

## **Comments on “Climatological impact of the Brewer-Dobson Circulation on the N<sub>2</sub>O budget in WACCM, a chemical reanalysis and a CTM driven by four dynamical reanalyses” by Daniele Minganti et al.**

The manuscript presents an evaluation of the climatological impact of the stratospheric BDC on the long-lived tracer N<sub>2</sub>O using the inter-comparison approach of the TEM budget estimated from BRAM2 reanalysis, WACCAM CCM, and BASCOE CTM models. The information is very useful as in the context of a changing climate, any BDC changes will impact UTLS trace gas budget, which might, in turn, radiatively impact surface climate, therefore, it is important understanding the role of the advection and mixing processes in BDC changes, including the vertical residual advection and horizontal mixing. Although the paper contains some interesting material, which should be published, the manuscript itself could be significantly improved qualitatively in some parts (introduction and results). Some paragraphs and sections are poor, therefore, they need to be revised by enhancing the discussion about the scientific content, the structure of results presentations as well as the wording to improve the quality of the paper. Particularly, the differences between WACCM and reanalyses and their possible physical causes could be significantly emphasized. Appropriate references need to be used at the right places instead and properly discussed when necessary. I recommend major revisions. In the following here are my major points and general concerns:

### **Major points:**

1. The introduction is poorly written, appropriate references are not properly used at some places, and some sentences are vague (not specific).
2. It is important to show the contribution of the remaining terms such as the vertical mixing and horizontal advection in zonal mean as they are not negligible but just small than the vertical advection and horizontal mixing. This can be added as a supplement information.
3. As the calculation of  $w^*$  from CCM in CCMI project leads to a bias due to stratospheric shrinking (Eichinger & Shacha, 2020), this make wonder if the  $w^*$  from WACCM-CCMI calculated consistently with the  $w^*$  from BASCOE?
4. The scientific discussion of the figure 1 and 2 in the two paragraphs (234-239) is not clear and very poor. Differences/similarities in different terms and in different products are just omitted. All terms contributing to N<sub>2</sub>O are not well identified and reported.
5. Why is there some differences in the vertical and horizontal mixing and residual terms in the SH between WACCM and reanalyses?
6. So far, ERAi is the reanalysis, which shows a closer pattern changes in the last decade of trace gases closer to observations, including O<sub>3</sub>, HCl, etc... but it's not shown in figure 3 and 4. A similar panel should be added in the supplement and discussed as well as the horizontal advection and vertical mixing term.

7. The scientific discussion of the figure 3 and 4 related to summer and winter variations of advective and mixing terms is poor and can be improved as well as linked to age spectrum/age of air published articles (Li et al., 2012, Diallo et al, 2012, Ploeger and Birner, 2016).
8. It would be very instructive to reproduce the figure 8 in Randel et al, 1994 which will compare WACCM ensemble mean versus all reanalysis means.
9. The results discussed in “climatological seasonal cycles” section is not clear. It is missing a clear structural organization and not all panels are discussed. Thus, it is very difficult to follow. One suggestion would be to organize the discussion by latitude bins and by term: “In the tropic, ...”, “In the mid-latitudes, ...” and “In the polar region, ...”
10. Is there any physical explanation of the spread in the tropical and mid-latitudinal N<sub>2</sub>O vmr in figure 8? What is the contribution of different QBO representation and modulation of the upwelling to the differences?
11. The results’ discussion in section 3.3 are also poor. Need to be improved.
12. The main issue of the paper is results part is poor. The scientific content of the figures are better discussed in the discussion part than in the main part of the paper. This gives to a reader the feeling that he is reading twice the same article. It would be great to put necessary elements in the main part of the manuscript when commenting the figures. This could be done by moving the information in the Discussion section to where it belongs for each figure in the main text.
13. The differences in the tropics, mid-latitude and high latitude need to be discussed clearly by taking into account the difference in the QBO. Showing a tropical mean cross-section (5S-5N) of N<sub>2</sub>O vmr from reanalysis means versus WACCM ensemble means as time series over the dataset period will be great for discussion and for illustration of the possible differences related to QBO (timing, amplitude, phases, ...). For insight, please see Park et al. 2017 (fig 9 and 12). In addition for the polar region discussion, it would be very instructive too related the discussion to Randel et al, 1994, where a case study of SSW have been illustrated using N<sub>2</sub>O budget.

