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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.

Anonymous Referee #2

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This is an interesting and overall well written paper comparing the representation of the Brewer-Dobson circulation in the latest ECMWF reanalysis, ERA5, with its predecessor, ERA-Interim. I have only a few general points and some minor revisions to suggest:

General comments:

- 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. - 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.

Specific comments:

Pg 1, line 21: gain -> gained

Pg 1, line 22: distributions -> distribution

Pg 2, line 6: ascent -> ascend

Pg 2, lines 31-32: "... strengthen the BDC, consistent with negative temperature trends" – Do you mean negative temperature trends in the tropical lower stratosphere? Please clarify this statement

Pg 2, line 34: e.i. -> i.e.

Pg 2-3: I'm a bit confused by the statement that reanalyses show BDC lower branch strengthening whereas figures like Fig 12 seem to indicate a slowdown (at least in ERA-interim). Are the reanalyses actually consistent on this, or is it just some of them? This comment ties in with my general comment about placing the ECMWF results within the

broader context.

Pg 4: Both ERA5 and ERA-Interim use a hybrid vertical coordinate system. Are the authors using model level data for both calculations, or the lower vertical resolution pressure-gridded data typically provided by reanalysis centers? If using model level data, please state so explicitly. Also, it would be helpful to have some mention what kind of error in the TEM calculations would be induced by using data that is not strictly on pressure levels. Related to this, what is the lowest level (i.e., highest pressure value) at which the model levels are pure pressure levels (i.e., where b=0 for a vertical coordinate system where p = a + b*PS), and is this different between ERA-Interim and ERA5? This might impact the discussion of the residual circulation below 20 km.

Pg 4, line 19: What do the authors mean as the ERA5 dynamical fields are archived as "tendencies" over one hour? I assume fields like u,v,T are either archived as averages over some period (e.g., 1 hour) or are instantaneous quantities.

Pg 4, line 29: Omega is introduced here, but is not used until f is defined in the following sentence. I recommend to define omega in the sentence defining f.

Pg 5, line 3: "is a the" -> "is a"

Pg 6, line 10-11: references need proper parentheses.

Pg 7, line 8: "which the" -> "which are the"

Pg 9, Fig 2: I believe the lower row of plots is showing ERA5 minus ERA-Interim, expressed as a percent difference. Please make this more clear in the caption.

Pg 10, line 9: I don't really see ERA5 being stronger than ERA-Interim above 20 km in general. It does appear to be stronger from about 35 to 40 km (Fig 2e).

Pg 10, line 11: The use of a 15 sigma error bar strikes me as odd. Please justify the use of this quantity.

Pg 10, line 11-12: I don't see what the authors are pointing out here. It looks to me like

the variance increases with height.

Pg 14, line 7-8: Why is only a 2 year period used? Regarding the period used, I assume the 2017-2018 period is somewhat of a best case scenario given that there are more/better observations constraining the reanalysis in this period than, say, in the earliest part of the record. Pg 14, line 14: mode -> modes

Pg 14, line 32: For those of us not intimately familiar with non-orographic gravity wave drag parameterizations, could the authors give some brief information on the difference between the schemes?

Pg 17, line 10: Fig 8c,d should be Fig8 a,b.

Pg 18, line 5: Fig 8a,b should be Fig8 c,d.

Pg 22, line 33: awkward wording. Maybe the authors mean "therefore" instead of "there"?

Pg 24, Fig 11a. I don't understand why the authors are showing the streamfunciton here instead of w*

Pg 24, line 3 and Fig 12: The authors should show the uncertainties on these trends. Does the ERA5 trend use the ERA5 v5.1 data for the period 2002-2007? It would be really helpful to include both ERA5 and 5.1 in the timeseries plot, trend numbers, and discussion. It may not matter, but it's not obvious whether or not that is the case.

Pg 26, line 22: Could the authors be more specific about resolved vs. unresolved GW forcing in the discussion here, and elsewhere in the manuscript?

Pg 26, line 24: large -> larger

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