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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 #3

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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

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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.

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.

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

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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) ...

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.

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?

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

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

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

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

line 144: is the runtime for the $4xCO_2$ experiments long enough to call the final state

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"quasi-equilibrium"? I could imagine that there's still drift due to ocean response, even after 150 years.

line 147/148: please add comment about the 3.4 K warming, especially comparing it to the equilibrium response to $4xCO_2$ (which should be double the climate sensitivity if I understand correctly, so the 3.4 K value seems small)

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?

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*).

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

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

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)

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

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?

line 330: SSW -> SST

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

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

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?

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

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?

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