#### **Minor points:**

1. Page 1, line 1-2, please rephrase the sentence it sounds wrong “*from the well-mixed tropical troposphere to the polar stratosphere*” and “*..., chemistry, ozone distribution and recovery*”
2. Page 2, line 33-34, the BDC is the stratospheric circulation and it is not a tropospheric circulation. Please rephrase this sentence “The stratospheric circulation is mainly characterized by the Brewer Dobson Circulation..... from the troposphere...”
3. Page 2, line 38, please replace “The BDC is generated by Rossby waves propagating” by “The BDC is driven by Rossby wave breaking into ...”
4. Page 2, line 39, please rephrase “This departure”
5. Page 2, line 41-43, note that the residual circulation can be split into 3 branches: transition, shallow and deep branch for more detail see Lin & Fu (2013). Please improve the discussion by including the relevant previous studies: Haynes et al., 1991, Rosenlof and Holton, 1993; Newman and Nash, 2000; and Birner and Bönich (2011). Please add also the term “breaking” after “synoptic-scale” and “Rossby” and replace

“generate/generated” by “drive/driven” in the whole manuscript. The paragraph (line 38-43) is very poor and need to be improve, and also the natural variability modulations, including QBO and ENSO, of the BDC branches, trace gas transport need to be mentioned see Yang et al, 2014; Baldwin et al 2002, Tweedy et al., 2017, and Diallo et al, 2018, 2019.

6. Page 2, line 50, Please rephrase this sentence “Simulations by Chemistry Climate Model (CCM)...” by “Chemistry Climate Model (CCM) simulations...”
7. Page 2, line 54, the references in the sentence “Observations of long-lived chemical tracers (e.g. H<sub>2</sub>O, N<sub>2</sub>O) are often used to derive estimates of the BDC...” is not the appropriate one. Please use the right articles, which examined BDC from H<sub>2</sub>O, N<sub>2</sub>O, like e.g. Hegglin et al 2014; Andrews et al. 2001; Kracher et al. 2016; Schoeberl et al, 2008 and H. K. Roscoe, 2006.
8. Page 2-3, line 55-56, the sentence is not correct because the balloon observation trend in the whole NH but only for the deep branch. Please be specific.
9. Page 3, line 58, please “Stiller et al. 2012” among the early papers using SF<sub>6</sub> satellite observation to estimate decadal BDC trends.
10. Page 3, line 59-60, please cite Diallo et al, 2012 and Monge-Sanz et al 2012 among the early papers using reanalysis and observation to assess BDC changes. Add Ploeger et al., 2019 as well.
11. Page 3, line 59-60, the whole sentence “A number...” seems a bit off here as it is break the continuity from the previous session and mixes again reanalysis, climate model & observations while mainly talking about BDC derive from observations and its limitation.
12. Page 3, line 64-65, CLaMS is a Lagrangian transport model driven with reanalyses not a climate model, therefore, the citation of Ploeger et al 2019 is out of place here. Please move it to line 59-60.
13. Page 3, line 66, this “nitrous oxide (N<sub>2</sub>O)” is already mentioned in page 2, line 53 but online define now.
14. Page 3, line 77, please be specific here by replacing “from several reanalysis datasets.” With “from the Chemical Observations (BASCOE) Chemistry-Transport Model (CTM) driven by several reanalysis datasets (Chabrilat et al., 2018).”
15. Page 3, line 77, remove “Dynamical” and replace by “Reanalysis products”
16. Page 3, line 81, move “Fujiwara et al., 2017; Cameron” after “models”.
17. Page 3, line 86-88, please citations for each reanalysis product (e.g. Dee et al. 2011, Kobayashi et al 2015, Rienecker et al. 2011, Gelaro et al., 2017).
18. Page 4, line 97-99, the description section 3.1, 3.2 and 3.3 could be combine into section 3 to avoid redundant description.
19. Page 4, line 102, “Data and methods”. There is no “s” to “method”.
20. Page 4, line 107-108, please precise what you did “ran” by yourself or “downloaded/use” existing simulations. Rephrase this sentence “We ran one realization of the public version of WACCM (hereafter WACCM4, Marsh et al., 2013), that we downloaded at [https://svn-ccsm-models.cgd.ucar.edu/cesm1/release\\_tags/cesm1\\_2\\_2cesm1\\_2\\_2](https://svn-ccsm-models.cgd.ucar.edu/cesm1/release_tags/cesm1_2_2cesm1_2_2).”
21. Page 4, line 104, replace “transport (see Sect. 4).” by “transport (see Sect. 4 for detailed analysis)”. The same remark for “dataset (see Sec. 2.3)”.
22. Page 4, line 119, the “... (Lin, 2004).” is not correctly reported in the reference.

