National Aeronautics and
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March 16, 2023

Dr. Stephen Yeager
Editor
Journal of Climate

Dear Dr. Yeager:

Thank you very much for your evaluation of the reviewers' assessments. We have modified the manuscript significantly in light of this feedback.

First, per your recommendation, along with those from Reviewer 3, we have recast all figures in terms of differences between the collapsed versus recovered SSP 2-4.5 simulations, rather than in terms of anomalies relative to the preindustrial control simulation. Second, we also followed the recommendation of Reviewer 3 to first present the SSP 2-4.5 results, followed by the comparison with the 2xCO_2 and 3xCO_2 integrations, as reflected in the reorganization of Sections 3a1-3. Third, both Reviewers 1 and 3 requested that we incorporate results from the full ten-member SSP 2-4.5 ensemble, which we have done, as reflected in all figures. The SSP 2-4.5 differences are therefore cast in terms of differences between the 8 "recovered" simulations and the 2 "collapsed" simulations, after year 2400. The incorporation of the full ensemble strongly supports the conclusions that were drawn in the previous version of the manuscript (which only showed two ensemble members) but now captures the statistical significance of these changes, supporting the robustness of our main findings. Fourth, we have agreed with all reviewers that the manuscript is simply too long. To this end, as we also agree with you and Reviewer 3 that the section describing the zonally varying response was the weakest part of the original manuscript and we have removed that entire section, only retaining a brief reference to the zonally varying eddy kinetic energy responses, now shown in (new) Figure 5.

Overall, we feel that we have addressed all of the reviewers concerns through a major reworking of the manuscript and figures which has encouraged us to focus on the robustness and implications of our key results. We hope that the reviewers feel the same and we look forward to receiving their reviews on the revised manuscript.

Finally, we agree with you that is important to provide the reviewers with a copy of the heavily referenced companion study entitled "Stochastic Bifurcation of the North Atlantic Circulation under a mid-range future climate scenario with the NASA-GISS ModelE," led by Dr. Anastasia Romanou (NASA GISS). We have now provided that as reference material for you and the reviewers.

Kind regards,

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Response to Reviewer 1

This paper quantifies the impact of internal variability in the AMOC response in the 21st century on the Northern Hemisphere (NH) large-scale atmospheric circulation using an ensemble of runs conducted with the same forcing in the same climate model (GISS E2.1). In two out of ten ensemble members, the AMOC collapses, which results in a northward shift and strengthening of the NH Hadley cell and a poleward shift of the NH midlatitude jet. The results are compared to runs with an abrupt quadrupling of CO2 and with slab ocean runs (which, by definition, do not capture this behavior).

Understanding the impact of AMOC collapse on the global atmospheric circulation is very important, and it's great that the authors have been able to isolate this in runs with identical forcing in the same model (which avoids complications of alternative methodologies used in past studies). The caveat is, of course, this is just from one model, but the authors have comprehensively compared their results with those from other studies using different models and methodologies.

I recommend publication after minor revisions. My main complaint about the paper in its present form is that it's too long, probably to its own detriment (as there are many internal inconsistencies within the text, as described below). The paper is comprehensive and well-written, but at times explores idiosyncrasies of the GISS model and the past literature that most readers would not care about and that get in the way of the central message. I would encourage the authors to be more concise and to eliminate text that is not centrally related to the key focus of the paper. Additionally, the analysis Section 3d (point #4 in conclusions) is not as carefully done as in the rest of the paper (see comments below) and could easily be removed or shortened (as many of the zonally asymmetries noted in Section 3d can easily be inferred from Figure 4).

We thank the reviewer for providing this very constructive feedback. We agree with her/him that the manuscript is too long and we have followed her/his recommendations to remove the more speculative material, most notably the entire section discussing the zonally varying characteristics of the responses. We agree that this section was handled with less care and we reserve examination of those results for future work. These changes have reduced the manuscript length by several pages and have contributed to an overall more concise and targeted story. We hope that the reviewer agrees.

Minor Revisions

Lines 21, 176-180, 688, 997-998: The midlatitude jet shifts, not strengthens. Please correct this to be consistent with the text on Lines 521-528.

As highlighted now in the new version of the manuscript, wherein we now present anomalies as "Collapsed-Recovered," the midlatitude jet does strengthen over the Atlantic (as it also extends eastward over Europe) while it mainly shifts poleward over the Pacific (new Fig. 3e). Therefore, while one could describe the zonal mean wind response (new Fig. 4b) as a poleward shift, the basin-specific response is more nuanced. We have substantially revised this section in order to explain this more carefully.

Lines 29-31: The direct radiative effects of increasing greenhouse gases can also change the circulation. It's not just modulated through the SSTs, although that is the biggest component. See Deser and Phillips (2009) and Grise and Polvani (2014).

Excellent point — we realize now that this sentence overlooks the direct radiative response of the circulation (as would be inferred from fixed SST experiments) which we agree with the reviewer can contribute significantly to the total circulation response. We thank the reviewer for these references, which we now cite.

Lines 115-117: Why examine only one ensemble member for each? If you have multiple ensemble members, averaging over the two (for AMOC collapse) and eight (for non-AMOC collapse) would help to eliminate some of the noise due to internal variability among members. The similarity of ensemble member I to ensemble A for the AMOC collapse is addressed on lines 199-201/442-444, but how representative is ensemble member C of the other seven ensemble members without AMOC collapse? Are the eight ensemble members without AMOC collapse statistically distinguishable from the two with the AMOC collapse?

Another excellent point. We have now redone all analysis and figures contrasting the response among the 8 recovered ensemble members (now referred to more intuitively as SSP 2-4.5 R) and the 2 collapsed ensemble members (SSP 2-4.5 C). As the reviewer will now see, the equilibrated (i.e., year 2400-2500) responses among the recovered versus collapsed simulations are remarkably consistent, reinforcing the robustness of our conclusions. Note that statistical significance is also now denoted in all plots, where we have accounted for the different ensemble sizes of the recovered versus collapsed ensembles. Thank you for encouraging us to pursue this analysis.

Lines 157-162, 358-364: These outlines of the paper do not actually correspond with what is shown in the results section in section 3. Please correct.

We apologize for this oversight and have fixed this is the manuscript.

Line 257: Correct citation here is Grise and Polvani (2017).

Thank you. This reference has been changed.

Line 323: I think you mean the largest eddy momentum flux convergence is co-located with the storm track. The largest eddy momentum fluxes are near the Hadley cell edge, as stated above.

