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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The advective Brewer-Dobson circulation in the ERA5 reanalysis: variability and trends" *by* Mohamadou Diallo et al.

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Please find enclosed the responses!

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-881, 2020.

Answer to Reviewer's comments on "The advective Brewer-Dobson circulation in the ERA5 reanalysis: climatology, variability and trends" by Mohamadou Diallo et al.

We are submitting our revised article titled "The advective Brewer-Dobson circulation in the ERA5 reanalysis: climatology, variability and trends". We thank the two Reviewers for their detailed and well thought-out comments, which helped to significantly improve the paper. We have made substantial changes to the manuscript in order to thoroughly address the Reviewers' suggestions and comments. Main changes concern:

- The calculation of residual circulation from wave drag using the downward control principle, as suggested by Reviewer #1, a new figure showing these results and the related discussion.
- Addition of statistical significance using Student's t-test to the differences as suggested by Reviewer #1
- Addition of information related to S-RIP and references.
- Re-calculation of the RCTT using the w* instead of heating rates for 2010-2018.
- rephrasing of several paragraphs in order to clarify the manuscript.

With these changes, we are convinced that the paper has been significantly improved and is highly relevant for a wide-ranging journal like *Atmospheric Chemistry and Physics*. Please see below our answers point by point to all reviewers comments and suggestions.

Reviewers comments are in bold, followed by our respective replies. Changes in the manuscript are in blue, allowing them to be tracked easily.
Kind regards,
Mohamadou Diallo (on behalf of the co-authors)

Anonymous Referee #2:

General comments:

1. ***- While the detailed comparison between ERA5 and ERA-interim is very useful, it would also help to have some discussion linking how these reanalyses compare to other reanalyses. It is briefly mentioned that previous work has shown ERA-interim to be to have too strong upwelling, but it would be nice to know more. Since this paper is part of the S-RIP special issue, it would the authors better tie their work in with the other S-RIP work on the BDC. I'm not sure if the authors are contributors to the S-RIP BDC chapter, but I would recommend they get in touch with the authors of that chapter and have some content on the broader context of reanalysis representations of the BDC.***

   We thank the Reviewer for this thoughtful suggestions. We are involved in several S-RIP chapter, including the S-RIP BDC chapter 5. We have enhanced the discussion about the comparison to other reanalyses, about the too strong upwelling in ERA-Interim and the S-RIP work on the BDC at needed places in the manuscript (page 4, lines 26-30 and section 4).

2. ***- I found some of the discussion of the regression modeling confusing, and would appreciate if the authors could make some of this clearer. The most unclear part is the discussion of things like QBO amplitude variability that is plotted in figures 7 through 9. This is not really well defined in the paper. I'm guessing it might be the QBO coefficient in the regression fit, or it might be something like the RMS of the QBO timeseries for the fit. Also, the authors regress things like the zonal wind field against zonal wind defined at a specific level (i.e., the QBO defined as zonal wind at 50 hPa), which is a bit odd and requires a bit more nuanced interpretation. When doing something like this, the correlations at a higher level (at, say, 30 hPa) aren't really "caused" by the QBO wind at 50 hPa, but rather reflect the climatological structure of how the equatorial zonal winds propagate downward as part of the QBO. I think the discussion around these figures (7-9) results could be clarified on this point.***

   We thanks the Reviewer for pointing this out. The simplified description of the regression model was motivated by the fact that the regression model is well described in our previous studies (e.g. Diallo et al. 2018, 2019). Our regression model uses a lag term, therefore, allowing us to use only one QBO proxy

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Fig. 1.