and evaluate its implications in the revised "**Summary and Discussion**" section (see lines **495-504**). To assist this discussion, we show the time evolution of the cluster proportions in **Fig. S12** (**Supplementary Information**) for the Pre- and Post-1995 periods, which clarifies that while the piecewise stationarity assumption holds reasonably well for the Pre-1995 period, it is less valid for the Post-1995 period, where changing cluster proportions indicate non-stationary responses under sustained warming. Despite this, the spatial structures identified by the GMM remain robust and recurrent across both periods, supporting the physical consistency of the identified circulation regimes. However, detrending the data would remove the externally forced component, which is central to our analysis, to compare the circulation patterns with and without external forcing, and hence is not applied here.

Lines 495-504: *"It is to be noted that we used the assumption of piecewise stationarity for our clustering analysis through GMM. We find that this assumption is reasonably valid for the Pre-1995 period, where cluster proportions remain stable over time (Fig. S12(a)). However, in the Post-1995 period, the clusters exhibit changing proportions over time, highlighting the non-stationary response of atmospheric circulation patterns under anthropogenic warming (Fig. S12(b)). Despite this non-stationary response, we find recurrent spatial patterns over the two periods, even with the inclusion of external forcing, indicating structural continuity. Hence, while we acknowledge that the stationarity assumption is violated for the Post-1995 period, the spatial patterns captured by the GMM are physically meaningful and consistent with the Pre-1995 period. This supports the consistency of our analysis in quantifying circulation changes across periods."*

4. The structure with first considering the SVD and regimes and only then looking at the NAO-index could also be switched around. At the moment the discussion on the NAO results comes out of the blue and at first does not appear to fit an overarching story. I suggest thinking through the storyline of the results and the best order in which to discuss them, as well as better linking the different sections.

→ We appreciate this suggestion from the reviewer. However, we would like to retain the current structure, where we discuss the atmospheric regimes first and then the changes in NAO. This order is methodologically consistent, as the regime-based cluster indices form the foundation for interpreting changes in the NAO and its behaviour within different regimes. To strengthen the connection between these two analyses, we now explicitly highlight in the "**Introduction**" section (see lines **63-38**) and at the beginning of the "**Results**" subsection, i.e., **NAO under Global Warming** (see lines **327-331**). Additionally, studies have shown that the atmospheric regimes are an imprint of Rossby waves, with the NAO being the remnant of breaking Rossby waves (Franzke, Lee and Feldstein, 2004). This clarification enhances the natural conceptual linkage of atmospheric regimes with the NAO changes. Further, we have refined the transitions across the sections to improve the manuscript's readability and logical flow.

Lines 63-68: ***"****There has been a lot of evidence regarding the emergence of the NAO from the internal dynamics of the extratropical atmosphere[44–46]. In particular, the NAO can be interpreted as a manifestation of large-scale, quasi-stationary Rossby wave patterns[43], which govern the preferred configurations of the midlatitude atmospheric flow. The NAO is inherently a part of the regime structure over the North Atlantic[20,43], representing one of the dominant modes through which Rossby wave dynamics project onto the regional circulation."*

Lines 327-331: ***"****The atmospheric regimes, including the NAO, are shaped by the dynamics of the Rossby waves, with the NAO representing the remnant of breaking Rossby waves[43]. The NAO is a key internal mode of variability in the climate system and a dominant atmospheric teleconnection pattern over the North Atlantic region. Therefore, understanding the role of anthropogenic climate change in influencing the internal mode of NAO is crucial."*

- *Franzke, C., Lee, S. and Feldstein, S.B. (2004) "Is the North Atlantic Oscillation a Breaking Wave?" Journal of the Atmospheric Sciences, 61(2), pp. 145–160. Available at: https://doi.org/10.1175/1520-0469(2004)061%253C0145:ITNAOA%253E2.0.CO;2.*

5. In the discussion section the results are not placed inside the wider literature on e.g. changes in the NAO in a changing climate or the link with the ocean cold blob. This needs to be done. The discussion could also benefit of some discussion on the physical mechanisms of the observed changes as well. It is currently written it is a conclusion and summary, but not as much a discussion.

→ We thank the reviewer for this insightful comment. Following the suggestion, we have now expanded the "**Discussion**" section, now renamed as "**Summary and Discussion**". In the revised manuscript, we explicitly place our findings within the context of wider studies, including studies on projected NAO changes under global warming and their connection to the North Atlantic "cold blob". We have also incorporated a detailed discussion on the underlying physical mechanisms driving the regime and NAO responses. These additions ensure that the section now provides a more comprehensive and interpretive view rather than a summary (see lines **475-547**).

Minor comments:

- *Your results are only valid for winter, so please specify that in the title.*

→ Thank you for this comment. Now, the revised title reads: "**Anthropogenic Climate Change Leads to a Pronounced Reorganisation of Wintertime North Atlantic Atmospheric Circulation Regimes**".

- *I suggest rewriting the abstract to clarify you separate the forced signal and internal variability and that the results differ, specifying how many regimes you find and rephrasing lines 10-12 as that sentence is hard to follow and it is unclear to me what you mean by "fingerprint". Also, the last sentence refers to "important implications", which you don't mention or refer to in the main text.*

→ We appreciate the reviewer's comment. We have rephrased the "**abstract**" (see lines **1-14)** to emphasise our main findings and the significant implications our results have within a broader scientific context. The implications of understanding the changes in the regime states in the future under continued warming have now been discussed in the "**Summary and Discussions**" section (see lines **503-509**).

Lines 1-14: "*Regional climate variability manifests through distinct atmospheric regimes influencing weather, climate, and predictability. Yet, their response to global warming remains unresolved. Using a hundred realisations from the Community Earth System Model Large Ensemble (CESM2-LE), we examine shifts in wintertime North Atlantic atmospheric regimes and the North Atlantic Oscillation (NAO) before and after 1995, marking the detectable influence of anthropogenic warming on atmospheric circulation. The large-ensemble framework isolates internal variability by removing the ensemble mean. Under anthropogenic* *warming, the number of regimes remains stable, but their associated circulation patterns reorganise, whereas internal variability alone shows a reduction in the number of regime states. Projections indicate a shift toward NAO-positive phases during the 21st century, alongside more frequent low-amplitude NAO-negative events late in the century. Anthropogenic warming also imprints a marked decline in the NAO variability, with strengthened mid-tropospheric westerlies in NAO-positive regimes and weakened westerlies in NAO-negative regimes in the future.*"

Lines 541-547: *"The changes in the frequency of occurrence and structure of atmospheric regimes alter large-scale temperature, precipitation, and wind patterns that enhance seasonal climate variability. These alterations can affect the frequency and severity of extreme weather events, modify regional energy demand and wind power capacity, and influence water resources through shifts in precipitation and runoff patterns. As a result, the evolving regime dynamics may have cascading impacts on agriculture, water management, and food security across the North Atlantic region."*

- *Can you explain why you decided to use the streamfunction instead of geopotential height?*

→ We thank the reviewer for this important comment. We use the streamfunction rather than geopotential height because our objective is to investigate the circulation regimes with (FF) and without (IV) external forcing. Geopotential height inherently reflects the thermal expansion of the atmosphere, and therefore won't be an appropriate metric to analyse the changes in the atmospheric circulation patterns, particularly for the FF. In contrast, streamfunction directly represents the volumetric flow of the atmosphere and is a more physically consistent field for studying large-scale circulation patterns. We have also included a cautionary statement regarding the use of geopotential height as a metric for studying large-scale circulation changes with external forcing in the "**Introduction**" section (see **lines 46-50**).