Correct — we miswrote. This has been fixed.

Lines 427-430, 512-515, 529-531, 552-555: The sentences on these lines pose conflicting statements in my view. Some argue for a change in SH meridional temperature gradient due to AMOC collapse (i.e., that is more warming at SH high latitudes), and some argue for no change in SH meridional temperature gradient due to AMOC collapse. Please clarify this issue, and make the statements consistent throughout the paper.

We thank the reviewer for pointing out these conflicting statements and we have now taken care to more accurately describe these temperature gradient responses in the revised manuscript. Please note that these earlier sections have been significantly revised.

Lines 465-467: Here is a good example of extraneous information that is included that is tangential to the focus of this study on the atmospheric circulation.

Agreed. This material has been removed.

Line 479: I wouldn't describe this as a poleward shift. It's more of a strengthening of the eastern extension of the jet stream over Europe.

Agreed — we have revised the text to clarify that this is a strengthening and eastward extension of the jet over Europe. Please see the revised text.

Lines 488-494, 814-817: This text can be cut in my opinion. These details are parenthetical in nature and can be examined by the reader in Appendix figures if they so choose. The latter (lines 814-817) is also discussed later in more detail on lines 965-967, so there is no need to mention this twice.

Agreed — we have decided to cut all references to normalization by GMST as this is unnecessarily complicated and distracts from the key messages of the manuscript.

Line 501 (Figure 5): It would be helpful to have a difference figure in the appendix for this figure, as was done for the two previous figures (as in Fig. A2 and A3).

At the recommendation of another reviewer, we have now cast all responses in terms of "Collapsed-Recovered" SSP 2-4.5 and "3xCO_2-2xCO_2" differences. Please see new Figures 3-7. We hope that this makes the AMOC "signal" more transparent.

Lines 529-531, 552-555: If anything, you would expect an equatorward jet shift due to a collapsed AMOC, due to the enhanced warming at SH high latitudes compared to the tropics. You can see this to some extent in the right column of Fig. 5b, as the poleward jet shift in the bottom panel is weaker than in the top panel.

Yes — we agree with the reviewer. We have added a brief clause to one of the sentences in this paragraph mentioning this possibility.

Lines 650-653: I question the close relationship between the latitude of max eddy momentum flux convergence (Fig. 7d) and the NH midlatitude dry static stability (Fig. 7e). Note the very different behaviors in the cyan lines in these two panels. The behavior of the static stability (Fig. 7e) much more closely matches that of the Hadley cell edge (Fig. 7a). Also, the latitude of max eddy momentum flux convergence resides in the midlatitudes and co-locates with the midlatitude jet. So, if the authors are interested in the Hadley cell edge, they should examine the latitude of maximum eddy momentum fluxes (not their convergence) as in Chemke and Polvani (2019) and Menzel et al. (2019).

Our apologies — we were completely inaccurate in our description of what we were plotting. Indeed, we *are* examining the latitude of maximum eddy momentum fluxes, not their convergences. We have removed all erroneous references to "momentum flux convergences" in the text.

Line 674, Figure 8c: How is baroclinic eddy kinetic energy generation defined? And, how is it calculated? Please include this methodology in the paper.

Apologies for the oversight — the baroclinic EKE generation term in the model dynamics, which refers to the lifting of heavy air, is ~ $-\alpha'\omega'$, where α is one over the density, and ω is the vertical velocity in pressure coordinates. We have clarified this in the text.

Lines 677-678: What is the proposed causal mechanism underlying Fig. 8? Changes in static stability will impact baroclinic eddies, which are closely related to the location of the Hadley cell edge. But, the Hadley cell edge and Hadley cell strength are not necessarily closely linked to one another (e.g., Menzel et al. 2019).

First, Menzel et al. (2019) did not make a direct statement about the coupling between HC edge and HC strength, but, rather, noted a strong disconnect between subtropical jet (STJ) strength and HC edge. It seems, therefore, that the reviewer is assuming that HC strength and STJ strength are nearly interchangeable, but Figure 3 from Menzel et al. (2019) highlights that the two quantities are actually not that well correlated, especially during DJF in the NH (correlation 0.21). So, we question the premise that the HC strength need necessarily be equated with the STJ strength. Second, we note that conclusions about HC strength and width made in that study were based on interannual variability and the response to 4xCO_2 forcing. For both cases a HC expansion is always associated with a weakening of the HC strength. In response to a weakening of the AMOC, however, HC strength increases (a widely reported result). This strengthening of the Hadley Cell in the NH has often been linked in previous studies to a southward displacement of the ITCZ, which is evident in our figures in the precipitation changes (Fig. 3, Fig. 6) and is also a canonical response to a collapsed AMOC. As we do not wish to reinvent the wheel, we lean on previous studies have shown that this displacement in the ITCZ and strengthening of the HC is reflective of ocean and atmosphere energy fluxes as was done in Orihuela-Pinto et al. (2022), noting that this compensation is also operative in our model (see tropics in (new) Figure 11). Therefore, in the analysis by Menzel et al. (2019) in which the HC overwhelmingly weakened we suspect that models were used in which the AMOC-induced ITCZ changes were not present or very weak. Therefore, we suspect that different conclusions about HC strength and HC edge changes may be drawn depending on which forcing scenario is considered.

All of that said, we want to be clear that we are not attributing the Hadley Cell strength increases to the same northern midlatitude eddy changes that are linked to changes in static stability and midlatitude EKE. In this respect we agree with the reviews that our introduction of (new) Figure 10 is misleading and care must be taken to suggest that the HC strength changes are not necessarily coupled to the changes in the fields shown in the other panels. It is certainly possible that midlatitude baroclinity changes over the Atlantic could drive a local intensification of the regional Hadley Cell in that region. This idea was proposed in Bradshaw et al. (2019) who used hosing experiments to show that in response to an AMOC collapse there is an enhancement of the NH subtropical jet in the region around 20ºN-30ºN and between 30ºW-10ºW (see their Figure 5). However, as we have removed much of the zonally varying HC analysis from the text we reserve further discussion to future work.

To summarize: we suspect that the apparent discrepancy with Menzel et al. (2019) relates to the different SST responses present in the different simulations, specifically as relates to the southward ITCZ shift that occurs in response to an AMOC collapse. By now acknowledging that this HC strengthening is more directly related to region SST anomalies associated with the ITCZ as has been reported in previous studies, we hope that this removes any apparent contradictions with the previous literature.

Line 706: Approximately 1 PW, not exactly 1 PW

Noted. This has been changed.