23. Page 5, line 124-126, please replace the existence by these ones “In this study, the considered WACCM versions are not able to internally generate the Quasi-Biennial Oscillation (QBO, see e.g. Baldwin et al., 2001). Thus, the QBO is forcing (nudged) by a relaxation of stratospheric winds to observations in the Tropics (Matthes et al., 2010).”
24. Page 5, line 130, add coma after “In addition”
25. Page 5, line 137-138, please rephrase this sentence “The transport module requires on input only the surface pressure and horizontal wind fields from reanalyses, as it relies on mass continuity to derive vertical mass fluxes”
26. Page 5, line 135, please add a comma before “which”
27. Page 5, line 139-141, please add a comma after “but” and “In this way”.
28. Page 5, line 147, please rephrase this sentence “For this work the BASCOE CTM provided daily mean outputs over the 2005-2014 period *as for the WACCM experiment.*”
29. Page 5, line 150, for analogy to the tow previous model description, this part “The TEM diagnosis is also applied to N2O” is out of place here. First describe the BRAMS2 and then...
30. Page 6, line 164, please remove this “Livesey, in preparation”
31. Page 6, line 170, please the sentence after “temperatures,” and start a new one.
32. Page 6 line 180, please add a comma after “Hence”
33. Page 7, line 195, replace “hence retaining” by “while conserving”
34. Page 7, line 201, please a comma before “which”
35. Page 7, line 202, add a comma after “Furthermore in WACCM”
36. Page 7, line 206, replace “timestep” by “time step”
37. Page 7, line 205-207, this sentence can combine to one concise sentence avoid the use of “This”. Please rephrase “Finally, the daily mean fields are interpolated from their native hybrid-sigma levels to constant pressure levels prior to the TEM analysis. This could lead to numerical errors in the lower stratosphere.”
38. Page 7, line 207, please add a comma after “For WACCM-CCMI”
39. Page 7, line 211, the term “realistic” does not fit well with second part of the sentence “but”. What lead to the different representation of large-scale transport is not the fact that the temperature and winds are realistic but because the reanalyses have some differences in wind and temperature. Please see Fig. 5 in Tao et al 2019. You can rephrase the existing sentence as following “The four dynamical reanalyses used in this study provide comparable (consistent) temperature and winds in the stratosphere, but can also lead to a different representation of large-scale transport (e.g. Chabrilat et al., 2018) due to the biases in the temperature and wind fields (Kawatani et al., 2016, Tao et al., 2019). ”
40. Page 7, line 213, add a comma after “In the rest of the paper”
41. Page 7, line 214, replace “BASCOE reanalysis BRAM2” by either “BASCOE reanalysis” or “BRAM2 product”
42. Page 8, line 217, add a comma after “In Figs. 1 and 2”
43. Page 8, line 219, replace “the strongest” by “stronger ...”. In addition DJF & JJA can be term as boreal winter and summer season.