Lines 46-50: "*Furthermore, they attributed the northern hemispheric warming to natural circulation regimes' thermal structure rather than anthropogenic climate change influences. Although geopotential height is widely used to diagnose circulation changes, including in Corti et al. (1999)[34], interpretations based on this metric should be approached with caution under a warming climate due to the thermal expansion of the atmosphere."*

- *What are the bounds of your region and how did you decide on those? How sensitive are your results to the chosen region?*

→ We thank the reviewer for this question. The domain bounds chosen are 0-70°N and 98°W-12°E to encompass the full North Atlantic basin, including the adjacent western European and eastern North American regions, which are central to the North Atlantic Oscillation (NAO) and associated circulation regimes. These domain bounds of our analysis are mentioned in the "**Data and Methods**" section as well (see lines **525-528 and 614-616**). The longitudinal limits were selected sufficiently to capture these key domains. Our results are not expected to be highly sensitive to the modest changes in the regional boundaries, as the large-scale regime structures and NAO variability are spatially coherent across the North Atlantic sector.

Lines **563-566:** *"We perform a coupled Singular Value Decomposition (SVD) analysis[88] between the monthly mean streamfunction at 500 hPa and surface temperature on the full (FF) and internal variability (IV) flow fields from 1850 to 2100 (months from December to February only) over the North Atlantic region (0-70°N and 98°W-12°E)."*

Lines **614-616**: *"The EOF analysis is performed over the North Atlantic region (0-70°N and 98°W-12°E) for December to February from 1850 to 2100, after removing the monthly climatology and weighting according to the latitude."*

- *For the CESM-LE matching with ERA-5 data. Can you show the PDFs, both for the streamfunction, surface temperature and NAO index, as the variance of ERA-5 appears smaller. Also, can you show similar figures to Fig. S1 for the N. Atl. region you consider, to see how well it matches there? (I assume Fig. S1 is global, but it does not say this in the caption, so please check that and add to the caption.) It would also be valuable to know how much the spatial patterns of the variability deviate, e.g. how does the NAO compare? Do you expect an impact of this on your results? I would think it could primarily affect the internal variability signal.*

→ We thank the reviewer for this comment. We have now added the probability density functions (PDFs) for both the streamfunction at 500 hPa and surface temperature from ERA5 and CESM2-LE over the North Atlantic region in the revised "**Supplementary Information**"

(**Fig. S1**). As shown, the ERA5 distributions closely match the simulations from CESM2-LE, exhibiting near identical means and comparable spreads (**Fig. S1(a-b)**). We also show that the ERA5 time series for both variables lies well within the ensemble spread of the CESM2-LE, demonstrating the model's ability to capture the amplitude and temporal evolution of the observed variability. On comparing the variance across the ensemble members from the CESM2-LE with that of ERA5, we find the variance from ERA5 lies well within the ensemble spread, providing the representation of internal variability amplitude consistent with ERA5.

Additionally, the spatial patterns of the NAO derived from FF and IV in CESM2-LE are consistent with the observed NAO pattern, and the Hurrell PC-based NAO index lies well within the ensemble spread, supporting the robustness of our analysis. We have also clarified that the statistical comparison of ERA5 with CESM2-LE is over the North Atlantic region in the caption of **Fig. S1** in the **Supplementary Information**.

- *You refer to Fig. 1, which shows pre- and post-1995 results, before introducing the changepoint detection. This is confusing and I suggest restructuring.*

→ We thank the reviewer for pointing out this structural inconsistency. We acknowledge that the previous version could be confusing by introducing the changepoint detection, which serves as the basis for the temporal division of the data into Pre- and Post-1995 periods. Further, we restructured the paragraph, where the discussion on the SVD patterns and their associated principal components was shifted to the "**Supplementary Information**" section. The discussion in our "**Results**" section now begins directly with **Fig. 1** (see lines **118-133**), which presents the ensemble mean principal components of coupled streamfunction and surface temperature variability with identified change points.

Lines 118-133: *"Accordingly, we apply SVD between the streamfunction at 500 hPa and surface temperature across the one hundred ensemble member simulations to both the flow fields, i.e., FF and IV, for the period 1850 to 2100, to identify the two most dominant modes for both streamfunction at 500 hPa and surface temperature. The spatial patterns associated with the first two SVD modes (left and right singular vectors) for FF and IV, along with their corresponding principal components, are presented and discussed in the Supplementary Information.*

To further understand the role of anthropogenic forcing in modulating the temporal evolution of these coupled modes, we focus on the ensemble mean, i.e., averaging across the hundred realisations from the CESM2-LE, of the first two principal component time series, i.e., PC1 and PC2 from the FF (Fig. 1(c-d)), which represents the forced response linked to the 500 hPa streamfunction and surface temperature. To identify the anthropogenic influence on the mid-tropospheric circulation patterns, we apply a changepoint detection analysis to the first two principal components of the streamfunction at 500 hPa over the North Atlantic to identify the time when the slope changes (refer to "Data and Methods"). This change point determines the start of the anthropogenic global warming imprint on the atmospheric circulation in the North Atlantic (Fig. 1(c))."

- *Can you explain why you decided on using the changepoint of PC2 of the full flow field? And not PC1 or the internal variability ones? And in line with this, what would be possible physical reasons for the changepoints being found where they are? Is it changes in the ocean circulation, is it a threshold of sorts?*

→ We thank the reviewer for raising this important point. In our analysis, the changepoint detection is employed as a statistical diagnostic to identify the earliest detectable imprint of anthropogenic forcing on the coupled streamfunction and surface temperature variability, rather than to infer its physical cause. Among the ensemble-mean principal components (representing the forced response of the FF), PC2 represents the earliest statistically significant change point ($p<0.01$), whereas PC1 shows around 2038. We therefore select PC2 as the reference for dividing the data into the Pre- and Post-1995 periods, as it marks the earliest onset of detectable externally forced changes in atmospheric circulation over the North Atlantic. In contrast, the principal components of the IV show no significant trends, as the ensemble mean (forced response) has already been removed. We have rephrased these discussions (see lines **124-140**) in the revised manuscript. Also, we want to note that exploring the underlying physical mechanisms of this changepoint lies beyond the scope of the present study.

Lines 124-140: ***"****To further understand the role of anthropogenic forcing in modulating the temporal evolution of these coupled modes, we focus on the ensemble mean, i.e., averaging across the hundred realisations from the CESM2-LE, of the first two principal component time* *series, i.e., PC1 and PC2 from the FF (Fig. 1(c-d)), which represents the forced response linked to the 500 hPa streamfunction and surface temperature. To identify the anthropogenic influence on the mid-tropospheric circulation patterns, we apply a changepoint detection analysis to the first two principal components of the streamfunction at 500 hPa over the North Atlantic in order to identify the time when the slope changes (refer to "Data and Methods"). This change point determines the start of the anthropogenic global warming imprint on the atmospheric circulation in the North Atlantic (Fig. 1(c)).*

Notably, PC2 of the streamfunction at 500 hPa exhibits the earliest statistically significant change in slope around 1995 ($p<0.01$), marking the onset of anthropogenic warming influence on atmospheric circulation patterns in the North Atlantic (Fig. 1(c)). We therefore focus on the changepoint in PC2, as it provides the earliest detectable signal of the externally forced changes, preceding the response captured by PC1. Based on this detected changepoint, we divide our data into two distinct periods, i.e., Pre-1995 (1850 to 1995) and Post-1995 (1996 to 2100), to identify the number of atmospheric regimes in each of these two periods."