Lines 756-758: This argument doesn't make sense to me. The top row shows stronger northward (not poleward) latent heat transport in the SH subtropics, but there is also stronger southward dry static energy transport in the same region compared to the bottom row.

We are talking about the fact that there is stronger southward oceanic transport in the SSP 2-4.5 collapsed runs, compared to the 3xCO_2 run. This reflects differences in the degree of compensating northward latent heat and dry static energy transports, resulting in increased northward MSE in the SSP 2-4.5 collapsed runs, but decreased northward MSE in the 3xCO_2 run. We hope this clarifies any confusion.

Line 768: What is the expectation from Held and Soden (2006)? This is not discussed elsewhere in the paper that I could find, so please be more specific here.

Yes, it is — see their section 6 and Figure 10 lines discussing the expectation that latent and sensible transports compensate.

Line 772, and hereafter (especially when describing Fig. 11): The manuscript uses the acronym DSE to refer to both dry static energy and dry static energy transport. When referring to transport, please say DSE transport to distinguish.

Thanks for the comment — we have fixed throughout. Note also that we have now removed (old) Figure 11 as part of our goal to trim down the manuscript.

Line 778: Do you mean lower latitudes here? The poleward extent of the dry static energy changes in Fig. 10 is 40° latitude.

We have removed this section.

Line 820: How are the local Hadley circulations calculated? I didn't see this described anywhere. To do this correctly, you need to use the method of Schwendike et al. (2014), which uses the meridional component of the divergent wind (see also Staten et al. 2019). Otherwise, if the total meridional wind is used, rotational wind features can complicate the interpretation of the results (see Karnauskas and Ummenhofer 2014).

We agree. Note that we have now removed this section.

Line 824: Figure 12a is presumably reproduced from Fig. 5d, but the color scale must be changed. Yet, the color bar is identical in the two figures. Please reconcile.

Note that we have now removed this section.

Lines 824-827: The strengthening of the streamfunction above 200 hPa is the signature of the tropopause rising and the Hadley cell expanding upward in a warming climate.

We agree. Note that we have now removed this section.

Lines 830-832: The west Pacific (Fig. 12d) looks more like the zonal mean (Fig. 12a) than the east Pacific (Fig. 12c).

Note that we have now removed this section.

Lines 832-833: I don't follow the logic here. There are a number of theories for what governs Hadley cell strength (not just meridional SST gradients) (see Chemke and Polvani 2021), and it's unclear whether these same arguments would apply to the local Hadley circulation defined over a narrow longitude band. Furthermore, the meridional SST gradient also increases in the Atlantic, and the local Hadley cell does not strengthen there.

Note that we have now removed this section.

Lines 852-854: I don't follow the logic here, either. The Walker circulation should primarily affect vertical motion on the Equator, not at 20°-30° latitude. Yet, the dominant differences seen in Figs. 12c-d are not on the Equator.

Note that we have now removed this section.

Lines 856-858: This might be a situation where the answer may depend on whether or not you normalize by the global-mean surface temperature change. This was already discussed above for the precipitation response in this region (lines 489-491).

We agree. Note that we have now removed this section.

Lines 867-869: What are the KE and P-K terms? These are not defined in this paper. This is an example of a detail that seems unnecessary to include unless you explain it fully.

Our apologies. P and K refer to the zonal mean available potential and kinetic energies, respectively. This is now clarified in the text.

Lines 951-957: Here is another example of speculative text that could be deleted. These details are not understandable to the reader without being more fully explained.

Having trimmed down the text substantially we reserve the right to retain this text as we do think it is important to make the connection back to the results from Mitevski et al. (2021).

Line 962: Also over the west Pacific. See Figure 11.

Note that we have now removed this section.

Lines 1026-1029: I don't see how the Zurita-Gotor and Alvarez-Zapatero (2018) study is relevant here. Zonal mean dynamics cannot simply be applied to explain overturning cells confined to specific longitudes.

Note that we have now removed this bullet as we have removed the section describing the local circulation responses.

Typos

Lines 207, 252, 274, 537, 863: Section 3b

Thanks for noting. This has been fixed.

Line 253, 356, 959: Section 3c

Noted.

Line 364: Delete extra closed parenthesis.

Noted.

Line 404: El Niño

Noted

Line 639: Figure 4b

Noted.

Line 791: Section 3a

Noted.

Line 824: Figure 5d

Noted.

Line 947: Figure 5

Noted.

Line 976: a decrease

This section has been removed.

References

Thanks for these references. They have now been included (the ones that are relevant to the retained text).

Chemke, R., & Polvani, L. M. (2021). Elucidating the mechanisms responsible for Hadley cell weakening under 4 × CO2 forcing. Geophysical Research Letters, 48, e2020GL090348.

Deser, C., and A. S. Phillips (2009), Atmospheric circulation trends, 1950-200: The relative roles of sea surface temperature forcing and direct atmospheric radiative forcing, J. Clim., 22, 396-413.

Grise, K. M., and Polvani, L. M. (2014), The response of midlatitude jets to increased CO2: Distinguishing the roles of sea surface temperature and direct radiative forcing, Geophys. Res. Lett., 41, 6863- 6871, doi:10.1002/2014GL061638.

Grise, K. M., & Polvani, L. M. (2017). Understanding the Time Scales of the Tropospheric Circulation Response to Abrupt CO2 Forcing in the Southern Hemisphere: Seasonality and the Role of the Stratosphere, Journal of Climate, 30(21), 8497-8515.

Karnauskas, K. B., & Ummenhofer, C. C. (2014). On the dynamics of the Hadley circulation and subtropical drying. Climate Dynamics, 42(9-10), 2259-2269.

Schwendike, J., Govekar, P., Reeder, M. J., Wardle, R., Berry, G. J., & Jakob, C. (2014). Local partitioning of the overturning circulation in the tropics and the connection to the Hadley and Walker circulations. Journal of Geophysical Research: Atmospheres, 119, 1322-1339.

Staten, P. W., Grise, K. M., Davis, S. M., Karnauskas, K., & Davis, N. (2019). Regional widening of tropical overturning: Forced change, natural variability, and recent trends. Journal of Geophysical Research: Atmospheres, 124.

