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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatological impact of the Brewer–Dobson Circulation on the N_2O budget in WACCM, a chemical reanalysis and a CTM driven by four dynamical reanalyses" *by* Daniele Minganti et al.

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Response to Reviewer#1 for: Climatological impact of the Brewer-Dobson Circulation on the N_2O budget in WACCM, a chemical reanalysis and a CTM driven by four dynamical reanalyses

Minganti et al., ACPD, 2020

We thank the reviewer for his in-depth review and useful comments. Following the reviewer's suggestion, we changed the structure of the paper and added supplemental figures. In order to better interpret our results, we inserted new figures showing the Eliassen-Palm Flux Divergence across the datasets. We also strove to improve the text by improving the structure and clarity of the Introduction and scientific discussions and by taking into account the additional references suggested by the reviewer. In our replies below the italic type is used for the reviewer's comments, the plain text for authors' answers and the bold type for the revised text in the manuscript.

Replies to general comments.

- *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.*

  The Introduction was throughly revised and changed. We added new references (see list of references below) and enhanced the discussion by adding the relevant scientific content and removing (or reducing) when necessary. Regarding the results Section, we merged together the Section 3 with Section 4, and the manuscript was restructured in the following layout:

Section 3. Latitude pressure cross sections

Section 4. Climatological seasonal cycles

Section 4.1 Polar regions

Section 4.2 Middle latitudes

Section 4.3 Tropics

Section 5. Interannual variability of the seasonal cycles

Section 6. Summary and Conclusions

This new structure allowed to remove some purely descriptive parts in former Section 3, and add scientific content in former Section 4, i.e. the comparisons with relevant previous studies and physical interpretations of the differences/similarities in the different datasets. The layout of Figs. 5 and 6 changed as well: we separated them by latitude bands (one figure for the polar regions, one figure for the surf zones and one for the tropics) in order to better follow the flow in the Section 4 of the revised manuscript and its subsections. Fig. 9 was also modified according to this new structure, and it is described in the response to the comment below.

- *Particularly, the differences between WACCM and reanalyses and their possible physical causes could be significantly emphasized.*

  The differences (or similarities) between WACCM and the reanalyses are better addressed now, as the pertinent studies comparing WACCM and the reanalyses or Aura MLS observations are considered and discussed. Furthermore, we expanded the Fig. 9 to include the northern and southern middle latitudes and polar regions, while the Tropics were moved in the supplement (Fig. S7). We separated it by latitude bands, i.e. one figure for the polar regions and one figure for the surf zones. We also added an additional row in each figure, showing the divergence of the Eliassen-Palm flux, as a measure of the forcing from resolved waves for all the considered datasets (BRAM2 is not shown because it uses the dynamical fields from ERAI). WACCM shows an underestimation of the divergence of the Eliassen-Palm flux, that allowed to enhance the discussion about the differences in the mid-stratospheric A_z and M_y. The Sect.3.3 was also improved in terms of scientific discussion thanks to the merging with the pertinent parts of Sect. 4, that were expanded and improved accordingly.

- *Appropriate references need to be used at the right places instead and properly discussed when necessary.*

  Additional references were added through the revised manuscript. In the Introduction, we added Lin and Fu (2013); Fueglistaler et al. (2009); Birner and Bönisch (2011); Haynes et al. (1991); Rosenlof and Holton (1993); Newman and Nash (2000); Bönisch et al. (2011) for the description of the BDC. For the natural variability of the BDC we added Riese et al. (2012); Yang et al. (2014); Diallo et al. (2019, 2018); Salby and Callaghan (2005). In the part about trend studies of the BDC we added Fritsch et al. (2019). In the reanalyses and CTM description we included Gerber et al. (2010); Rao et al. (2015); Long et al. (2017); Waugh and Hall (2002); Chipperfield (2006); Monge-Sanz et al. (2012); Ménard et al. (2020). For the description of the chemical reanalysis BRAM2 we added Errera et al. (2008); Lahoz and Errera (2010). In Sect. 2 we included the suggested references In Sect. 3 we included Li et al. (2012) for the discussion of the seasonality of the BDC, we added also Roscoe et al. (2012) for the discussion of the differences in M_y above the Antarctic, and Ploeger and Birner (2016); Konopka et al. (2010) for the discussion of the lower branch of the BDC. In Sect. 4 we added Konopka et al. (2015); Gerber (2012) in connection to the divergence of the Eliassen-Palm flux, and Sato and Hirano (2019) for the discussion about A_z in the middle latitudes. In Sect. 5 we added the suggested reference Park et al. (2017) to discuss the inter-annual variability of N_2O.