- *Currently the results on the regimes come very out of the blue and to me do not naturally follow from the discussion before. I suggest restructuring and starting a paragraph with the main question you want to answer by studying the regimes. In your initial reasoning it is unclear to me how Fig. 1 fits into that discussion.*

→ We thank the reviewer for raising this point. We have now restructured the paragraph, mentioning a brief outlook in our motivation to study the evolution of the atmospheric regimes under sustained greenhouse forcings. **Fig. 1** provides us with the temporal division of our data into Pre- and Post-1995 periods, which is used throughout the study, marking the onset of the detectable anthropogenic warming influence on the mid-tropospheric patterns over the North Atlantic region. To better interpret the flow and readability, please see the revised lines **150-156**.

Lines 150-156: *"Having identified the onset of anthropogenic influence around 1995 from the changepoint analysis on the streamfunction at 500 hPa (Fig. 1), we next examine how the large-scale atmospheric circulation responds to this external forcing in terms of its distinct regime states. Specifically, we aim to determine whether the number, frequency, or structure of* *these regimes changes between the Pre- and Post-1995 periods. Hence, to determine the number of atmospheric regime states during the Pre- and Post-1995 periods, we apply the Gaussian Mixture Model (GMM)[57] (refer to "Data and Methods") to the phase space defined by PC1 and PC2 of streamfunction at 500 hPa."*

- *Have you tried applying GMM to more of the PC's? E.g. up to PC5. And if not, why did you decide to only use the first two PC's?*

→ We thank the reviewer for this comment. We limited the Gaussian Mixture Model (GMM) analysis to the first two principal components (PCs), i.e., PC1 and PC2, because they together explain over 80% of the total explained covariance between the 500 hPa streamfunction and surface temperature fields for both FF and IV. This indicates that the leading two modes capture the dominant coupled variability between the mid-tropospheric circulation and surface warming. We therefore consider them sufficient for identifying the principal circulation regimes of interest. Hence, we have not tried extending the analyses to more PCs, as their contribution would be relatively small.

- *The order of the regimes is not aligned between the pre- and post-1995 data. It would significantly help readability if they would be aligned (with the same colour).*

→ Thank you for pointing this out. All the revised figures in the revised manuscript (wherever necessary, such as **Figs. 2, 6, and 8**) are arranged now with a similar colour, specifically the regimes having structural similarity or recurrent ones are now paired with the same colour combination across both Pre- and Post-1995 periods.

- *The abbreviation FRS is confusing to me, as it is the most frequent regime state, whereas frequent on its own does not make that sufficiently clear.*

→ We thank the reviewer for this suggestion. We have now replaced the **Frequent Regime State (FRS)** with the **Most Frequent Regime (MFR)** throughout the manuscript. This revision better reflects the intended meaning, as the regime with the maximum number of data points represents the most dominant or frequently occurring circulation regime.

- *The "gradual shift" (line 206) is hard to see from Fig. 2. Can you make this more clear in the plot, also that there is a time component to this? And how does this align with the stationarity assumption?*

→ Thank you for this comment. To represent the "**gradual shift**" in a more evident way, we have added the mean lines in the scatter plot of PC1 (X-axis) and PC2 (Y-axis) for the Pre- and Post-1995 periods in **Fig. 2(a-b)** for FF. In the Pre-1995 line, the mean lines for both PC1 and PC2 are below zero, whereas in the Post-1995 period, they lie above zero, clearly illustrating the gradual shift in the phase space in the Post-1995 period. This visual shift is also corroborated by the increasing trends in PC1 and PC2 after 1995 (**Fig. 1(c-d)**). Furthermore, we have rephrased the corresponding paragraph (see lines **192-199**) in the revised manuscript to clarify this reasoning and emphasise this temporal evolution. For the discussion on the stationarity assumption, please refer to our response to the reviewer's 3rd Main comment.

Lines 192-199: *"The number of atmospheric regime states in both the Pre- and Post-1995 periods for the FF remains consistent, with four unique regime states identified both before and after 1995, i.e., Pre- and Post-1995 periods (Fig. 2(a-d)). However, in the Post-1995 period, a clear displacement in the mean line towards higher PC1 and PC2 values emerges, highlighting a systematic shift in the phase-space distribution of circulation regimes under anthropogenic warming (Fig. 2(b)) with no change in the number of regime states. This is corroborated by the rising trend in the PC1 and PC2 of the 500 hPa streamfunction arising due to the influence of surface warming on the mid-tropospheric circulation (Fig. S3(a-b) and Fig. 1(c))."*

- *In line 211-212 you refer to results you will discuss later, without making this specific. This is done multiple times in the results section and makes reading confusing at times. I suggest specifically saying you will discuss this later in Sec. ….*

→ We thank the reviewer for this suggestion. We acknowledge that in the previous version of the manuscript, several sentences referred to future results without specifying their corresponding sections. In the revised version, we have carefully removed such instances to improve clarity and readability throughout the manuscript.

- *Discussion in Lines 203-224. In the first paragraph you call the full flow field regimes steady, to at the end of the second paragraph say they are changing. This does not align. While I appreciate your likely referring to the patterns, you have not discussed those yet. Also, in the last sentence you say "true physical shift". How do you assess whether this is the case or not?*

→ We thank the reviewer for this point. We acknowledge this potential confusion in the description of the number of regime states for the FF, and hence we have removed the concerned lines and have modified them in the revised manuscript (see lines **202-208**). This revision now ensures conceptual consistency while improving the flow of our storyline.

Lines 202-208: *"In contrast, the IV shows a reduction in the number of clusters, from four to three in the Post-1995 period (Fig. 2(e-h)). The reduction in the number of uniquely identifiable regime states for the IV suggests fewer, more dominant regime states in the later period. These findings are validated from the donut chart illustrating the distribution of the data points across the clusters in the Post-1995 period associated with the IV (Fig. 2(f)); where the MFR (Cluster 23) for the Post-1995 period constitutes more than half (52.8%) of the total distribution, highlighting the dominance of the MFR in the later period (Fig. 2(f))."*

- *Throughout the discussion of the results there are sentence like in line 230, 242. These sentences are repeated often, do not add much to the discussion and disrupt from the story. I suggest removing them to make the story more concise. Instead focus on making the influence specific, what are the actual changes you find. Doing this throughout would make the results section easier to read.*

→ We thank the reviewer for this suggestion. We have now removed these concerned lines to ensure structural consistency and focus, while making our discussion on the results more straightforward and specific.

- *The "southward displacement" (line 234) is hard to see, maybe plot overlapping contours? Also, what do you mean with "conventional"?*

→ We thank the reviewer for this comment. By "**southward displacement**", we refer to the relative shift of the cyclonic and anticyclonic centres compared to their canonical positions in the North Atlantic Oscillation (NAO) pattern, typically located over Iceland and the subtropical North Atlantic. The term "conventional" thus denotes this canonical NAO flow structure, which has been added now in the concerned sentences (see lines **216-220**) in the revised manuscript. Additionally, we have not added overlapping contours, as the figure depicts the spatial patterns of the MFRs, and adding additional contours would deviate from the intended narrative focus of these regime structures.