Response to Reviewer 2

This manuscript examines the differing impacts of collapsed vs. recovered AMOC on atmospheric circulation. The authors make use of two fully coupled simulations during the "extension" portion (beyond 2090) and four XxCO2 experiments ranging from 2-5xCO2 in both fully coupled and slab ocean model configurations. They address two central questions:

1) ***How does collapsed AMOC impact atmospheric circulation relative to recovered AMOC at the same forcing level and how does this compare with different abrupt CO2 forcing scenarios?***
2) ***How important are AMOC related impact and how do they scale with global mean surface temperature?***

To address these questions, the authors conduct a wide range of diagnostics looking at differences in simulations during time periods with either a collapsed or recovered AMOC state. The authors find a robust impact on the Hadley cell with a strengthening and Northward shift that is associated with a Bjerknes compensation, around 40N associated with impacts of AMOC on the storm track. They also show that the circulation response does not scale with equilibrium climate sensitivity.

Overall, the paper uses appropriate methodologies, logical arguments, and comes to impactful conclusions. The paper is well written, but I suspect is well beyond the word limit for the journal. My comments are all relatively minor and if addressed, I believe this manuscript will be suitable for publication.

We thank the reviewer for her/his constructive feedback and overall positive tone.

Comments

1- ***This manuscript is very well written and easy to follow, however in my estimation it might be over 1.5 x the maximum length. There are many instances where the authors refer to different subsections and summarize the results in subsections, both of which are symptoms of and contribute to the manuscript length. I would recommend working to trim down the manuscript for publication.***

We completely agree with the reviewer and have substantially trimmed down the manuscript by several pages, taking care to remove redundant passages, unnecessary appendix figures, etc. We hope that the reviewer thinks that this has contributed to a more readable study.

2- ***I think there could be some subtle but important differences in how the two SSP experiments are described.***

   Although in this paper we only see two of the simulations, on line 104-107 the authors state "During this time period the authors show that internal variability alone results in a spontaneous bifurcation of the ocean flow, wherein two out of ten ensemble members exhibit an entire AMOC collapse, while the other eight recover at various stages". I believe the definition of collapse in the paper is ~5Sv (lines 234-235), but this is mentioned in the context of the time averaged and not in the first discussion of these two simulations, I would suggest including this earlier in the discussion.

At the request of Reviewer 2 as well another reviewer we now have included the results from the entire 10-member SSP 2-4.5 member, now contrasting the responses in the 2 "collapsed" simulations (more intuitively referred to as SSP 2-4.5 C) with the 8 "recovered" simulations (SSP 2-4.5), where "collapse" and "recovery" refer to the AMOC behavior beyond year 2400. To the point that the reviewer is making here, note that we have slightly altered our definition of the AMOC to reflect the maximum stream function value at 48°N (earlier we inadvertently were sampling at 900 m as well). The collapse (to 0 SV) is now more apparent and therefore does not need much explaining.

> ***On line 19-20 the authors state "We show that an AMOC collapse results in an abrupt northward shift and strengthening of the Northern Hemisphere Hadely Cell and intensification of the northern midlatitude jet". Another example is on line 987-988 "we have isolated the atmospheric response to a spontaneous collapse of the AMOC in the context of a warming climate". Focusing on the "collapse" gives the impression that there was an abrupt AMOC reduction in some of these simulations and this is what is being studied. When I look at the two simulations that are provided, I don't see a rapid AMOC collapse, I see a slow and steady continued decline in AMOC in SSP2-4.5 I and a recovery in SSP3-4.5C.***

Fair point. We have taken care to change references in the manuscript to "collapse" to "strong decline and eventual collapse" (and variations on this phrasing).

> ***I think small language changes could make it clearer that you are looking at the recovery period when GHG emissions are reduced. Given the time period of study and the forcing impacts, I think a better characterization of these simulations is the those with and without an AMOC recovery with reduced GHG emissions.***
>
> ***It is possible that this view point of focusing on the collapse in two members makes more sense given the mechanisms responsible that are outlined in AR2022, the manuscript under review, but without access to this manuscript or explanation in the current manuscript I can't be sure if this is true.***

We agree the reviewer's concern that AR2022 was not made available for the initial submission. We have now supplied this material and hope that this adds the context that the reviewer was seeking. We thank her/him for being patient.

3- ***Measures of statistical significance are missing from all the difference plots. These should be added.***

Excellent point. We have now added measures of statistical significance and a description of our methodology in Section 2.

4- ***When it comes to comparing the SOM to the FOM simulations, are differences in surface temperatures found only in the North Atlantic or are there generally higher SSTs in the SOM simulations. If so, how does that impact the interpretation of the results?***

Thank you very much for this comment as it has highlighted the fact that we did not mention an important detail when explaining the FOM and SOM comparisons. Indeed, another reviewer raised the same point! The reviewer here is correct in pointing out that the FOM and SOM setups produce the same warming at 2xCO_2. This is *not correct*. In plotting these results we adjusted the SOM 2xCO_2 simulation to match that of the FOM 2xCO_2 results as we wanted to focus on the difference between their scalings, relative to CO_2 (and GMST). Again, thank you very much for highlighting this oversight, which we now clarify in the figure captions.

5- ***In general, I find it strange to conduct the analysis choosing only two ensemble members. However, I can appreciate that with the large amount of analysis that this choice might have been necessary. Why are the two ensemble members SSP 2-4.5 I and SSP 2-4.5 C given the***

> ***letters "I" and "C"? Does this have any significance? If so, it would be good to mention it and if not, perhaps a nomenclature that allows a reader to recall which simulation exhibits which behavior might be better (e.g. "r" for recovery and "c" for collapse).***

This is an excellent comment. As explained in our previous response, we now incorporate results from the entire 10-member SSP 2-4.5 ensemble and use the reviewer's suggested "R" and "C" to denote recovery and collapse, respectively.

Minor Comments:

Line 678-683: I don't understand this argument. Do you mean that other have argued that changes in the jet are simply an artifact of changes in the tropopause height but that here you do not see a change in the tropopause height? When you speak of the tropopause height how are you defining it, for example did you look at the dynamic tropopause on the 2PVU surface or the thermodynamic tropopause output from the model.

We mention this as previous studies have suggested a strong link between tropopause height changes (defined using the WMO thermal-based definition) and the response of the Hadley Cell. For example, Held (2000) claim that an increase in tropopause height should increase the critical shear necessary for baroclinity instability, thus pushing poleward the latitude where it is equal to the angular momentum conserving shear (Hadley Cell edge) (Lu et al. (2007)). However, this appears not to play out as predicted, as shown, for example, in the analysis presented in Section 4b in Chemke and Polvani (2019). Our results also confirm a weak relationship. We now reference these studies when motivating this brief discussion of the tropopause height changes.