Replies to Major points.

1. *The Introduction is poorly written, appropriate references are not properly used at some places, and some sentences are vague (not specific).*

   The Introduction was deeply revised according to the comments of both reviewers. BDC. The description of the BDC was improved by describing its different branches, as well as how the wave breaking that leads to the BDC is quantified. The natural variability of the BDC is also discussed. The trend part was de-emphasized, as the current manuscript does not look at BDC trends (which will be the topic of a follow-up study). The Introduction now states that it is important to study the climatological behaviour before trend studies.

   CCM and WACCM. The sentences/paragraphs about CCMs and WACCM were put together into one paragraph.

   Dynamical reanalyses, CTM and BASCOE. The former Introduction was too vague on these topics, therefore they were re-structured and clarified. The Introduction of dynamical reanalyses has been expanded to mention S-RIP. CTMs driven by reanalyses are described and related to BDC studies of Age of Air. Finally, the BASCOE CTM description was slightly expanded.

   All the sentences about Chemical reanalyses and BRAM2 were also merged into one paragraph that starts with a general description of the added value of a chemical reanalysis, continues with the use of chemical reanalyses in TEM studies, and ends with a short description of BRAM2. We conclude the Introduction by summarizing the approach of the paper and providing its structure.

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.*

The contributions of A_y, M_z and $P-L$ for DJF and JJA are shown in the Supplement (Figs. S1 and S2) and appropriately mentioned in Section 3.

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?*

   The WACCM output we used includes only the basic meteorological variables, i.e. surface pressure, temperature and horizontal and vertical winds fields. w^* is re-calculated consistently across all datasets through equation 3b using the daily 3-D output of meridional and vertical wind velocity and temperature from WACCM and the dynamical reanalyses. These calculations are performed as recommended by the CCMI project (Chrysanthou et al., 2019).

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 N2O are not well identified and reported.*

   Figures 1 and 2 are not meant for describing the differences/similarities between the datasets, rather for showing how the N_2O TEM budget and how the different terms balance each other depending on the latitude. The scientific discussion of the differences between datasets, and their possible physical causes, belongs to the following Sections. Yet, this was not stated in the manuscript, and understandably raised some confusion. We now state explicitly the purpose of these figures have shortened their description to focus on the physical meanings of the budget terms as follows:

   Figs. 1 and 2 show the N_2O TEM budget terms at 15 hPa for all the datasets for the boreal winter (December-January-February, DJF mean) and summer (June-July-August, JJA mean) respectively. The 15 hPa level (around 30 km altitude) was chosen because large differences can be found

between WACCM-CCMI, BRAM2, and the CTM runs at this level, and because the dynamical reanalyses are not constrained as well by meteorological observations at higher levels (Manney et al., 2003). Figs. 1 and 2 aim to show how the dynamical and chemical terms of the budget balance each other to recover the tendency $\bar{\chi}_t$ at different latitudes. The discussion about the differences between the datasets, and their possible physical causes, are addressed in the next Sections.

The vertical advection term A_z shows how the upwelling contributes to increasing the N_2O abundances in the tropics and summertime mid-latitudes, and how polar downwelling contributes to decreasing the N_2O abundances in the winter hemisphere. The horizontal transport out of the tropics due to eddies, as represented by M_y, reduces the N_2O abundance in the tropical latitudes of the wintertime hemisphere, and increases the N_2O mixing ratio at high latitudes in the winter hemisphere. The other terms of the TEM budget are weaker than A_z and M_y: the meridional advection term A_y tends to increase the N_2O abundance in the winter subtropics and extra-tropics, while the vertical transport term due to eddy mixing, M_z decreases it over northern polar latitudes and the chemistry term $P-L$ shows that N_2O destruction by photodissociation and $O(1D)$ oxidation contributes to the budget in the tropics and also in the summertime hemisphere. All budget terms are weaker in the summer hemisphere than the winter hemisphere. Over the southern polar winter latitudes, the reanalyses deliver negative M_y that are balanced by large positive residuals, which implies a less robust TEM balance (Fig. 2). This is not the case with WACCM, where M_y tends to increase the N_2O abundance in the polar vortex. Such differences between the datasets are highlighted and discussed in the next sections.