44. Page 8, line 223, regarding the Figure 1, please replace “time der” by “ $X_t$ ” or “tendency” and redo the figure that the  $My$  (green) appear properly in all panels. The fact tendency, residual & horizontal bold line are all in black make different components hard to distinguish. Please fix it.
45. Page 8, line 225, please rephrase “In the northern tropics the  $N_2O$  decrease due to horizontal mixing is clearly”. Also the tendency term of WACCM-CMM is near zero in the NH. I don’t see any directional sign therefore the sentence does not match what the panel is showing. Maybe for WACCM panel you can change the vertical scale and note that in the figure caption that the vertical scale of WACCM is different from the reanalyses.
46. Page 8, line 225-226, the interpretation in this sentence is wrong “In the northern tropics ... sufficient to do so.” Overall the  $Ay$  term is consistent between WACCM and the reanalyses at all latitudes.
47. Page 8, line 226-229, please rephrase this sentence “At the higher latitudes the main terms contributing to the  $N_2O$  TEM budget are the positive horizontal mixing term in the  $N_2O$  increase, and the negative vertical advection and vertical mixing terms for the  $N_2O$  decrease in all the datasets, with negligible contributions from the other terms.” It’s not clear and poor.
48. Page 8, line 230-231, what about the except of MERRA where the horizontal advection is comparable to Production-lost term as well as the JRA “ $Ay$ ” increase in the NH. Here also the discussion is poor.
49. Page 8, line 232, this statement is not true for the reanalysis “a general balance between the  $My$  and  $Ay$ ” because for some reanalysis the residual and P-L term are as large as the “ $My$ ”.
50. Page 8, line 233-234, the term “ $Ay$ ” also contribute in the mid lat.
51. Page 8, line 235, please replace “is affected mostly” by “is mostly affected...”
52. Page 8, line 235-239, Why their differences in the vertical and horizontal mixing and residual terms in the SH between WACCM and reanalyses is not discussed here?
53. Figure 3 and 4, it would be good to add the arrows indicating the residual mean circulation  $v^*$  and  $w^*$  as well as the zero zonal mean wind but remove the full zonal mean wind fields.
54. Page 9, line 245, add a comma after “CCMI”
55. Page 9 line 250, add a comma after “During the DJF season” and before “but”
56. Page 9 Why the colorbars in figures 3 and 4 have a different scales?
57. Page 9 Why the differences between summer and winter term are not discussed?
58. Page 9, line 259, add a comma after “In the JJA season”
59. Page 9, line 259-267, why the large “ $My$ ” term from BRAM2 is not mentioned?
60. Page 9, line 262, replace “very positive values” by “large positive values”
61. Page 9-10, regarding the figures 5 and 6, over the whole manuscript you have always discussed NH and then SH. Why then starting with the SH when it comes to figure 5 and 6? It would be good to keep a fix structure.
62. Page 10, line 270-271, the affirmation regarding “ $My$ ” and “ $Az$ ” terms showing maxima at 15hPa is wrong because the “ $Az$ ” terms maximum is around 5 hPa for WACCM-JRA55 and a bit high for the others reanalyses in both seasons DJF & JJA figures. Your previous argument was that it’s level of better assimilation of meteorological observations according to Manney et al. 2003. Please correct that.
63. Page 9, line 274, add a comma after “For WACCM-CCMI”

64. Page 9, line 275-281, this information should move to the caption. In addition, BRAM2 is a BASCOE reanalysis, while the other reanalysis products (ERAi, JRA55, MERRA) use well-established assimilation system constrained with observations. I don't see why BRAM2 is consider here as the "truth"?
65. Page 9, line 282, replace "We first investigate" by "First, we investigate..."
66. Pages 9-10, line 283-285, Is there any possible physical explanation of ERAi underestimation in tropics? Is there any link to the upwelling or extent of the tropical pipe? Or just a different location of the maximum for ERAi compare to JRA-WACMM?
67. Page 9-10, line 283-287, the discussion is not clear and very hard to follow. Why "the subtropics 40-60" is just not mentioned in the N2O vmr? All panels in the figure have to be discussed, if not please do not show them. It will be clearer and easier to follow if the discussion is done by latitude band e.g. "In the tropic, ...", "In the mid-latitudes, ..." and "In the polar region, ..."
68. Page 10, line 289, replace "We then investigate" by "Second, we investigate..."
69. Page 10, line 322-323, the sentence is not clear and can be split into 2 sentences and formulated clearly.
70. Page 10, line 326, add a comma after "Finally". Same after "In the Tropics from November to April (Fig. 6(g))", same after "In the middle latitudes (Fig. 6(h))", same after "In the arctic region (Fig. 6(i))"
71. In this section 3.2, differences are reported but there is no physically explained attempt.
72. Page 11, line replace "After reporting on the climatological annual cycles, it is desirable to estimate their inter-annual variability. To this end," by " To analyse the inter-annual variability of the annual cycle, we..."
73. Redo panel f) and i) of figure 6 in order to get the quantities shown properly. It is not necessary to keep the same y-axis scaling identical for "Az" and "My" terms.
74. Page 11, line 341, replace "We first consider" by "First, we consider"
75. Page 11, line 342-343, in the  $[0^\circ, 20^\circ]$  at 15hPa, BRAMS N2O mixing ratio is more closer to the reanalyses at the first half of the year.
76. Page 11, line 344, add a comma after "In the northern mid-latitudes (Fig.7(d))"
77. Redo panel a) and b) of figure 8.
78. Page 11, line 345-346, why there is no attempt of physical explanation or to link of the spread to differences in upwelling or tropical pipe in the dataset?
79. Page 11, line 347, add a comma after "In the middle latitudes (Figs. 7(e) and 7(h))"
80. Page 11, line 348, add a comma after "In the antarctic region (Fig. 8(c))"
81. Page 11, line 348-350, what is the physical explanation of the hemispheric differences in the Az and My? The strength of the polar? Sudden stratospheric warming?
82. Page 11, line 349, replace "the vortex break-up," by "the breaking vortex period"
83. Page 12, line 350-351, replace "We now move to the variability of the horizontal mixing term My starting from the Tropics (Figs. 7(j) and 7(k)). In the southern tropics (Fig. 7(j))" by "Regarding the variability of the horizontal mixing in the southern tropics (Figs. 7(j, k)), My term shows... In the northern tropics (Fig. 7(k)), My....."
84. Page 12, line 355, add a comma after "In the mid-latitudes"
85. Page 12, line 338, add a comma after "In the antarctic region (Fig. 8(e))".

