

Author response to referee comments on Chrysanthou et al. (2020) “Decomposing the response of the stratospheric Brewer-Dobson circulation to an abrupt quadrupling in CO₂” submitted to Weather and Climate Dynamics Discussions

Reply to Anonymous Referee #1

General comments:

This is an interesting, relevant and well-presented paper. The authors consider the drivers behind the response of the BDC to 4xCO₂ using 50-year simulations of HadGEM3. As such, the paper serves as an experimental report. The authors decompose the components of the response into those arising from rapid adjustment (holding sea-ice and SSTs constant at pre-industrial values), the global-mean SST warming (relative to pi-control), and the specific pattern of SST warming (global mean removed). I can recommend the paper for publication with a few changes as I have outlined below, none of which are particularly major and most of which just improve the readability and flow of the manuscript (to allow the science to stand out). I have a few specific comments which pertain more to the choices the authors have made in what they show/do.

Thank you for your positive comments and suggestions for improving the readability of the study. We reply to the specific points raised below in red.

Specific comments:

L47: I think “turnaround latitudes” needs to be briefly explained here.

Thanks for this suggestion. The first mention of the turnaround latitudes is on line 44 - we have updated the text there.

L57: I am not sure why the more general “tropical waves” is mentioned and then elaborated as “equatorially trapped quasi-stationary Rossby waves”. One is more general, while the other is more specific. Only one is necessary.

Thanks for catching this. We have kept the more general reference to tropical waves.

L140: Would it not have also been possible to perform an experiment where sea ice is allowed to vary? Comparing the results of this perturbation with those where it is held constant could show some of the possible effects on the BDC to which the authors allude is “uncertain”.

As suggested, we have performed a further simulation in which sea ice + SSTs are changed based on the multi-model mean CMIP5 4xCO₂ experiment. We compare this “full 4xCO₂ + sea-ice loss” with the full 4xCO₂ experiment (run B) as described in the manuscript. The annual mean w* anomalies due to the changes in sea ice are shown below in Figure R1. This shows no significant changes in w* due to the additional effect of sea ice loss. We have added a comment in the Methods to say we have tested the sea ice effect and it was negligible.

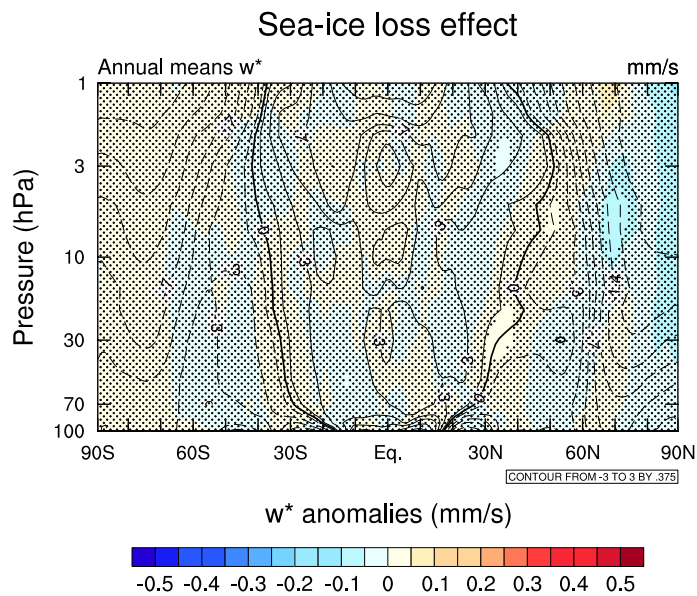


Figure R1: Annual mean w^* anomalies (mm/s) due to $4\times\text{CO}_2$ sea ice changes. Anomalies are calculated as the difference between the “full $4\times\text{CO}_2$ + sea ice loss” and “full $4\times\text{CO}_2$ ” (run B) experiments. Stippling denotes differences that are not significant at the 95% confidence level.

L242-245: This mention of changes to the QBO is tantalising! Would it be possible to include this in at least supplementary figures? This is up to the authors, and I agree it is not the focus of the study, but this mentioning of it without further information leaves me wondering what stones are unturned.

We agree this point is also interesting, but we feel it would be a separate study in its own right and that to offer enough detail to satisfactorily explain the QBO changes would detract from the main focus of the paper. We have therefore removed the mention of the changes in QBO and defer this for future study.

L403: The statement “the SST pattern imposed here is very different from a canonical ENSO SST pattern” confused me. Is it? Does this refer to the global pattern, or the tropical Pacific in particular? This seems important given what is mentioned on L90.

Thanks for pointing this out. The issue of the SST pattern was also raised by reviewer 3. On reflection the description was an oversimplification. ENSO SST anomalies are confined to the tropical Pacific, whereas the $4\times\text{CO}_2$ SST pattern shows features globally (by construction). This includes relatively higher SST across the tropical oceans and North Pacific and relatively cooler SSTs in the Southern Ocean. We have amended the text in the Methods to provide a more nuanced discussion about the features of the $4\times\text{CO}_2$ SST pattern (lines 163-165). We still discuss the BDC response to ENSO but only as a point of comparison in terms of the magnitude of its effect (tracked changes manuscript lines 461-462).

L384/Figure 10: the SST pattern effect result is non-significant (the confidence interval overlaps with 0). This should probably be mentioned.