5. *Why is there some differences in the vertical and horizontal mixing and residual terms in the SH between WACCM and reanalyses?*

The differences in M_y between WACCM and the reanalyses above the winter South Pole are discussed in Sects. 3 (fifth and sixth paragraphs) of the revised manuscript:

In the austral winter, over the Antarctic Polar cap and below 30 hPa, M_y agrees remarkably well in all datasets (Fig. 4). Closer to the vortex edge and above 30 hPa, the wintertime decrease of N_2O is mainly due to downwelling in WACCM-CCMI, while the reanalyses, especially BRAM2, show that the horizontal mixing plays a major role (Fig. 4). The impact of horizontal mixing on N_2O inside the wintertime polar vortex is not negligible (e.g. de la CÃÂąmara et al., 2013; Abalos et al., 2016a), as Rossby waves breaking occurs there as well as in the surf zone. In constrast with the reanalyses, in WACCM-CCMI the M_y contribution is close to zero in the Antarctic vortex and maximum along the vortex edge (Fig. 4). This disagreement can be related to differences in the zonal wind: it is overestimated in WACCM above 30 km in subpolar latitudes compared to MERRA (Garcia et al., 2017) and the polar jet is not tilted equatorward as in the reanalyses (see black thin lines in Fig. 4, and Fig. 3 of Roscoe et al., 2012). Yet, the differences in M_y and A_z above the Antarctic in winter should be put into perspective with the relatively large residual terms that points to incomplete TEM budgets in the reanalyses (Fig. 4 and S4 right columns). Near the antarctic polar vortex, the assumptions of the TEM analysis (such as small amplitude waves) are less valid leading to larger errors in the evaluation of the mean transport and eddy ïÂňÂĆuxes (Miyazaki and Iwasaki, 2005). Since the relative importance of the residual is considerable above the Antarctic in the reanalyses (Fig. 4), it is necessary to better understand its physical meaning. DietmÃ$\frac{1}{4}$ller et al. (2017) applied the TEM continuity equation to the Age of Air (AoA) in CCM simulations. Computing the "resolved aging by mixing" (i.e. the AoA counterpart of M_y +M_z) as the time integral of the local mixing tendency along the residual circulation trajectories, and the

"total aging by mixing" as the difference between the mean AoA and the residual circulation transit time, they defined the "aging by mixing on unresolved scales" (i.e. by diffusion) as the difference between the latter and the former. This "aging by diffusion", which can be related by construction to our residual term, arises around 60âÂŮÂę S from the gradients due to the polar vortex edge. Even though we use a real tracer (N_2O), we find a qualitative agreement with this analysis based on AoA: our residual term is larger in regions characterized by strong gradients such as the antarctic vortex edge, and larger with dynamics constrained to a reanalysis than with a free-running CCM (see EMAC results in Fig. 1d by DietmÃ$\frac{1}{4}$ller et al., 2017). We thus interpret the residual as the sum of mixing at unresolved scales and numerical errors (Abalos et al., 2017).

They are also discussed in Sect. 4.1 of the revised manuscript :

We now turn to the contribution from M_y . In the antarctic region, M_y is very different among the datasets during winter: in BRAM2 it contributes to the N_2O decrease during fall and winter, with the strongest contribution in July, but with the CTM simulations this contribution is two times weaker, while in WACCM-CCMI the horizontal mixing has almost no effect on N_2O (Fig. 6(m)). As already mentioned, the TEM analysis suffers from large residuals in the wintertime antarctic region. Yet we note that the disagreement between WACCM-CCMI and BRAM2 is significant, because in fall and winter the envelope of WACCM-CCMI realizations falls completely outside of the possible BRAM2 values when accounting for the residual. During the austral spring, the vortex breakup leads to an increased wave activity reaching the Antarctic (Randel and Newman, 1998), and mid-stratospheric M_y is in better agreement among all datasets compared to austral winter. Note that WACCM-CCMI exhibits large internal variability in this season (Fig. 6(m)).

and briefly mentioned in Sect. 2.4:

The BASCOE datasets have a coarser horizontal resolution than their input reanalyses (especially BRAM2; see Table 1). This affects the accuracy of the vertical and horizontal derivatives, with possible implications for the residual.

