Weather Clim. Dynam. Discuss.,
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Interactive comment on "Decomposing the response of the stratospheric Brewer–Dobson circulation to an abrupt quadrupling in CO_2" *by* Andreas Chrysanthou et al.

Anonymous Referee #1

Received and published: 14 February 2020

General comments:

This is an interesting, relevant and well-presented paper. The authors consider the drivers behind the response of the BDC to 4x CO2 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.

Specific comments:

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

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.

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”.

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.

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.

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

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

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

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quantitative (e.g. X% more...) like in section 3.5.

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.

Technical corrections:

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

L90: Tilde is missing from Niño.

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...".

L91: Capital H on Hemisphere (also elsewhere)

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

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..."

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

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

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.

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

L204: "maximum" should be "maximised", and a mention of by how much would maybe be good here L209: Which figure panel is run C?

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

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

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

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.

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

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

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.

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

L634: This reference has two hyperlinks.

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

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

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.

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

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