Author response to referee comments on Chrysanthou et al. (2020) "Decomposing the response of the stratospheric Brewer-Dobson circulation to an abrupt quadrupling in CO_2" submitted to Weather and Climate Dynamics Discussions

Reply to Anonymous Referee #3

Changes in the Brewer-Dobson circulation (BDC) due to increased CO2 levels are studied by distinguishing the response to CO2 changes in the atmosphere only, globally uniform changes in SSTs, and SST pattern changes. The former corresponds to the rapid-adjustment of the climate system when CO2 levels are increased abruptly. The latter two correspond to long-term changes due to the longer time scales of the oceanic response. These effects are studied consistently by using a single state-of-the-art climate model (HadGEM3-A). The BDC generally increases in strength due to increased CO2. The authors find that in the lower stratosphere the majority of this BDC strengthening can be attributed to globally uniform SST increase. In the upper stratosphere the changes due to rapid adjustment are of similar magnitude. The authors furthermore estimate a linear sensitivity of the change in BDC strength as a function of global surface warming of roughly 9 %/K in the lower stratosphere and 6 %/K in the upper stratosphere. Overall, the paper is well-written and the results are straightforward. I have a few general comments that I hope will help the authors to sharpen their discussion and to better put the work into broader context. Other than that I only have minor comments; once these comments have been taken into account this manuscript should be publishable.

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

General comments:

SST pattern changes and ENSO: there are frequent remarks about the resulting BDC changes from the SST pattern changes to be similar to ENSO-induced anomalies. However, in the discussion section (line 403) the authors remark that "the SST pattern imposed here is very different from a canonical ENSO SST pattern". If that is the case, isn't it surprising then that the BDC changes due to the SST pattern changes look similar to those due to ENSO? To me this calls for corresponding discussion/elaborations somewhere in the manuscript.

Thanks for the comment. The issue of the pattern was also raised by reviewer 1. On reflection this description was an oversimplification. ENSO SST anomalies are confined to the tropical Pacific, whereas the $4xCO_2$ 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 $4xCO_2$ 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).

Shallow versus deep branch changes: it seems that the authors interpret changes in upwelling strength through 70 hPa as representative of the shallow BDC branch, whereas those at 10 hPa as representative of the deep branch. Although it is certainly true that there isn't a clear vertical level where the shallow branch stops and the deep branch takes over, perhaps a useful distinguishing factor is related to the tropical pipe concept (much less meridional exchange at pressure levels within the tropical pipe than below). The shallow branch could then be interpreted as being primarily confined to that part of the BDC that involves strong meridional dispersion below the bottom of the tropical pipe. My recollection is that this level (bottom of tropical pipe) is close to 70 hPa, although this may vary from model to model. By that argument the upwelling through 70 hPa is more a measure of the deep rather than the shallow BDC branch and neither of the quoted upwelling changes correspond to the shallow branch strength. I admit that all of this may be a bit philosophical, but the authors may wish to include a bit of discussion on this point. This isn't an issue when simply referring to 70 vs. 10 hPa without the connotation of shallow vs. deep branch. But even in that case, one wonders about BDC changes in the lower half of the stratosphere (assuming a global mean tropopause pressure somewhere around 150 hPa, roughly half of the stratosphere is located below 70 hPa)

We agree there is no accepted pressure level to delineate the different branches of the BDC, that different levels have been used throughout the literature, and that these levels likely vary in models too. Our choice of levels to focus on was partly motivated by the comparison with earlier model intercomparisons (e.g., Fig 4.10 SPARC, 2010), which tended to analyse the BDC at 70 hPa and 10 hPa. For clarity, we have amended the text to always refer to the specific pressure levels and/or to the lower and upper stratosphere rather than to the shallow and deep branches. Based on the reviewer's points about the mass flux in the lower stratosphere, we have also amended our cross-section plots to show levels down to 150 hPa.

Seasonal versus annual means: residual circulation changes are shown in terms of annual means in the main manuscript, whereas those of EP flux divergence are shown in terms of seasonal means. Line 308 presents a specific argument in favour of seasonal means. I didn't understand why this argument should apply to the wave forcing but not the resulting residual circulation, hence why seasonal means were delegated to the supplement in the case of the residual circulation? Please clarify somewhere.

Following this comment, we have moved the seasonal mean psi* Figures S1 and S2 to the main text and moved the annual mean psi* in Fig. 5 to the supplement. This part of the manuscript was rewritten to reflect the above changes.

Minor comments:

line 47: not sure I can follow the argument here - why couldn't the wave forcing around the turnaround latitudes change if there was a change in wave activity from the troposphere?

This sentence was indeed misleading. We meant that the wave forcing *change* needs to take place near the turnaround latitudes (TL) to directly affect the BDC. Poleward of the TL and within the deep tropics it will only lead to a latitudinal re-distribution of the downwelling or upwelling respectively (Shepherd and McLandress, 2011). We have updated the text to clarify this point.

line 51: the Randel and Held reference is appropriate for the connection of wind pattern and critical levels, but I don't think these authors talked about an upward movement of critical levels due to climate change; so the placement of this reference may be misleading

Indeed, the placement of this reference was misleading, so we have now removed it. We added a new sentence to better put into context the observed link between the wind patterns and the critical levels of wave breaking associated with an accelerated BDC under climate change.

line 81: GEOS-CCM model: the acronym "CCM" already contains "model"

Thanks for spotting this, we have now removed the word "model".

line 88: here and at other place: "warmer SSTs" should be something like "higher SSTs" (or "warmer sea surfaces")

Following your suggestion, we have changed the warmer SSTs to "higher" SSTs.

line 106: "three distinct effects" – would be good to briefly remind reader about the effects

This suggestion surely improves the readability in this part of the manuscript, so we briefly list these effects. Thanks!

line 144: is the runtime for the 4xCO2 experiments long enough to call the final state "quasi-equilibrium"? I could imagine that there's still drift due to ocean response, even after 150 years.