Again, Figs. 1 and 2 are not meant for the discussion of differences between datasets (this is left to Sects. 3 and 4), but only for showing the TEM budget and pave the way for the following discussion.

6. *So far, ERAi is the reanalysis, which shows a closer pattern changes in the last decade of trace gases closer to observations, including O3, 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.*

   The full N_2O TEM budget obtained with ERAI and MERRA, for DJF and JJA, are now shown in Figs. S3 and S4 of the supplement.

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).*

   The summer and winter variations are now addressed through the seasonality of the deep branch of the BDC on the TEM budget (first paragraph of the revised section 3):

   Large differences arise in the dynamical terms of the budget between summer and winter for both hemispheres in the extratropics. The strong seasonality of the deep branch of the BDC and of the transport barriers are the causes of these differences, as also shown for the seasonal variations of the Age of Air spectum (Li et al., 2012).

and through the differences between the shallow and deep branches of the BDC, which are discussed the the third paragraph of revised section 3:.

In the lower stratosphere, A_z shows the contribution of the residual advection by the shallow branch of the BDC to the N_2O abundances in the winter and summer hemispheres. The two-cell structure, consisting in upwelling of N_2O in the subtropics and downwelling in the extratropics, consistently agrees across all datasets.

... and in a new paragraph at the end of section 3:

In the summertime lower stratosphere, we note a stronger contribution of M_y to the N_2O abundances above the subtropical jets in both hemispheres and for all datasets compared to higher levels in summer (Figs. 3 and 4 middle columns). This behavior is consistent with calculations of the effective diffusivity and age spectra (Haynes and Shuckburgh, 2000; Ploeger and Birner, 2016). It is due to transient Rossby waves that cannot travel further up into the stratosphere due to the presence of critical lines, i.e. where the phase velocity of the wave matches the background wind velocity, generally leading to wave breaking (Abalos et al., 2016b). In particular, above the northern tropics during the boreal summer (Figs. 4, S2 and S4), the horizontal mixing is primarily associated with the Asian monsoon anticyclone, and causes a decrease in N_2O (Konopka et al., 2010; Tweedy et al., 2017). In the lower stratosphere, the contributions from M_y combine with that from A_z in the total impact of the shallow branch of the BDC on N_2O all year round (Diallo et al., 2012).

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.*

We reproduced it for DJF for the WACCM ensemble and the reanalysis ensemble mean, and they are shown in the Supplement (Fig. S5 and Fig. S6 respectively). This is mentioned in the second paragraph of revised section 3:

We also reproduced the results of Randel et al. (1994, Fig. 8) for the WACCM-CCMI multi-model mean and the reanalysis mean in DJF (Figs S5 and S6 respectively). The WACCM-CCMI and the reanalysis means agree with the Community Climate Model version 2 of the early 1990's with regard to the general pattern of the TEM terms, but both deliver stronger contributions, especially the reanalyses mean.

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, ..."*

   We agree with the comment from the reviewer. As stated before, the Section "climatological seasonal cycles" was merged with the Discussion section and divided in three subsections: Polar regions, middle latitudes and Tropics. This allows the structured discussion by latitude bands that the reviewer suggested.

10. *Is there any physical explanation of the spread in the tropical and mid-latitudinal N2O vmr in figure 8? What is the contribution of different QBO representation and modulation of the upwelling to the differences?*

    Regarding the tropical regions, the differences between the datasets are discussed in more detail, and in the revised section 5 we now illustrate the contribution of the QBO on WACCM and BRAM2as follows:

    In the Tropics, the inter-annual variability of the N_2O mixing ratio in both hemispheres depends considerably on the dataset (Figs. 10(b) and (c)). WACCM-CCMI and the BASCOE reanalysis of Aura MLS BRAM2 show very similar variabilities, especially in the southern Tropics. Similar results are found by Park et al. (2017), who showed a good agreement between the

WACCM model and MLS observations in the middle stratosphere in terms of the inter-annual variability of N_2O due to the QBO (the major source of variability in the tropical stratosphere, Baldwin et al., 2001). Among the CTM simulations, ERAI succeeds to deliver $\sigma(\bar{X})$ as large as BRAM2 and WACCM-CCMI in the southern tropics, but not in the northern tropics.