Lines 216-220: *"The MFR of the mid-tropospheric circulation for the FF during the Pre-1995 period (Fig. 3(a)) exhibits southward displacement of cyclonic and anticyclonic centres relative to their conventional arrangement over Iceland and the subtropical North Atlantic. Both centres being displaced towards lower latitudes indicates a deviation from the canonical North Atlantic flow structure, i.e., NAO (Fig. 3(a))."*

- *For the internal variability after 1995 you only find 3 regimes. In line 257 you mention the corresponding patterns have a reduced loading. To me this is to be expected as less regimes, means more data per regime and thus a centroid that considers more data, automatically weakening the pattern. Or am I wrong?*

→ We appreciate this comment from the reviewer. We also acknowledge that the earlier phrasing using "**reduced SVD loadings**" and "**reduction in the loadings of the SVD patterns**" could lead to misinterpretation, as the SVD patterns are derived by projecting the cluster centroids in the phase space (PC1-PC2) onto the corresponding SVD patterns (left and right singular vectors) of the 500 hPa streamfunction and surface temperature. To avoid these confusions, we have removed these phrases from the revised manuscript (see lines **234-242**). Further, we have added a dedicated subsection "**Projection of Cluster Centroids onto Singular Value Decomposition Patterns**", which now clearly illustrates how the spatial patterns are obtained and indicates that these patterns are independent of the number of data points per cluster, since each pattern reflects the position of the centroid in the SVD-defined phase space.

Lines 234-242: *"On comparing the MFR for the IV between the Pre-1995 and Post-1995 periods, we observe a substantial weakening of the mid-tropospheric circulation in the latter, i.e., Post-1995 period (Fig. 3(g)). Correspondingly, the variations of the surface temperature are also weaker for the MFR without external forcing (Fig. 3(h)). We note an increased dominance of the MFR, along with a weakened mid-tropospheric circulation and surface temperature signature in the Post-1995 period for the IV. However, with the inclusion of the external forcing, i.e., the FF, we find a spatial reorganisation of the dominant atmospheric circulation patterns, as evidenced by the northward displacement of the cyclonic and anticyclonic centres, indicating that anthropogenic forcing modulates the position of circulation centres over the North Atlantic."*

- *I do not see how you arrive at the conclusion in line 260.*

→ We thank the reviewer for pointing this out. The concluding sentence in the concerned paragraph in the previous version of the manuscript (see lines **259-260**) is mentioned to highlight the contrast in the spatial patterns of the MFRs for both FF and IV. In the case of FF, we find that external forcing reorganises the circulation patterns in the future, whereas for IV, the influence of anthropogenic warming on the internal mode of mid-tropospheric circulation patterns leads to a dominant weakened response. We acknowledge that this underlying contrast was not clearly articulated in the previous version of the manuscript, which has been rephrased in the revised manuscript (see lines **234-242**), as mentioned in the previous response to the reviewer's comment.

- *When discussing the changes in regime patterns, you mention structural similarity. Can you make this concrete, by e.g. computing the distance or pattern correlation between the regime patterns. And what then would be the physical interpretation of these changes*

→ We thank the reviewer for this suggestion. We have now computed the pattern correlation coefficients between regimes that exhibit spatial similarity for both the FF and IV. The results are discussed in the revised manuscript to provide a quantification measure (see lines **249-255 for FF** and lines **262-270 for IV**). Regimes, by definition, represent recurrent circulation states; thus, structural similarity between corresponding regimes is expected. Physically, the observed differences primarily manifest as meridional or zonal displacements in the centres of cyclonic and anticyclonic activity, indicating reorganisation, as well as localised strengthening or weakening of these centres, rather than as fundamental changes in regime structure. These aspects are explicitly discussed throughout the revised manuscript to clarify the physical interpretation of the regime changes.

Lines 249-255: *"Along with the SR, the spatial patterns associated with the MFRs for both the Pre- and Post-1995 periods show a resemblance in their structural similarity, i.e., Cluster 11 with Cluster 22 (Fig. 3(a-d)). To quantify this structural similarity, we compute the spatial pattern correlation coefficients between the corresponding clusters of the two periods. The resulting correlations ($r_{11 \text{ and } 22} = 0.80$, $r_{12 \text{ and } 24} = 0.87$, $r_{13 \text{ and } 23} = 0.98$, and $r_{14 \text{ and } 21} = 0.62$) indicate that dominant circulation patterns over the North Atlantic region remain, albeit with some changes in their intensity, frequency, and precise organisation caused by external forcing."*

Lines 262-270: "*The MFR and SR for the internal variability flow field also show structural similarity across the Pre- and Post-1995 periods. For example, the MFR in the Pre-1995 period, i.e., Cluster 13, aligns well with Cluster 22 (Fig. 3(e-f) and Fig. S6(i-j)), both exhibiting strong negative surface temperature anomalies over the Baffin Bay along with a strong anticyclonic circulation over the subtropical Atlantic Ocean ($r_{13 \text{ and } 22} = 0.93$). Similarly, Cluster 12 in the Pre-1995 period shows structural resemblance with that of Cluster 21 in the Post-1995 period ($r_{12 \text{ and } 21} = 0.99$; Fig. S6(c-d and g-h)). Moreover, we observe that the second most dominant regime state in the Pre-1995 period (Cluster 11, Fig. S6(a-b)) becomes the MFR in the Post-1995 period (Cluster 23, Fig. 3(g-h)), but with weakened amplitude and increased frequency of occurrence ($r_{11 \text{ and } 23} = 0.99$)."*

- *The discussion in lines 272-280 feels out of place after already having drawn a conclusion on the changes, I suggest re-ordering.*

→ Thank you for this suggestion. We agree that the discussion in lines **272-280** of the previous version of the manuscript is inconsistent with the manuscript's flow. Hence, we have now shifted this discussion to the changes in the Secondary Regimes (SR) observed for the FF in the revised manuscript (see lines **256-261**).

Lines 256-261: *"Additionally, we observe an increased frequency of events belonging to the cluster that corresponds to extreme NAO-negative-like circulation and surface temperature patterns in the Post-1995 period for the FF (7.36%; Fig. S5(i-j)) relative to the Pre-1995 period (4.35%; Fig. S5(c-d)). This finding demonstrates that even under anthropogenic forcing, episodes of widespread cooling can occur as a part of intensified extreme circulation regimes. Further, supporting the claim that anthropogenic warming is amplifying the intensity of future extreme events."*

- *I do not see how you can draw the conclusion in line 279-280 based on the discussion before.*

→ Thank you for this suggestion. Please refer to our response to the previous comment, wherein we have rephrased the sentences (see lines **256-261**) to highlight the role of anthropogenic warming in intensifying extreme events in the future.

- In line 297 you say "revealing a clear physical circulation behaviour" linked to the opposite polarity. What sort of behaviour? This is unclear.

→ We thank the reviewer for pointing this out. We acknowledge that the usage of the term "**revealing a clear physical circulation behaviour**" makes the discussion of the result potentially confusing. We have therefore rephrased the concerned lines to provide a clearer description in the revised manuscript (see lines **276-281**). Specifically, we state that Cluster 14 exhibits spatial similarity to that of Cluster 12 but with opposite circulation patterns. Based on the positioning of the cyclonic and anticyclonic centres and associated surface temperature patterns, we identify this regime as representing the Eastern America pattern. The revision now clarifies this aspect.

Lines 276-281: *"However, a closer inspection of Cluster 14's spatial structure exhibits similarities to Cluster 12 but with opposite circulation patterns (Fig. S6(e-f and c-d)). Given the positioning of both the cyclonic and anticyclonic circulation centres for Cluster 14, along with its corresponding surface temperature variability, the atmospheric regime state is physically meaningful, representing the Eastern America pattern."*

- I do not understand how you arrive at the conclusion that "the potential disappearance of Cluster 14 is due to physical suppression and is not a statistical artefact" (lines 300-301) from the discussion before?