Caption 10:
I believe "are shown in the left and right panels" should be "are shown in the upper and lower panels"

Thanks for catching this mistake! We have fixed this is in the manuscript.

Caption 12: For climatological values I'm assuming you have positive solid and negative dashed. This should be included in the caption.

This figure has now been removed.

Caption 13: A statement of the contour interval would be helpful.

This figure has now been modified and appears as Figure 5. We have now included a reference to the contour interval that is used.

Line 1022-1023: Watch the consistency in using the abbreviation HC for Hadley Cell.

Thanks. Noted.

Finally, this is more of a comment and not an item to address. The authors refer to a paper under review by Romanou et al. very frequently. Though it is not necessarily a problem to refer to a manuscript under review, it should be noted that it is challenging as a reviewer as we do not have access to this paper.

We are in complete agreement with the reviewer and have now included this as reference material for the reviewer.

Response to Reviewers 3 and 4

This manuscript discusses a series of experiments performed with the NASA GISS E2.1 climate model to characterise and understand the NH atmospheric response to an AMOC collapse. In this model, the AMOC collapses for a forcing equal to or exceeding 3xCO2, as well as in 2 out of 10 members forced by the extended SSP 2-4.5 scenario (corresponding to radiative forcing of about 2.5XCO2). By analysing one AMOC-collapsed (SSP 2-4.5 I) and one AMOC-recovered (SSP 2-4.5 C) member, the authors show that the AMOC collapse results in a shift in atmospheric circulation which, for a number of dynamical aspects, substantially exceeds the differences in circulation between 2xCO2 (AMOC recovers) and 3xCO2 (AMOC collapses). They show that an AMOC collapse completely disrupts the relationship between atmospheric circulation and global mean temperature change compared to simulations with a thermodynamic slab ocean version of the model. Specific examples include a wintertime strengthening and poleward shift of the NH jet stream, a strengthening of the Hadley cell, and a poleward shift of the NH Hadley cell edge. Such circulation changes are further placed in the context of changes in the meridional atmospheric and oceanic energy transports. The authors note that this is the first time the impact of an AMOC collapse under climate change is isolated within a single climate model and without adding an external freshwater forcing.

I found this study very interesting. This model shows a noise-induced bifurcation of the AMOC for the SSP 2-4.5 scenario which is fascinating and, regardless of the realism of the model, I fully agree with the authors that these experiments are extremely useful the examine the global climate and atmospheric circulation responses to an AMOC collapse. The proposed analyses are mostly sound, and this manuscript will be a very useful reference for any future study on the atmospheric response to an AMOC collapse. However, what this manuscript lacks is sufficient clarify in the discussion of some of the results. In particular, I have concerns regarding the choice of some of the displayed figures, which make really hard to follow the discussion in a few parts. Moreover, the manuscript is really long (about twice a standard J Clim paper). While I was initially skeptic about the need of such a long paper, I now overall feel that a longer than standard manuscript is justified to keep all the information in the same paper. Yet, I think some discussions and analysis are a bit redundant and I would suggest dropping them for the sake of conciseness. Finally, the authors mention a companion paper which is under review with supposedly a stronger oceanic flavour. I would appreciate receiving a copy of the manuscript to evaluate the presence of any major overlap. Overall, I have do doubt that this manuscript is suitable for publication in J. Climate with some revision.

We thank the reviewer for her/his very constructive feedback. While we appreciate that she/he has been open to the possibility of retaining the longer format, we have decided to shorten the manuscript substantially at the request of the editor and other reviewers. This has involved removing much of the discussion of the zonally varying circulation response and several figures in the appendix. In the process of doing this we have also trimmed down several sections that were redundant and tried to be clearer in distilling the key messages. We hope that the reviewer agrees that the revised version is an improvement.

Main comments

1) The manuscript hardly ever shows spatial maps of the differences between the responses to the collapse and recovered AMOC experiments (either abrupt or SSP). The only cases where this is shown are supplementary Figure A1 and A3. This is probably motivated by the goal of comparing the full CO2 and SSP responses. However, I believe that communicating what the AMOC response is in the first place is a higher priority, and a number of discussions in the paper are really hard to follow without looking at the difference plot. Without the difference plot, the reader can only follow the

discussion by mentally inferring the differences. This is quite tiring in the long term, especially for such a long manuscript. For example, while reading the discussion of Fig 4, I had to look at Fig A3 all the time, The text was then clear to follow, but surely that shouldn't be the purpose of a figure in the Supplementary Material! Other figures (particularly, Fig 5, Fig 9, Fig 11 and Fig 12) didn't come with any difference plot and while I could follow most of the arguments, I cannot say it was straightforward. A related caveat of the current presentation, is that some figures consist in a very large number of small sub-sub-panels. Some of them, particularly Fig 4 and Fig 14, are really too small to be read.

We completely agree. We have now recast all anomalies in terms of "Collapsed-Recovered" SSP 2-4.5 and "$3xCO_2$-$2xCO_2$" differences, putting the former first (as requested by the reviewer) followed by comparisons with the latter. This has reduced the number of panels in each figure. We hope these changes have made the manuscript easier to follow.

I am not sure how the authors would like to address this, but it will probably require a moderate reshape of the presentation of the material, at least for the first part of the paper (Fig 4 and Fig 5). One option, certainly not the only one, would be to first answer what the response to the AMOC collapse is in the SSP runs (including the relevant difference plots in the main paper), and then ask how does this compare to the response to the AMOC collapse under 2xCO2 vs 3xCO2 (perhaps just for some variables).

This is a great recommendation – please see the revised Section 3a. As suggested, we now discuss the SSP 2-4.5 results and follow with a comparison with the $XxCO_2$ analysis. Please see the revised manuscript.

2) I think the authors could stress a bit more in the conclusions the important result that - for a number of variables - the impact of the AMOC collapse way exceeds that from having a 2x or 3xCO2 increase. I found this result remarkable, and I would suggest stressing it as much as possible.

We agree – indeed, this is a remarkable result. We have now tried to make this point clearer in the conclusions. For example, please see the revised first bullet in the conclusions. The other points have been modified as well, in order to better distill the key messages.

3) The manuscript is very long and very detailed. I liked it, but some parts felt redundant to the key messages conveyed. I have suggested specific parts that could be cut or removed in the specific comments below.

Agreed and thanks for these recommendations, which we have used when determining how to reduce the text.