As stated in the last paragraph of the conclusions, a detailed study of the impact of the QBO on N_2O or the TEM quantities does not belong to this paper, but to a follow-up study that will investigate inter-annual changes.

11. *The results' discussion in section 3.3 are also poor. Need to be improved.*

    The Sect. 3.3 was merged with the relevant parts of Sect. 4, to become Sect. 5 in the revised manuscript. The text is less descriptive and the scientific discussion is improved, using existing and new references.

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 session to where it belongs for each figure in the main text.*

    Indeed, the reviewer is right. The results part (Sect. 3) was merged with the Discussion (Sect. 4), and new subsections were created (see above). This allows to enhance the scientific discussion and cut the descriptive parts that were not necessary.

13. *The differences in the tropics, mid-latitude and high latitude need to be discuss clear by taking into account the difference in the QBO. Showing a tropical mean cross-section (5S-5N) of N2O 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 N2O budget.

As announced in the title, the scope of the paper is limited to climatologies. Time series will be investigated in a follow-up study about inter-annual changes. Thus, we decided not to show the suggested time series plot of the reanalysis mean vs WACCM mean. A reference to the work from Park et al. (2017) was added to the third paragraph of revised Section 5:

WACCM-CCMI and the BASCOE reanalysis of Aura MLS show very similar variabilities, especially in the southern Tropics. Since the QBO is the major source of variability in the tropical stratosphere (Baldwin et al., 2001), this confirms an earlier comparison that showed a good agreement between the WACCM model and MLS observations in the middle stratosphere in terms of the inter-annual variability of N_2O due to the QBO (Park et al., 2017)

as well as a connection to the SSW case study in Randel et al., 1994 for the Arctic (second paragraph of Section 5):

Above the Arctic, M_y and A_z are most variable during winter, reflecting the frequent disruptions of the northern polar vortex by sudden stratospheric warmings (SSWs, Butler et al., 2017). A case study of the effect of a SSW on the N_2O TEM budget showed that A_z and M_y contribute more to this budget during the SSW event than in the corresponding seasonal mean. Thus, the large wintertime variability of A_z and M_y is explained by the occurrence of seven major SSWs detected in the reanalyses for the 2005-2014 period (Butler et al., 2017).

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Discussion paper

Replies to 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"*

   The sentence was rephrased:

   The Brewer-Dobson Circulation (BDC) is a stratospheric circulation characterized by upwelling of tropospheric air in the Tropics, poleward flow in the stratosphere, and downwelling at mid and high latitudes, with important implications for chemical tracers distribution, stratospheric heat and momentum budgets and mass exchange with the troposphere.

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..."*

   The sentence was rephrased:

   The Brewer-Dobson Circulation (BDC, Dobson et al., 1929; Brewer, 1949; Dobson, 1956) in the stratosphere is characterized by upwelling of tropospheric air to the stratosphere in the Tropics, followed by poleward transport in the stratosphere and extratropical downwelling.

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 ..."*

   Done.

4. *Page 2, line 39, please rephrase "This departure"*

   The part is rephrased as follows:

...away from its radiative equilibrium. This is balanced by a meridional...

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 BÃÂűnich (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.*

   The discussion has been improved and the suggested references added as follows:

   The BDC is driven by tropospheric waves breaking into the stratosphere (Charney and Drazin 1961), which transfer angular momentum and force the stratosphere away from its radiative equilibrium. This is balanced by a meridional (poleward) displacement of air masses, which implies tropical upwelling and extra-tropical downwelling (Holton, 2004). The residual circulation can be further separated in three branches: the transition, the shallow and the deep branch (Lin and Fu, 2013). The transition branch encompasses the upper part of the transition layer between the troposphere and the stratosphere (the tropical tropopause layer, Fueglistaler et al., 2009). The shallow branch is an all year-round lower stratospheric two-cell system driven by breaking of synoptic-scale waves, and the deep branch is driven by Rossby and gravity waves breaking in the middle and high parts of the stratosphere during winter (Plumb, 2002; Birner and Bonisch, 2011). The contributions of different wave types to the driving of the BDC branches has been quantified using the downward control principle, which states

that the poleward mass flux across an isentropic surface is controlled by the Rossby or gravity waves breaking above that level (Haynes et al., 1991; Rosenlof and Holton, 1993), and using eddy heat flux calculations as an estimate of the wave activity from the troposphere (e.g., Newman and Nash, 2000).