→ We thank the reviewer for this comment. From our GMM-based cluster analysis of the IV, we observe a clear reduction in the number of regime states from the Pre-1995 period to the Post-1995 period. All regimes identified in the Pre-1995 period reappear in the Post-1995 period, with varying frequencies, except for Cluster 14, which represents the Eastern America pattern and is the least frequent regime in the Pre-1995 period. The disappearance of this regime in the Post-1995 period is not a statistical artefact, as confirmed by the PERMANOVA tests that establish statistical separability among the identified clusters (**Fig. 2**). Therefore, we attribute the disappearance of Cluster 14 to physical suppression linked to anthropogenic climate change rather than a statistical artefact. To make this notion better, we have rephrased these reasons (see lines **281-285**) in the revised manuscript.

Lines 281-285: *"Importantly, our cluster analysis using GMM together with PERMANOVA tests demonstrates that the cluster centroids are statistically well separated (Fig. 2). Hence, the potential disappearance of Cluster 14 is due to physical suppression, i.e., reflecting the imprint of anthropogenic warming on the internal mode of mid-tropospheric circulation, and is not a statistical artefact."*

- *On the NAO index. As currently written it is unclear whether you compute the PC for each ensemble member separately, or for the full ensemble in one go. Can you clarify this?*

→ We thank the reviewer for pointing out the need for clarification. We have now clarified this point by adding a separate subsection, "**Empirical Orthogonal Function-based Derivation of the NAO Index",** in the "**Data and Methods**" section (see lines **609-621**). Specifically, the Empirical Orthogonal Function (EOF) analysis used to derive the NAO index is performed on the full ensemble in one go, not separately for each ensemble member. The procedure closely follows that of the Singular Value Decomposition (SVD) analysis described earlier in the manuscript. The only difference is that, instead of computing a cross-covariance matrix between two geophysical variables, the EOF analysis constructs a covariance matrix for a single variable, here, the sea-level pressure (SLP) field. Further, we have also rephrased the concerned lines (see lines **332-338**) in the revised manuscript to improve readability.

Lines 332-338: "*There are several methods to compute the NAO index*[67–70]*; here, we use the standardised first principal component of the sea-level pressure flow field (seasonality removed) from 1850 to 2100 (December to January) over the North Atlantic region as our NAO index. The NAO index time series is computed for all the hundred ensemble member simulations. This computation follows the procedure which is analogous to that used for the coupled SVD between the streamfunction at 500 hPa and surface temperature, ensuring methodological consistency across analyses within the large-ensemble framework (refer to "Data and Methods")."*

- *You look at the distribution of the NAO phases pre- and post-1995. Since the regime patterns change, wouldn't you also expect the NAO to change, i.e. the pattern to change with the regime change? Can you comment on this.*

→ We appreciate this important comment from the reviewer. As shown by our findings on the evolution of regimes throughout the 21st century, we observe no change in the number of regime states with the external forcing (FF). Most importantly, the regimes identified for FF display recurrent characteristics, mainly involving a reorganisation of circulation patterns, i.e., zonal and meridional displacements of cyclonic and anticyclonic centres, rather than the emergence of new regime types. These dynamical adjustments in the regime patterns are reflected in future changes in the NAO, where we observe an increased frequency of NAO-positive phases. Nevertheless, the spatial pattern of the NAO under FF remains structurally identical to that of the IV, indicating that the externally forced response is not strong enough to alter the fundamental NAO structure, but rather modulates its phase frequency and persistence.

- *The statement on the "consistent shift of regime states towards a more positive NAO phase" (lines 362-363) is hard to see from the figure. Can you make this clearer by, e.g. indicating the maximum of the other distributions in the figure?*

→ We thank the reviewer for this suggestion. To improve clarity, we have revised the corresponding paragraph and added the following explanatory line in the revised manuscript (see lines **396-398**). We have not added additional reference lines in the figure, as this textual clarification now makes the phase shift clearly interpretable from the displayed distributions.

Lines 396-398: *"All clusters in the Post-1995 period exhibit a displacement of their mean values away from the zero-line compared to the Pre-1995 period, indicating a systematic tendency toward the NAO-positive phase."*

- *Around line 424, it took me a while to realise you use the regimes to look at the impact of the jet stream on the NAO. It would be good to mention this earlier.*

→ We thank the reviewer for this comment. We have rephrased and added an explanatory line to clarify that the regime states are further used to investigate the influence of the jet stream variability on the NAO (see lines **442-449**). This addition improves the logical flow and makes the subsequent discussion on the mid-tropospheric jet and NAO linkage clearer to the reader.

Lines 442-449: *"Having established a projected decline in the variability of the NAO index, we next investigate how the identified circulation regimes can be used to examine the impact of the mid-tropospheric jet stream on NAO variability. Specifically, we assess how increasing NAO-positive events accompanied by declining NAO-negative conditions in a warming world would dynamically respond to changes in the mid-tropospheric jet stream patterns over the North Atlantic region. We examine the behaviour of mid-tropospheric westerlies (positive zonal winds at 500 hPa) to characterise the zonal jet stream across different circulation regimes in the Pre- and Post-1995 periods (Fig. 8)."*

- *You mention a decline of the westerly wind strength (line 441), but looking at the figure I also see shifts. Is that correct and can you comment on that?*

→ We thank the reviewer for this marked observation. We agree that our findings also reveal shifts in the jet core regions for regimes under both NAO-positive and negative phases. For regimes having a dominant NAO-positive distribution, we identify strengthening of the mid-tropospheric westerly jet accompanied by a poleward shift of its core position. Whereas, for the NAO-negative regimes, the mid-tropospheric westerly jet weakens in the future, followed by an equatorward shift of the jet core. We have now added these relevant findings in the revised manuscript (see lines **459-461 and 465-466**).

Lines 425-427: *"In addition to this strengthening, the westerly jet shows a slight poleward shift in its core, consistent with the northward displacement of the mid-latitude storm track under warming conditions[75–78]."*

Lines 431-432: *"Along with the weakening of the mid-tropospheric westerlies for NAO-negative regimes in the future, we observe a slight equatorward shift of its westerly core."*

- *Can you clarify what you mean by "transitional NAO phases" in line 447?*

→ We thank the reviewer for this comment. We have rephrased the concerned phrase (see lines **470-472**) to clarify that our results demonstrate a clear linkage of the mid-tropospheric jet stream and the NAO, encompassing both its positive and negative phases under global warming.

Lines 470-472: *"Collectively, these results demonstrate a clear linkage of the mid-tropospheric jet stream with the NAO, encompassing both positive and negative phases under global warming."*

- *In line 462, what do you mean by "the regime states intensify"? And how can I see this from the discussion in the results section?*
- *The discussion is the first place you mention extreme NAO events (line 464). Please mention this in the results section, linking it to the figures.*
- *Can you make the statement "reveal distinct shifts in regime characteristics" (line 474) more concrete?*
- *In line 489 you mention "previous studies". Please give those (also see my main comment on the discussion).*
- *How dependent do you think your result on the specific warming scenario you use? Can you discuss this in the discussion.*

→ We thank the reviewer for all of these comments concerning the "**Discussion**" section. Please find our response to the reviewer's 4th main comment. We have now completely recontextualised the "**Discussion**" section into "**Summary and Discussion**" in the revised manuscript. This section has undergone the maximum amount of changes to ensure that it provides a more comprehensive and interpretive view of our findings placed with respect to the wider available literature. Most of the less informative and potentially confusing phrases, as pointed out by the reviewer, have been removed and rephrased, including proper citations have also been added wherever necessary. Please see lines **475-547**, wherein we present a modified "**Summary and Discussion**" of our findings.