4) The introduction does a very good job at convincing the reader that this is the first study in which the impact of an AMOC collapse is studied in a single model and without freshwater hosing. But then it would be nice if, in the conclusions, the authors could elaborate on this and explain whether their cleaner approach has revealed any potential caveat arising from studies that employ fresh water perturbations or analyses of the inter-model spread, or whether it mainly confirms previous studies.

The point that is being communicated is that this is truly a unique ensemble in which the disparate AMOC behavior arises entirely spontaneously. Furthermore, compared to previous studies we have identified new results (for example, the HC edge shift). We have tried to make this clearer throughout.

Specific comments:

L29-31: I had never heard the use of "direct response" to refer to the SST warming response without eddy feedback. Usually, the direct response refers to the response to radiative forcing before ocean warming, and the indirect response to that to SST warming (e.g. Shaw and Voigt, 2015). Please rephrase.

Agreed – another reviewer pointed out this sloppiness in our phrasing. We now distinguish between the direct radiative response and the slow vs. fast SST responses. Please see the revised text and added references.

l52: I think the poleward shift is only found for the zonal-mean NH jet stream. Please correct. The results of Liu et al. also suggest a strengthening of the NH zonal-mean jet at lower levels, though I agree that this is not found in Bellomo et al.

Agreed – we have taken care to distinguish between the zonal wind responses over the Atlantic (a strengthening and eastward extension) and over the Pacific (a poleward shift). This is an important point and we appreciate the reviewer for pointing this out.

l109: Do you mean that sea ice melting induces a regional cooling that decreases evaporation in ocean convective regions? Please be consistent with line 197, where you also mention a role for salinity changes.

At the request of another reviewer, we removed this material as it was tangential to the main points that we were making.

l133-135: I think referring to dynamical sensitivity is complicating the message, especially since it is not defined what it is meant. I think it could be simply phrased as to whether the circulation changes scale linearly or not with global-mean warming. I would postpone a discussion linking with the literature on dynamical sensitivity to the final discussion, and use a more readable language before.

We appreciate the reviewer's perspective, but we do not agree that we should reserve discussion until the end of the manuscript. Rather, we feel it is important to make a direct and clear link to these recent studies as they provide much of the motivation for examining the GMST and circulation responses that comprise Section 3. No changes to the manuscript.

l138: "it remains unclear": I agree this was never quantified, but, as far as I know, the proposed argument in the literature is always that the AMOC influences climate via changed T gradients / Bjerkness compensation. Please rephrase.

We do not agree that this is an obvious statement to the broader community, as reflected in the commonly used practice of pattern scaling (see the AR6 report). Therefore, while the reviewer may be right that the AMOC community may find this result obvious, we are seeking to broaden our reach here.

l169: Second part of Q1 is not clear. Please add: "response to an AMOC collapse induced by different CO2 forcing". It think it should be listed as a separate bullet point. I also think this second bullet could incorporate the first part of Q2.

Good points – we have rephrased Q1 and Q2 as suggested by the reviewer.

l172: It would be useful to make the second part of Q2 clearer, .e.g. is the impact of the AMOC mediated by GMST or by changes in temperature gradients? I would list this as a separate question.

Agreed – please see now the new questions Q1-Q3.

l241: I can't see any notable differences in the figure. Please clarify.

Agreed – the differences are too subtle to be significant. We have removed this sentence.

l262: I and C -> C and I

As we explain below, we have now incorporated the results of all ten members in the SSP 2-4.5 ensemble, defining now a "recovered" 8-member (sub) ensemble (SSP 2-4.5 R) and a "collapsed" 2-member ensemble (SSP 2-4.5 C). We feel that this has substantially reinforced the robustness of our results (and "R" and "C" are now more intuitive designations).

l265: ppb -> ppm

Thanks for catching this typo – it has been corrected.

l309: What is TropDMetricPSI?

This has now been removed.

l314: What is UAS?

As explained in the manuscript, UAS is a metric within the TropD package that uses the surface zonal wind to define the Hadley Cell edge (zero-crossing of the latitude of the surface zonal wind).

l318: These concepts were also mentioned in Schneider T, 2006.

Good point. This reference has now been added.

l323: "are closely collocated": collocation is not entirely correct; it would imply the stormtrack is located at the Hadley cell edge, but this is only true for seasons/regions with a merged subtropical and eddy driven jet. Please rephrase.

We inadvertently missed adding a "convergence" after the "momentum flux" – please see the revised text.

l326: It's not clear what cyclone tracking algorithm is adopted. Please clarify and provide some more details about previous studies that have employed this method.

In our attempt to trim the manuscript we have removed this section describing the zonally varying response (including the storm tracks).

l335: I understand you may want to keep consistency with the referenced paper, but I am just wondering whether T is the best variable to refer to fluxes, since T is also used to refer to temperature.

We agree that this may be confusing – however, we prefer to maintain as is to keep consistency with the referenced study.

l346: If you would like to use E2.1 as a short for GISS-E2-1-G, please specify it somewhere.

We already defined E2.1 when we introduced the model in Section 2. No change to the manuscript.

l365: The term "Equilibrated responses" in the section heading, and within the section, is misleading. The responses from the SSP simulations could be called quasi-equilibrium, but certainly not those from the Abrupt runs. Please correct.

We respectively disagree with the reviewer and lean on the large body of previous literature examining the abrupt 4xCO_2 simulations, all of which designate the 100-150 year average response as the "equilibrated" response. While she/he is certainly true that the AMOC (and other circulation features) may end up behaving quite differently were these integrations to be extended to millennial timescales, we reserve the right to keep consistent with the previous literature.

L378: remove "more realistic".

Done. Please see the revised manuscript.

l390: The enhanced gradient at the gulf stream is difficult to see and the impact of temperature changes on the zonal temperature gradient depends on the latitude. Please clarify.

We agree and have removed this from the (significantly revised) section.

L397: stronger meridional SSTs compared to what?

Please note that this section has been significantly revised.

l404: it would be easier to appreciate the lack of ENSO response if the difference plots were showed.

Please see the revised Figures 3 and 6, which now show the differences. There is no ENSO response in each comparison.

l408-419: Does it have to be dynamical? Couldn't it not be due to the thermodynamic advection of colder North Atlantic air?

Good point – we now include a caveat in this sentence mentioning this possibility.

l421: I don't agree that NH SSTs adjust within the first 100 years. In the scenario with an AMOC collapse, NH temperatures keep evolving during the entire simulation (this is clear in the Appendix 1 I-C panel). Please clarify/rephrase.