86. Page 12, line 360 add a comma after “The Arctic (Fig. 8(f)) ”
87. Page 12, line 360 add a comma after “Among the reanalyses”
88. Page 12, line 370, please don’t oversell the agreement. Replace “excellent agreement” by “fairly good” and complete the sentence “but some differences also occur at ...”. In addition this part of the sentence “while the CTM delivers overall smaller variabilities.” is not true as the reanalysis also show spread in the tropics.
89. Page 12, line 376, add a comma after “ Above the Arctic in the middle stratosphere”
90. Page 13, line 408, add a comma after “During the SH spring”

#### References:

- 1) Lin, P. and Fu, Q.: Changes in various branches of the Brewer–Dobson circulation from an ensemble of chemistry climate models, *J. Geophys. Res.-Atmos.*, 118, 73–84, <https://doi.org/10.1029/2012JD018813>, 2013
- 2) Haynes, P. H., McIntyre, M. E., Shepherd, T. G., Marks, C. J., and Shine, K. P.: On the “Downward Control” of Extratropical Diabatic Circulations by Eddy-Induced Mean Zonal Forces, *J. Atmos. Sci.*, 48, 651–678, 1991
- 3) Bönisch, H. B., Engel, A., Birner, T., Hoor, P., Tarasick, D. W., and Ray, E. A.: On the structural changes in the Brewer-Dobson circulation after 2000, *Atmos. Chem. Phys.*, 11, 3937–3948, <https://doi.org/10.5194/acp-11-3937-2011>, 2011.
- 4) Newman, P. A. and Nash, E. R.: Quantifying the wave driving of the stratosphere, *J. Geophys. Res.-Atmos.*, 105, 12485–12497, <https://doi.org/10.1029/1999JD901191>, 2000.
- 5) Rosenlof, K. and Holton, J.: Estimates of the stratospheric residual circulation using the downward control principle, *J. Geophys. Res.*, 98, 10465–10479, 1993.
- 6) Yang, H., Chen, G., and Domeisen, D. I. V.: Sensitivities of the Lower Stratospheric Transport and Mixing to Tropical SST Heating, *J. Atmos. Sci.*, 71, 2674–2694, <https://doi.org/10.1175/JAS-D-13-0276.1>, 2014.
- 7) Diallo, M., Konopka, P., Santee, M. L., Müller, R., Tao, M., Walker, K. A., Legras, B., Riese, M., Ern, M., and Ploeger, F.: Structural changes in the shallow and transition branch of the Brewer–Dobson circulation induced by El Niño, *Atmos. Chem. Phys.*, 19, 425–446, <https://doi.org/10.5194/acp-19-425-2019>, 2019.
- 8) Diallo, M., Riese, M., Birner, T., Konopka, P., Müller, R., Hegglin, M. I., Santee, M. L., Baldwin, M., Legras, B., and Ploeger, F.: Response of stratospheric water vapor and ozone to the unusual timing of El Niño and the QBO disruption in 2015–2016, *Atmos. Chem. Phys.*, 18, 13055–13073, <https://doi.org/10.5194/acp-18-13055-2018>, 2018.