- *You refer to the "recurrent characteristics" of the regimes (line 466). Can you also comment on their dynamics, i.e. how does the circulation transition from one regime to another and how persistent are they?*

→ We appreciate this comment from the reviewer and feel that exploring these caveats would strengthen the manuscript. We would want to clarify that our definition of "r**ecurrent**" is based on the repeatability of circulation patterns across the Pre- and Post-1995 periods, rather than on a transition from one regime to another. Regarding the inter-regime transition and persistence of the regimes for FF and IV, we now explicitly discuss these dynamics by analysing the inter-regime transition pathways and regime persistence using a Markov transition matrix for both FF and IV (**Fig. 4 and Fig. S7**). Our results reveal preferred transition pathways and distinct persistence characteristics. We have added an extra discussion on this new analysis in the revised version of the manuscript (see lines **286-316**).

Lines 286-316: "*Besides our findings on the evolution of the regimes and their associated spatial characteristics, we next examine how these dynamical patterns translate into inter-regime transitions and persistence. The transition networks for the FF and IV (Figs. 4 and S7) provide a dynamical view by quantifying how frequently the regimes sustain or transition among themselves.*

For the FF (Fig. 4(a-d)), the MFR in each period also emerges as the most persistent regime, as indicated by the maximum value from the diagonal elements of the Markov transition matrix (refer to "Data and Methods"). This joint dominance in both frequency and persistence indicates that this circulation state becomes more dynamically stable, as evidenced by the increased persistence value (0.53) under anthropogenic warming (Fig. 4(b)). As noted earlier, the MFR across the Pre- and the Post-1995 periods retains a structurally similar pattern with pronounced northward shift of the cyclonic circulation in the latter, further clarifying the move towards "regime locking" in future.

Moreover, the inter-regime transition pathways for the FF highlight how the dynamical connectivity among the regimes changes under anthropogenic warming. In the Pre-1995 period, transitions from the SR to the FR are relatively strong, indicating a tightly interconnected phase-space structure in which the SR frequently evolve into MFR (Fig. 4(a-b)). However, in the Post-1995 period, these pathways weaken, indicating that the MFR is reached less frequently from other regimes (Fig. 4(c-d)). The structurally similar pairs, or the recurrent regimes across the Pre- and Post-1995 periods, as identified earlier, also exhibit a systematic weakening of their internal connectivity. For instance, the transition probability from Pre-1995 Cluster 13 to Cluster 12 (0.27) decreases in the Post-1995 period to 0.21 for the analogous pair (Cluster 23 to Cluster 24) (Fig. 4(a and c)). A similar weakening occurs in the transitions from the MFR to its recurrent counterpart, with the MFR to Cluster 12 transition in the Pre-1995 period declining relative to the corresponding MFR to Cluster 24 pathway under sustained warming (Fig. 4(a-c)).

For the IV (Fig. S7), we identify that the inter-regime transition structure exhibits differences from that of the FF. Notably, the transition strength from the SR to the MFR increases in the Post-1995 period relative to the Pre-1995 period, indicating a greater tendency for the regimes to be directed towards the dominant weakened mid-tropospheric circulation state for IV (Fig. S7(a-d)). This shift is dynamically consistent with the reorganisation of the MFR itself in the Post-1995 period, wherein the physical suppression of Cluster 14 (Eastern American pattern) leads to an increase in the inter-regime connectivity. However, both the IV and FF show a consistent increase in MFR persistence during the Post-1995 period. (Fig. S7(b and d)).”

- *In Data and Methods, in the SVD section, it would aid if you in one or two lines discussed the physical interpretation of SVD compared to EOFs. How should people think about this?*

→ We thank the reviewer for this comment. Please refer to our response to the 2nd main comment by the reviewer, wherein we have briefly stated our modifications in the revised manuscript.

- *In the section on the GMM, you need to mention x is a data point, as is z? And what i and k represent in (2). Also, should the | in eqn. (5) be there, and if so why?*

→ We thank the reviewer for pointing this out. We have now completely revised the subsection "**Gaussian Mixture Model**", detailing each notation, under the "**Data and Methods**" section (see lines **622-648**) to explicitly state the fitting of GMM onto the first two principal components of the streamfunction at 500 hPa for both Pre- and Post-1995 periods, to capture the changes in atmospheric circulation regimes under external forcing. Further, to perform GMM clustering, we use the "fitgmdist" function in MATLAB, which uses the Expectation-Maximization algorithm to optimise the model parameters through maximum likelihood estimation.

- *Can you expand the discussion on the BIC? As it is not that straightforward to compute, e.g. what are the dimensions you use?*

→ We thank the reviewer for this comment. We clarify that we do not explicitly compute the Bayesian Information Criterion (BIC). The BIC values are obtained explicitly from the "**fitgmdist**" function in MATLAB, which returns the BIC for each fitted GMM configuration or cluster. We have now added a sentence in the revised manuscript (see lines **656-658**) to ensure clarity on how the BIC value is obtained.

Lines 656-658: *"The BIC values for each cluster fit are returned explicitly from the "fitgmdist" function in MATLAB, which has been discussed in the previous section."*

- *Would it be an option to make the code publicly available on e.g. Zotero? And if not, why?*

→ We thank the reviewer for this comment. The codes and relevant datasets required to reproduce the figures will be made publicly available through the Zenodo repository upon acceptance of the manuscript.

- *Figures: The location of the streamfunction patterns in Fig. 1 is confusing, as it appears one is about the streamfunction and the other about the temperature, while they are the streamfunction pre- and post-1995. In line with one of my minor comments, it would help to align the colors in Fig. 2, 5, and 7. Please check there are units at each plot! E.g. Fig. 3 is missing them. Also, please make the fontsize larger, especially for the colorbars and donut charts, but also for some labels such as the changepoint years or the Full flow* *field/Internal variability in Fig. 6. What do you mean by "transitional behaviour" in Fig. 7?*

→ We thank the reviewer for this suggestion. All figure-related modifications, including increasing the font size of colorbars and aligning the colours of the regimes for consistency, have been implemented. Furthermore, we have added an extra figure on our analysis on the inter-regime transitions and persistence, numbered **Fig. 4**. We also clarify that the SVD and EOF patterns are unitless; hence, no units are displayed in **Figs. S2 and 5**. Similarly, the spatial patterns of the cluster regimes are also unitless, as the cluster centroids are projected onto their corresponding SVD patterns (**Figs. 3, S5, and S6**).

Specific feedback:

Line 10 – Decline in the number of regime states.

→ Fixed. And, the abstract has been rephrased substantially to ensure more clarity on our findings.

Line 18 – Greenhouse gas -> greenhouse gases

→ Fixed in line **18**.

Line 21 – It is unclear to me what you mean by "shaping climate responses". Please clarify.

→ Thank you for this comment. The concerned phrase has been replaced (see lines **20-22**).

Lines 20-22: *"The subtle interaction of the internal modes of climate variability with the forced response plays a crucial role in determining how the climate evolves and adds complexity to accurate climate predictions[1–3]."*

Line 86 – It is unclear where "this region" refers to. Please clarify.