We completely agree and thank the reviewer for pointing this out! Indeed, especially over the Pacific northern latitudes, this slower SST response results in some differences with the 3xCO_2 simulation (where the jet not only accelerates, but also shifts poleward). Thank you very much for this comment – please see the revised manuscript.

l425: The reference to 2100-2200 is not consistent with the l 421. I would also say that the North Pacific Ocean adjustment takes longer than this. Please quantify/clarify.

Thanks. We have clarified this in the text.

Fig 4: these sub-subpanels are so small it's nearly impossible to see any detail.

We agree – the new figures are now much less cluttered as they now only show the "SSP 2-4.5C – SSP 2-4.5 R" and "3xCO_2 – 2xCO_2" anomalies.

l 479: Based on Fig A3, I see jet strengthening - with no poleward shift - in the North Atlantic at both 850 hPa and 500 hPa. The poleward shift seems to be confined in the North Pacific. Please modify. Regarding the presence of larger changes in the upper vs lower troposphere, it would be useful to examine whether this still holds in terms of percentage changes.

We completely agree and this point was also raised by the other reviewers. We have revised this text to reflect the different zonal wind response between the Atlantic and Pacific basins.

l483-494: this paragraph could be removed for the sake of space. KB2021 had to scale by mean warming to account for inter-model differences in climate sensitivity. In the SSP experiments, the differences in mean warming are due to the AMOC, so there is no particular point in scaling.

We agree – the normalization by GMST was adding unnecessary complexity. We have removed all related discussion.

l 532: KB2021 found a SH jet poleward shift. Could that be a consequence of the GMST normalisation too?

Good point. We now include a reference to this as another possibile reason for the SH jet response in that study (in addition to the different simulation lengths).

l530: It is difficult to tell without looking at the difference plot, but based on Fig 5b the AMOC collapse seems to induce a reduction of the SH jet poleward shift found in SSP-C. This seems consistent with the amplified Antarctic warming found in SSP-I. It this correct?

Please see the new version of Figure 4. In the SH there is, if anything, a uniform weakening of the SH jet.

l 535: It's is unclear to me that the NH EKE change can be interpreted as a poleward shift, instead of a strengthening. Could you introduce some metrics to quantify this?

We agree – we have now rephrased to emphasize that these changes reflect a strengthening. While the maps do show some suggestion of a poleward shift over the Pacific in the 3xCO_2-2xCO_2 comparison (Fig. 5b), it seem like the zonal mean response is dominated by a strengthening. Please see the revised manuscript.

l542-544: It is difficult to infer changes in Hadley cell edge based on Fig 5. Since the Hadley cell edge is discussed in detail in Fig 7, it might be useful to postpone this discussion.

We retain a passing reference to this change and clarify that this edge shift is most obvious in the lower troposphere for pressures greater than ~500-600 hPa. As this is evident in the zonal mean picture we do think it is important to mention here.

l547: I don't understand how shifts in the NAM would reinforce the Hadley/Ferrel cell coupling. Please clarify.

Agreed. We have removed this sentence.

l576: Fig 6 (and Fig 7): I am surprised by the 2XCO2 results. Are you saying that there is exactly the same mean warming and climate responses in the FOM and SOM setups? I would have expected at least some small differences to be present.

Thank you very much for this comment as it has highlighted the fact that we did not mention an important detail when making this figure. Indeed, the reviewer is correct in pointing out that the FOM and SOM setups do not produce the same warming at 2xCO2. In plotting these results we adjusted the SOM 2xCO_2 simulation to match that of the FOM results as we wanted to focus on the difference between their scalings, relative to CO_2 (and GMST). Again, thank you very much for highlighting this oversight, which we now clarify in the figure captions.

l588-591: I don't fully understand why the peak CO2 level is more relevant than the long-term level, especially considering that the temperature evolution is largely flat in the extended run. Please clarify.

We refer to the peak CO_2 level since the circulation will not necessarily scale with the long term (flat) GMST evolution, a point we have tried to highlight throughout.

l599: this is a very nice result. You could mention that the direction of the mismatch between the blue and cyan dots, for a given GMST, is consistent with the direct effect of GHGs, which tend to suppress global mean precipitation (see PDRMIP related papers, e.g. Samset et al., 2016).

This a very interesting point and one that we had failed to notice. Thank you very much for this observation, which we now note in the manuscript.

line 610: behaviour -> behaves

Thanks. This has been fixed.

line 615: suggests -> suggest

Thanks. This has been fixed.

line 617: simulations -> models

We actually mean “simulations” here, not models. No change to the manuscript.

line 629: you could note that the trends in the range 3-5xCO2 are approximately consistent between FOM and SOM (though the two are offset because of the AMOC-induced shift).

line 650-652: I don't see how these results can demonstrate the causality between changes in static stability, eddy flux convergence and Hadley cell edge shifts induced by the AMOC collapse. Attributing whether the driving comes changes in static stability, rather than horizontal temperature gradients, is particularly difficult. Please rephrase or remove.

We agree that it is difficult to establish causality between these responses, but, as we do not seek to reinvent the wheel, we lean on the previous studies (particulary Chemke and Polvani (2019) and Menzel et al. (2019)) who showed that the changes in HC edge respond on a similar timescale as the latitude of maximum eddy momentum fluxes. Chemke and Polvani (2019) also used this to implicate static stability changes as a potential leading driver of the momentum flux changes.

Of course, none of this really proves causality, however, so we agree with the reviewer that more care must be taken to acknowledge this. We have tried softening our language when discussing this material.

line 658-664: this paragraph is unclear. Please clarify or remove for sake of space.

We agree. We have removed this paragraph.

line 673-683: I think this paragraph could be removed with little loss of content. I agree that these four dynamical quantities are related - and this is consistent with their similar time evolution - but showing this figure/discussion adds very little on their cause-effect driving.

We agree with the reviewer that this doesn't prove causality, but we reserve the right to show this figure because it does show a similar timescale of response between these variables, something which is highlighted even more now that we have included the results from the full ten-member SSP 2-4.5 ensemble. As the timescale of the response has been used to suggest that certain quantities are linked to others (see Chemke and Polvani (2019)) we feel that this adds some value. Hopefully the reviewer is open to retaining this figure, especially as we have significantly reduced the text and removed several of the appendix figures.

line 686: dynamical sensitivity -> atmospheric circulation

As explained earlier we wish to retain this language.

Line 692: I think Fig 8 can be removed for the sake of space. It adds little content in my view.