- 9) Andrews, A. E., Boering, K. A., Daube, B. C., Wofsy, S. C., Loewenstein, M., Jost, H., Podolske, J. R., Webster, C. R., Her-man, R. L., Scott, D. C., Flesch, G. J., Moyer, E. J., Elkins, J. W., Dutton, G. S., Hurst, D. F., Moore, F. L., Ray, E. A., Ro-mashkin, P. A., and Strahan, S. E.: Mean age of stratospheric air derived from in situ observations of CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O, *J. Geophys. Res.*, 106, 32295–32314, doi: 10.1029/2001JD000465,2001a
- 10) Kracher, D., C. H. Reick, E. Manzini, M. G. Schultz, and O. Stein (2016), Climate change reduces warming potential of nitrous oxide by an enhanced Brewer-Dobson circulation, *Geophys. Res. Lett.*, 43, 5851–5859, doi: 10.1002/ 2016GL068390
- 11) Laura E. Revell et al. *GEOPHYSICAL RESEARCH LETTERS*, VOL. 39, L15806, doi:10.1029/2012GL052143, 2012
- 12) Roscoe, 2006: The Brewer–Dobson circulation in the stratosphere and mesosphere – Is there a trend? <https://doi.org/10.1016/j.asr.2006.02.078>
- 13) Stiller, G. P., von Clarmann, T., Haenel, F., Funke, B., Glatthor, N., Grabowski, U., Kellmann, S., Kiefer, M., Linden, A., Lossow, S., and López-Puertas, M.: Observed temporal evolution of global mean age of stratospheric air for the 2002 to 2010 period, *Atmos. Chem. Phys.*, 12, 3311–3331, <https://doi.org/10.5194/acp-12-3311-2012>, 2012.
- 14) Diallo, M., Legras, B., and Chédin, A.: Age of stratospheric air in the ERA-Interim, *Atmos. Chem. Phys.*, 12, 12133–12154, <https://doi.org/10.5194/acp-12-12133-2012>, 2012.
- 15) Monge-Sanz et al. (2012) , *QJ*, doi: 10.1002/qj.1996
- 16) Kawatani, Y., Hamilton, K., Miyazaki, K., Fujiwara, M., and Anstey, J. A.: Representation of the tropical stratospheric zonal wind in global atmospheric reanalyses, *Atmos. Chem. Phys.*, 16, 6681–6699, <https://doi.org/10.5194/acp-16-6681-2016>, 2016.
- 17) Tao, M., Konopka, P., Ploeger, F., Yan, X., Wright, J. S., Diallo, M., Fueglistaler, S., and Riese, M.: Multitimescale variations in modeled stratospheric water vapor derived from three modern reanalysis products, *Atmos. Chem. Phys.*, 19, 6509–6534, <https://doi.org/10.5194/acp-19-6509-2019>, 2019.
- 18) Li, F., Waugh, D. W., Douglass, A. R., Newman, P. A., Pawson, S., Stolarski, R. S., Strahan, S. E., and Nielsen, J. E.: Seasonal variations of stratospheric age spectra in the Goddard Earth Observing System Chemistry Climate Model (GEOSCCM), *J. Geophys. Res.*, 117, D05 134,doi:10.1029/2011JD016877, 2012.
- 19) Ploeger, F. and Birner, T.: Seasonal and inter-annual variability of lower stratospheric age of air spectra, *Atmos. Chem. Phys.*, 16, 10195–10213, <https://doi.org/10.5194/acp-16-10195-2016>, 2016.
- 20) Park, M., Randel, W. J., Kinnison, D. E., Bourassa, A. E., Degenstein, D. A., Roth, C. Z., ...Santee, M. L. (2017). Variability of stratospheric reactive nitrogen and ozone related to the QBO. *Journal of Geophysical Research: Atmospheres*, 122, 10, 103–10, 118, <https://doi.org/10.1002/2017JD027061>