→ Thank you for this comment. Here, "this region" specifically refers to " the North Atlantic region". We have clarified this in the revised manuscript (see lines **89-91**).

Lines 89-91: *"The SSP3.70 scenario[51], representing a high-emission pathway and strong regional disparities with limited climate mitigation, provides a valuable framework to assess how* *anthropogenic warming can reshape large-scale atmospheric circulation over the North Atlantic region."*

Line 106 – The sentence starting here is unclear to me. Please rephrase.

→ Thank you for this suggestion. We have substantially remodified the starting lines (see lines **118-121**) of the "Results" section to ensure clarity and improve readability. Specifically, we have moved the discussion of the SVD patterns for FF and IV to the Supplementary Information and improved the concerned lines.

Lines 118-121: *"Accordingly, we apply SVD between the streamfunction at 500 hPa and surface temperature across the one hundred ensemble member simulations to both the flow fields, i.e., FF and IV, for the period 1850 to 2100, to identify the two most dominant modes for both streamfunction at 500 hPa and surface temperature."*

Line 139 – The word "shift" to me suggests a change in time, which is not the case here. Please rephrase.

→ Thank you for this comment. We have replaced the word "**shift**" with "**northward and eastward displacement in the location of the cyclonic circulation**" for clarity. The revised wording for the concerned sentence can now be found under the Supplementary text in the Supplementary Information (Coupled Singular Value Decomposition Patterns for the Full and Internal Variability Flow Fields).

Line 142 – As for "shift", but here with "rearrangements".

→ Thank you for this comment. We have replaced the word "rearrangements" with "reorganisation" for improved clarity. The revised wording for the concerned sentence can now be found under the Supplementary text in the Supplementary Information (Coupled Singular Value Decomposition Patterns for the Full and Internal Variability Flow Fields).

Line 205 – You say the regime states are "steady", while their patterns to appear to shift. Only their number is steady. Please rephrase.

→ Thank you for this comment. We have modified the concerned lines (see lines **192-194)** in the revised manuscript.

Lines 192-194: "*The number of atmospheric regime states in both the Pre- and Post-1995 periods for the FF remains consistent, with four unique regime states identified both before and after 1995, i.e., Pre- and Post-1995 periods (Fig. 2(a-d)).*"

Line 254 – Please avoid terminology like "loadings of SVD patterns", as it is unclear to a non-specialised audience. Same for "polarity" in line 296.

→ Thank you for this comment. We have removed these terminologies throughout the manuscript and replaced them with clearer and more general expressions. E.g. "Polarity" replaced with "Opposite circulation patterns".

Line 292-293 – This sentence is unclear to me. Please rephrase.

→ Thank you for this comment. We have rephrased the sentence in lines **273-274**.

Lines 273-274: *"As previously stated, while the number of regime states of the IV reduces, the MFR becomes more dominant in frequency during the Post-1995 period."*

Line 479-482 – These two sentences make pretty much the same statement. Please remove one for conciseness.

→ Fixed and removed the repetitive line.

Caption Fig. 1 – Lines 830-831 repeat the one before. Remove for clarity. It would help to name the other changepoints in the caption.

→ Fixed and removed the repetitive line. Also, included the change points for the corresponding Principal components of 500 hPa streamfunction and surface temperature for clarity in the figure caption (see lines **956-958**).

Lines 956-958: *"Additionally, we identify a change point for PC1 of the streamfunction at 500 hPa in 2038, with 1982 for PC2 of surface temperature and 1991 for PC1 of surface temperature."*

Reply to the Reviewer #3 comments

We would like to thank the reviewer for their insightful comments and suggestions. Below, we provide a point-by-point response to each of the points raised. We have submitted both a revised manuscript and a version with tracked changes, in which all modifications are highlighted in red and underlined for clarity. The original comments from the reviewer are in black *italic*, the responses are marked in blue, and the revised manuscript text is presented in blue *italic*.

In this manuscript, the authors have investigated the response of North Atlantic atmospheric circulation regimes to anthropogenic global warming. By decomposing the circulation into a forced response and internal climate variability, the authors suggest that anthropogenic global warming will reorganize the North Atlantic atmospheric circulation regimes. The manuscript is well written and is worthy of publication. I think it can be further improved by considering following suggestions.

→ We sincerely thank the reviewer for their positive and constructive evaluation of our manuscript. We are pleased that the reviewer found the study to be well-executed and worthy of publication. Following the reviewer's suggestions, we have carefully revised the manuscript to enhance clarity and strengthen the overall presentation of our results. A detailed point-by-point response is provided below.

Major points:

1. Why does the ensemble mean represent the forced response, which means that the internal variability (e.g., NAO) have been fully eliminated? This issue is the basis of this study. I suggest the authors give a clear explanation.

→ We thank the reviewer for this comment. In large-ensemble simulations, such as the Community Earth System Model version 2 (CESM2-LE), all ensemble members are subjected to identical external forcing (greenhouse gases, aerosols, solar variability, and volcanic activity), but are initialised with slightly perturbed initial conditions (Rodgers *et al.*, 2021). These perturbations generate different realisations of the internal variability while preserving the externally forced component. Averaging across a sufficiently large ensemble of simulations effectively cancels out the uncorrelated internal variability, thereby leaving the ensemble mean to represent the forced response (Lehner, Deser and Terray, 2017; Deser *et al.*, 2020).

In this study, we decompose the atmospheric patterns and the NAO into two categories: (i) the full flow field (FF), which combines internal variability and forced response, and (ii) the internal variability flow field (IV), which represents the patterns without external forcing, obtained by subtracting the ensemble mean from each ensemble member. In **Fig. 6(c)**, we show the ensemble mean of the indices associated with the FF, which is the forced response component of the NAO index. Concurrently, in **Fig. 6(e)**, we show the NAO index for the IV, where the external forcing is effectively eliminated. To improve clarity on the insights of large-ensemble simulations, we have now added a brief explanation (see lines **553-560**) in the revised main manuscript

Lines 553-560: *"In large ensemble frameworks, such as CESM2-LE[36], each ensemble member is subjected to identical external forcing (e.g., volcanoes, anthropogenic emissions of greenhouse gases and aerosols) but initialised with slightly perturbed initial conditions. These perturbations lead to different realisations of internal variability, while the externally forced signal remains common to all the ensemble members. Averaging across the ensemble members effectively cancels out the internal variability, allowing the ensemble mean to isolate the forced response[36,37,52,53]. Consequently, the deviations of individual ensemble members from the ensemble mean represent the modelled internal climate variability[36,37,52,53]."*

- *Deser, C. et al. (2020) "Insights from Earth system model initial-condition large ensembles and future prospects," Nature Climate Change, 10(4), pp. 277–286. Available at: https://doi.org/10.1038/s41558-020-0731-2.*
- *Lehner, F., Deser, C. and Terray, L. (2017) "Toward a New Estimate of 'Time of Emergence' of Anthropogenic Warming: Insights from Dynamical Adjustment and a Large Initial-Condition Model Ensemble," Journal of Climate, 30(19), pp. 7739–7756. Available at: https://doi.org/10.1175/JCLI-D-16-0792.1.*
- *Rodgers, K.B. et al. (2021) "Ubiquity of human-induced changes in climate variability," Earth System Dynamics, 12(4), pp. 1393–1411. Available at: https://doi.org/10.5194/esd-12-1393-2021.*

2. It would be best to explain the differences between the NAO-like pattern obtained by the SVD analysis and the NAO obtained by the EOF analysis.