The reviewer is correct that the climate has not fully equilibrated after 150 years. We have removed the phrase "quasi-equilibrium" and replaced it with "centennial".

line 147/148: please add comment about the 3.4 K warming, especially comparing it to the equilibrium response to 4xCO2 (which should be double the climate sensitivity if I understand correctly, so the 3.4 K value seems small)

This value is not comparable to the equilibrium climate sensitivity (ECS), since this is the global surface temperature (land+ocean) and we consider only the global mean SST change. Since the land warms more than the ocean, we expect our imposed global SST change to be smaller than 2xECS. Furthermore, our warming is calculated over years 101-150 whereas ECS extrapolates to equilibrium which takes millennia to reach (Rugenstein et al., 2020). We have added a comment in the Methods on this point: "Note the global mean SST is smaller than the global mean surface temperature change in the abrupt-4xCO_2 experiment because land areas warm more than the ocean (e.g., Joshi and Gregory, 2008)."

line 175 and following: please clarify use of vertical coordinate; your model runs in height coordinates (not log-p height), but the TEM diagnostics are formulated in log-p height – was this done by first interpolating the data?

Thank you for spotting this, the equations were incorrectly written in the log-p coordinate system although the MetUM uses the primitive equations with the log-pressure z = -H ln(p/ps) coordinate. We did not perform any interpolation; we used the direct model output which was calculated based on the equations 3.5.1a and 3.5.1b from Andrews et al., (1987).

line 181: I assume that Eq. 2 is only integrated to the respective vertical level of interest (so that Psi* is still function of log-p height), not all the way to the surface (unless for lowest level)? Also, your definitions in Eqs. 1 and 2 are circular: Eq. 1 requires knowledge of Psi* and Eq. 2 requires knowledge of v* ... even though these are standard diagnostics, it would be more helpful to define the residual streamfunction first based on the vertically integrated v and the heat flux contribution; then define v* and w* (or, alternatively, define v* and w* in terms of v and w + heat flux contribution; then use Eq. 2 for Psi*).

For the first part of the question, yes for each level, we integrate from the top of the model to that particular level. Following up from your previous comment, we now define the residual circulation equations as shown in Andrews et al., (1987), changing our equation 1 and keeping equation 2 as it is in the manuscript. We have also updated the text to reflect these changes.

Eq. 4: the integral is missing a "dz"

Thanks for spotting this, we have now added it.

line 244: please make more definite statements about the direction of changes of these QBO characteristics (or omit the comment altogether)

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 amended the text: "This is likely related to changes to the QBO properties under climate change, which have been noted in other idealised GCM experiments (e.g. Kawatani et al., 2011), though a detailed investigation of the QBO is beyond the scope of this study".

line 259: this is a good example where you make a reference to ENSO-like SST perturbations, but fall short in discussing how your SST pattern changes actually do correspond to ENSO (or not)

Thanks for pointing this out. The issue of the pattern was also raised by reviewer 1. While the SST pattern shows an El Niño-like warming across the equatorial Pacific, the pattern shows other pronounced features such as relatively warmer SSTs across all tropical ocean basins and the North Pacific and relatively cooler SSTs across the Southern Ocean. We have clarified the text on the SST pattern and its interpretation in relation to the local tropical Pacific anomalies vs. the global pattern.

line 263: this is a good example where I found your reference to the shallow BDC branch confusing – to me it doesn't really extend to 30 hPa

We have reworded the text to make this clearer.

lines 299/300: should this result perhaps be shown / referred to right away with the methods section? Also: it sounds a bit misleading to me to start the paragraph with "An important question" and then talk about results shown in the supplement – if they really are important, why aren't they shown in the main part of the paper?

We prefer to place this discussion here because it follows from the detailed discussion of the individual responses and would therefore be premature in the Methods. We have reworded the opening phrase of the paragraph from "An important question..." to "We lastly consider...".

line 330: SSW -> SST

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

line 350: could you elaborate where this 20 % disagreement could come from?

The direct psi* calculation is derived from v* (see Equation 2). This was used because it was found to be less noisy than performing an equivalent integration of w* in latitude. However, in previous work we have noticed that, for model pressure level data, the psi* estimated from v* is generally larger than that from w* (see Fig. S2 of Dietmüller et al., 2018). We hypothesise that this is the source of the difference between the direct and downward control calculations, though we cannot explain its origin. We have added a reference to this in the text.

line 400: the statement is based on results from this paper, so I assume the reference to Lin et al. is meant to state that they found similar results? Please clarify

Thanks for spotting this, this was referring to the fact that the Lin et al., (2015) study had similar findings. We have slightly updated the text to better communicate this.

line 434: this value (~9 %/K) is exactly equal to the one you quote for your results, so the agreement is exact (or almost exact) and not just "relatively good" - am I missing something?

Thank you for pointing this out, as a matter of fact it is in exact agreement. We have now updated the text to reflect this.

line 675 (Fig. 3 caption): the u=0 lines are only critical lines for stationary waves - please clarify

Thank you for bringing this to our attention. We now clarify that the critical lines refer to stationary waves.

Fig. 4 and related discussions: visually, it doesn't look like the positive anomalies compensate the negative anomalies on a given pressure level (perhaps when scaled by surface area they do), but shouldn't they based on mass conservation? Did you check?

Yes, we have verified that the cos(latitude) area weighted w* anomalies on a given pressure surface produce a very small residual, e.g. at 70 hPa the area average value is -0.013 mm/s for the full experiment (annual mean), which is around 20 times smaller than the magnitude of anomalies at individual latitudes.

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