Please see our response to the previous related comment.

line 701: I would avoid using "abrupt" - here and in the following lines - to discuss the change between the 2xCO2 and 3xCO2 experiments, since these are two distinct experiments and there is no abruptness from a time evolution perspective.

Good point – we have replaced "abrupt" with "large".

line 733-744: this paragraph is confusing. It is not clear what net energy loss refers to. Are you referring to the reduced atmosphere+ocean poleward energy transport or to some change in the net globally-averaged energy balance, or to local imbalances in the North Atlantic? Please clarify and rephrase.

We are referring to the reduced atmosphere + ocean poleward energy transport (i.e. solid lines in (new) Figure 10). This has been clarified in the revised version of the manuscript.

line 757-758: Another reason of the possible cause of the difference in oceanic compensation may be the different length of the simulations, and hence their degree of equilibration. Especially considering that the differences between 3xCO2 and 5xCO2 are relatively small compared to those between SSP I vs C. Please discuss.

Excellent point – we now briefly mention this in the text.

line 779-788: for the sake of length, this paragraph may be sacrificed ... as the authors conclude: "these results are not too surprising".

Fair point, but we have chosen to retain this paragraph in light of the significant cuts that we have made to other sections of the manuscript. No changes made.

Line 794 and following: This section should be either removed or expanded. If kept, it would be useful to first analyse the contribution of transient vs stationary waves to the changes to the zonal-mean DSE transport. This cannot be inferred from Fig 11, since there is substantial compensation in the zonal direction associated with the stationary wave component. The discussion seems to assume that stationary waves are the dominant process, but this is not given.

This entire section has now been removed.

line 832-833: this sentence seems in contrast with the previous discussion of Fig 3, in which it was highlighted that the changes in temperature gradients were largest in the tropical Atlantic. Please clarify.

This entire section has now been removed.

line 851: It is hard to tell without the difference plot, but I would rather say the West Pacific seems entirely insensitive to the AMOC collapse.

This entire section has now been removed.

l853 and following: this seems a long discussion to just say that the AMOC weakening seems to have very little impact on the Walker cell. Please consider being more synthetic.

This entire section has now been removed.

Fig 14: It is nice to document storm track changes, but I am not sure this has added much information to the overall discussion. For sure, the figures are so small and full of detail that it is difficult to inform what the AMOC impact is.

Agreed. This entire section has now been removed.

l 879: Given the amount of spatial noise, could you please clarify whether the increase in the North Pacific storm intensity is statistically significant?

This entire section has now been removed.

l 889: for the sake of consistency, why not looking at members A and B for all the analyses in the paper then? Please explain.

Please note that now we have included the results from all ten members of the SSP 2-4.5 ensemble.

L 899: add: "sensitivity to greenhouse forcing"

Good point – this has been added.

l 925: what is an AMOC type freshwater forcing? please clarify.

Apologies – we realize this was obscure. We have replaced with “freshwater hosing forcing”.

l 933: I found that the goal of this discussion wasn't very clear. Could you try to spell out more clearly what is the question being discussed and why it is important? Are you asking why there is a compensation between LE and DSE changes in the tropics, but not in the NH extra-tropics?

We are asking why the compensation occurring in the NH extratropics is dominated by the DSE changes and not by the LE changes. We have tried to make this point clearer – please see the revised text.

l 934: as previously mentioned the term abrupt compensation does not seem appropriate to me.

As earlier, we have replaced "abrupt" with "large".

line 936: I would rather say the increase in DSE dominates northward of 20N. Please rephrase/clarify.

This has been fixed.

l941: it is not correct to say that changes in heat transport drive changes in dynamical aspects. If anything, it is the other way around, though there are mutual interactions. Please rephrase or clarify.

Good point – we have rephrased to remove any suggestions of causality. We now refer to "not fundamentally associated with" instead of "not fundamentally drive".

l958: Also in this section it would be nice if you could be clearer about what is the key question being discussed. Are you asking whether the atmospheric circulation changes might themselves contribute to reinforcing the AMOC weakening, via interactions along the gulf stream?

This section was removed.

l959: zonal --> zonally asymmetric

This section was removed.

l 959: 3e -> 3c

This section was removed.

l1010: what is ModelE?

Apologies – this is the encompassing term for all versions of the GISS model (including E2.1, E2.2, etc.). We have now replaced with "the GISS climate model."

l 1017: please add: "in presence of an AMOC collapse" after "does not scale GMST."

Done. Please see the revised text.

l 1017-1018: I find too simplistic saying that dynamic sensitivity does not scale with equilibrium climate sensitivity. For examples, based on the conceptual models of Grise and Polvani 2016 and Ceppi et al 2018, one would still expect that the doubling of the forcing leads to a doubling of the "equilibrated" circulation change. The current manuscript, and the cited papers, refer to very different situations: the present studies shows a real non-linearity in the amplitude of the forcing, while the others refer to (linear) superpositions of responses emerging on different time scales. Please rephrase or remove.

We have not "shown a real non-linearity in the amplitude of the forcing" – indeed, we never plotted the radiative forcing (as would be gauged from fixed SST experiments) as a function of CO_2. Rather, we have shown that the large-scale circulation response does not scale with GMST (which contributes, in addition to radiative feedbacks, to equilibrium climate sensitivity). The previous studies have attempted to assess this relationship by, for example, correlating HC edge responses with GMST so we do not agree with the reviewer that the context of our presentation is very different from that of these studies.

l 1050: These results also seem very relevant for the purpose of developing storylines of atmospheric circulation change (e.g. Zappa and Shepherd, 2017)

Good point– this is now mentioned in the text.

References

We thank the reviewer for these references, which we now cite in the manuscript.

Shaw, T., Voigt, A. Tug of war on summertime circulation between radiative forcing and sea surface warming. Nature Geosci 8, 560-566 (2015).

Schneider, T., 2006. The general circulation of the atmosphere. Annual Review of Earth and Planetary Sciences, 34, pp.655-688.

Samset, B.H., Myhre, G., Forster, P.M., Hodnebrog, Ø., Andrews, T., Faluvegi, G., Flaeschner, D., Kasoar, M., Kharin, V., Kirkevåg, A. and Lamarque, J.F., 2016. Fast and slow precipitation responses to individual climate forcers: A PDRMIP multimodel study. Geophysical research letters, 43(6), pp.2782-2791.

Zappa, G. and Shepherd, T.G., 2017. Storylines of atmospheric circulation change for European regional climate impact assessment. Journal of Climate, 30(16), pp.6561-6577.