→ We thank the reviewer for this comment. In our study, the SVD analysis is applied to the streamfunction at 500 hPa and surface temperature fields to extract the coupled modes of variability that describe the evolution of atmospheric circulation patterns to rising surface temperature. We use the SVD analysis to study the evolution of atmospheric regime states under continued warming. The leading SVD for the IV exhibits a pattern that resembles the NAO-positive-like pattern, but it is not intended to represent the NAO itself. Instead, this mode highlights the dominant coupled atmospheric-surface temperature variability that characterises the large-scale circulation regimes.

Conversely, the EOF analysis is applied to the sea-level Pressure (SLP) field to objectively extract the NAO index and its associated pattern for both the FF and IV. The EOF-derived NAO is used to quantify changes in NAO strength and persistence within different atmospheric regime states, as identified through SVD analysis and clustering. Therefore, our focus is not on comparing the NAO-like pattern obtained from SVD with that from the EOF, as their purposes differ conceptually and methodologically.

3. Some conclusions in the main text are obtained based on the figures in the supplementary materials (e.g., Fig. S2, Fig. S5, Fig. S6, Fig. S8, Fig. S9). It's best to place them in the main text.

→ We appreciate the reviewer's suggestion to include additional figures from the Supplementary Information in the main text. Owing to space and layout constraints, it is not feasible to move a lot of the supplementary figures into the main manuscript. However, we have incorporated the most critical material where possible. Specifically, the previous **Fig. S8** has now been merged into **Fig. 7** in the main text. Furthermore, following Reviewer 2's recommendation, we have added a new figure to strengthen the presentation of inter-regime transitions and persistence (**Fig. 4**). The revised manuscript now contains eight main figures, which we believe sufficiently and clearly support all major conclusions of the study while maintaining a balanced and readable structure.

Minor points:

L13-14: "These changes in regimes have important implications for extreme weather, energy, water, and food security". The important implications of the authors' findings have not been explained in the discussion.

→ We thank the reviewer for this comment. Under the " **Summary and Discussions**" section in the revised manuscript (see lines **541-547**), we add a few points on the role of mid-tropospheric circulation patterns manifested as regimes, along with the increased frequency of NAO-positive events in the future that could affect the temperature, precipitation, and wind, i.e., the variables that are directly related to extreme weather, energy source, agricultural productivity and water availability. For example, stronger NAO-positive occurrences are associated with warmer, wetter winters over northern Europe, but drier and colder conditions over southern Europe.

Lines 541-547: *"The changes in the frequency of occurrence and structure of atmospheric regimes alter large-scale temperature, precipitation, and wind patterns that enhance seasonal climate variability. These alterations can affect the frequency and severity of extreme weather events, modify regional energy demand and wind power capacity, and influence water resources through shifts in precipitation and runoff patterns. As a result, the evolving regime dynamics may have cascading impacts on agriculture, water management, and food security across the North Atlantic region."*

L255: Here, the authors seem to describe Fig. 3g not Fig.3 (e and h).

→ Thank you for this careful observation. We have now replaced the appropriate figure panel, i.e., **Fig. 3g** instead of **Fig. 3(e and h),** in the revised manuscript.

L259: "However, the change in the spatial structure of the SVD patterns indicates that external forcing reorganises the centres of circulation over the North Atlantic." Not understand.

→ We thank the reviewer for pointing this out. We agree that the original phrasing lacks clarity. Specifically, under the FF, the cyclonic centre over the North Atlantic shifts northward and eastward in the Post-1995 period compared to the Pre-1995 period (**Fig. 3(a and c)**). This displacement indicates that external forcing reorganises the dominant centres of circulation. The revised sentence in the manuscript (see lines **239-242**) now reads as follows:

Lines 239-242: *"However, with the inclusion of the external forcing, i.e., the FF, we find a spatial reorganisation of the dominant atmospheric circulation patterns, as evidenced by the northward displacement of the cyclonic and anticyclonic centres, indicating that anthropogenic forcing modulates the position of circulation centres over the North Atlantic."*

L331: "This comparison demonstrates that CESM2-LE effectively captures the observed NAO phase distributions." What are the correlation coefficients between the NAO indices in the CESM2-LE and the observed NAO index?

→ We thank the reviewer for this comment. As our analysis is based on the large-ensemble framework of CESM2-LE, we focus on assessing whether the observed NAO index lies within the ensemble spread rather than computing correlation coefficients for each ensemble member. Since each ensemble member is designed to represent a different realisation of internal climate variability under identical external forcing, computing individual correlations with the observed record is not meaningful and does not reflect the purpose of a large-ensemble experiment.

Instead, our comparison evaluates whether the observed Hurrell PC-based NAO index (https://climatedataguide.ucar.edu/climate-data/hurrell-north-atlantic-oscillation-nao-index-pc-based) falls within the spread of the 100-member CESM2-LE simulations, which would indicate that the model adequately samples the internal variability of the NAO and captures its observed phase distribution. This approach is consistent with previous large-ensemble studies (Kirchmeier-Young, Zwiers and Gillett, 2017; Maher *et al.*, 2019; Delworth *et al.*, 2020; Deser *et al.*, 2020; Rodgers *et al.*, 2021) where model fidelity is assessed through the ensemble spread encompassing the observed variability rather than through direct correlation with individual ensemble members.

- *Delworth, T.L. et al. (2020) "SPEAR: The Next Generation GFDL Modeling System for Seasonal to Multidecadal Prediction and Projection," Journal of Advances in Modeling Earth Systems, 12(3), p. e2019MS001895. Available at: https://doi.org/10.1029/2019MS001895.*
- *Deser, C. et al. (2020) "Insights from Earth system model initial-condition large ensembles and future prospects," Nature Climate Change, 10(4), pp. 277–286. Available at: https://doi.org/10.1038/s41558-020-0731-2.*

- *Kirchmeier-Young, M.C., Zwiers, F.W. and Gillett, N.P. (2017) "Attribution of Extreme Events in Arctic Sea Ice Extent," Journal of Climate, 30(2), pp. 553–571. Available at: https://doi.org/10.1175/JCLI-D-16-0412.1.*
- *Maher, N. et al. (2019) "The Max Planck Institute Grand Ensemble: Enabling the Exploration of Climate System Variability," Journal of Advances in Modeling Earth Systems, 11(7), pp. 2050–2069. Available at: https://doi.org/10.1029/2019MS001639.*

L411-423: Why does Fig. S9 mean "This highlights the imprint of the global warming signal in modulating the variability of the NAO index in the future"? Not understand. The decline in the variance of the NAO index in Fig. S9b is the "internal variability", which is not related to the global warming.

→ We thank the reviewer for this comment. We acknowledge that the IV field is defined after removing the ensemble mean, i.e., with no forced response. From a mathematical perspective, the ensemble mean is the first-order statistic (or mean state) of the ensemble. However, the variance is a second-order statistic that quantifies the amplitude of deviations from the mean. Thus, even after subtracting the ensemble mean, we observe changes in the variance of the NAO index, indicating that the forced response plays a role in modulating the NAO index in the future. However, external forcing can still indirectly influence the characteristics of internal variability by modifying the background mean state of the climate system (Liguori *et al.*, 2020; Chen and Dai, 2024; Jeong, Yoo and Yeh, 2024). This modulation does not alter the phase of internal variability but can affect its amplitude, persistence, and spatial expression. Hence, the decline in the NAO index variance in the IV field suggests that the anthropogenic warming dampens the spread or the amplitude of the variability, even when the ensemble mean is removed.

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