Thank you for pointing this out. We have added this point to the text: “though the latter is not statistically distinguishable from internal variability”.

Method: What method was used to determine the 95% confidence levels? I don't think the authors have stated this.

Thank you for spotting this. The statistical significance was computed based on a two-tailed Student's t-test. We have now updated all relevant figure captions (Figures 2, 5 and 7) to include the statistical method of calculating the 95% confidence.

Results: There are cases where the individual component results are compared in a qualitative sense (e.g. L280), but it would be useful if these were sometimes more quantitative (e.g. X% more: : :) like in section 3.5.

This surely improves the comparison and brings out a more quantitative sense of the responses of all perturbations. We have now quantified the contributions of each component in the annual-mean residual circulation response (Figure 4) as well as in the seasonal mean mass streamfunction anomalies (Figures 5 and 6) in this part of the text to reflect your suggestion.

Figures: In general I did not notice that the bottom y-limit changes quite a bit between each figure, as they are all in the same format. Perhaps worth mentioning in the captions.

Thank you for pointing this out, we have now updated the relevant figure captions to note the pressure range in the y-axis.

Technical corrections:

L75: GEOS is not defined here. It is not particularly important, but it stands out as all other acronyms are.

We have added the definition for GEOS.

L90: Tilde is missing from Niño.

Thanks for this, we have now corrected it.

L90: Although it would be common to say "ENSO itself" and not "The ENSO: : :", so I understand why the authors have written it in this way, I think this sentence should begin with "THE El Niño Southern Oscillation: : :".

Thanks for this, it makes more sense to add a "The" in the beginning of the sentence.

L91: Capital H on Hemisphere (also elsewhere)

Thanks for this, we have now changed this here and in line 95.

L95: Definite article is missing " : : :using THE Whole Atmosphere: : :"

Added "the" before mentioning WACCM.

L109: In the list of the components, the phrasing on (2) is slightly different to the other two, and different to how it is in the abstract which makes it 'flow' less well. Consider using the phrasing as used in the abstract "a contribution from: : :"

Thanks for this suggestion, we have now updated the text to reflect the listing of the components as seen in the abstract, as per your recommendation.

L172: Missing capital I on McIntyre (also in the references)

Thank you for spotting this, we have now corrected this.

L173: The final Andrews citation should probably be non-parenthetical “: : defined following Andrews et al. (1987): : :”

Again, thanks for spotting this, we have corrected this.

L175/equation 1: are the dots necessary? These are not consistently used in the equations shown in this paper and are not standard for scalar multiplication.

We agree that this was not consistent with the rest of the mathematical formulations of the study, so we removed the dots.

Equation 4: The integral is missing the variable of integration (dz')

Thanks for spotting this, we have now added it.

L204: “maximum” should be “maximised”, and a mention of by how much would maybe be good here

We corrected this to “maximised” and added the amount of tropospheric warming induced in that vicinity (~ 8 K).

L209: Which figure panel is run C?

This was an oversight which was corrected. Thanks!

L211-213: Is this sentence describing how the greenhouse effect works really needed?

We agree that it is redundant, so we removed it.

L230: Here, and elsewhere, additional hyphenation increases readability. For example, please revise to “annual-mean zonal-mean zonal wind”.

Thanks for this suggestion, we have corrected this here and elsewhere in the text.

L256: Insert “but small”, for “significant, but small, increases: : :”

Thank you for this suggestion, we have implemented it in the text.

L263 & 266 & 267: Why is Hardiman et al. cited for results that are in the figures? If it is to say that the result is consistent, then please state as such.

We have removed these citations.

L272: Consider changing $p < 10$ hPa to “below 10 hPa” for readability.

Thanks for this suggestion, we have updated the text.

L310: Eliassen-Palm Flux is earlier abbreviated to EPF.

We have now corrected this, thanks.

L405-410: “important role” and “decomposition performed here” and both repeated twice in the same paragraph. Consider revising one of each to a different phrase.

We have altered the second sentence to reflect your suggestion, which now reads: “*However, our results demonstrate that an increase of the BDC in the upper stratosphere comes mainly from the radiative cooling of the stratosphere by CO₂, as seen in the rapid adjustment component of the response*”

L437: “the projection reduction” should be “the projected reduction”

Nice catch, thanks.

L634: This reference has two hyperlinks.

We removed the additional hyperlink, thanks.

L637: Is this cited in the text? NCL is credited in the acknowledgments but is not linked to this reference.

It was only cited in the acknowledgments however the bibliography entry was wrong, thanks for spotting it. We have now corrected this.

Figure 1: For (b), some specification that the contour value is 3.4 K would be helpful.

Thank you for this, we have now added a string on Fig. 1b that states the value of the constant contour value of 3.4 K. The colour of the contour reflects the colormap of Fig. 1a.

Figure 2 (and similar): It would be helpful if the figures had the experiment labels on them, or in the caption, as this can get confusing.

Thank you for this suggestion. We have updated with the experiment labels the captions of all related figures to clarify which panel corresponds to which experiment.

Figure 6: This shows EP flux vector ANOMALIES which is not stated in the caption.

We appreciate that you spotted this, we now state that these are the EPF vector anomalies in the newly added DJF-mean Figure 7 caption.