6. *Page 2, line 50, Please rephrase this sentence "Simulations by Chemistry Climate Model (CCM)..." by "Chemistry Climate Model (CCM) simulations..."*

   Done.

7. *Page 2, line 54, the references in the sentence "Observations of long-lived chemical tracers (e.g. H2O, N2O) are often used to derive estimates of the BDC..." is not the appropriate one. Please use the right articles, which examined BDC from H2O, N2O, like e.g. Hegglin et al 2014; Andrews et al. 2001; Kracher et al. 2016; Schoeberl et al, 2008 and H. K. Roscoe, 2006.*

   As stated in the reply to major point 1, the part of the Introduction dealing with long-term trends was de-emphasized because this manuscript is about the climatology of the BDC, not its trends. In the revised manuscript, studies of BDC trends are introduced with one paragraph citing a few model papers and some observational papers including some of those suggested here by the reviewer.

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.*

   The sentence was correctedas follows:

   ... but balloon-borne observations of SF_6 and CO_2 in the Northern Hemisphere (NH) middle latitudes show a non-significant trend of the deep branch of the BDC in the past decades (Engel et al., 2009, 2017).

9. *Page 3, line 58, please "Stiller et al. 2012" among the early papers using SF6 satellite observation to estimate decadal BDC trends.*

The text was changed and the reference added.

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.*

    As the paragraph about the BDC changes was reduced, this sentence sentence was removed.

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.*

    The reviewer is right, and the sentence was removed from the manuscript.

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.*

    The citation to Ploeger et al., 2019 was moved to the paragraph of the Introduction that explains CTM studies about AoA:

    Recent intercomparisons showed that the AoA depends to a large extent on the input reanalysis, both using the kinematic approach (Chabrillat et al., 2018) and the diabatic approach (Ploeger et al., 2019).

13. *Page 3, line 66, this "nitrous oxide (N2O)" is already mentioned in page 2, line 53 but online define now.*

    The first occurrence of the nitrous oxide formula is now at Page 1 line 5:

    Since the photochemical losses of nitrous oxide (N_2O) are well-known,....

and the "nitrous oxide (N_2O)" at Page 3 line 75 is replaced by "N_2O":

In this study we use N_2O as ...

14. *Page 3, line 77, please be specific here by replacing "from several reanalysis datasets." With "from the Chemical ObsErvation (BASCOE) Chemistry-Transport Model (CTM) driven by several reanalysis datasets (Chabrillat et al., 2018)."*

    The paragraph was rearranged, we now mention the BASCOE CTM and the reanalyses used to drive it in a separate paragraph:

    Here we use the same CTM as for the kinematic AoA study, i.e. the Belgian Assimilation System of Chemical ObsErvation (BASCOE) CTM. Observations of another long-lived stratospheric tracer, HCFC-22, were recently interpreted with WACCM and BASCOE CTM simulations, showing the interest of this model intercomparison (Prignon et al., 2019). In order to contribute further to the S-RIP BDC activity, four different dynamical reanalyses are used here to drive the BASCOE CTM simulations, compute the N_2O TEM budget and compare its components with the results derived from WACCM. Namely we consider: the European Centre for Medium-Range Weather Forecasts Interim Reanalysis (ERA-Interim, Dee et al., 2011), the Japanese 55-year Reanalysis (JRA55, Kobayashi et al., 2015), the Modern-Era Retrospective analysis for Research and Applications version 1 (MERRA Rienecker et al., 2011), and version 2 (MERRA2 Gelaro et al., 2017).

15. *Page 3, line 77, remove "Dynamical" and replace by "Reanalysis products"*

    Done.

16. *Page 3, line 81, move "Fujiwara et al., 2017; Cameron" after "models".*

    Done, and the reference to Fujiwara et al., 2017 was removed:

    Reanalyses are made using different assimilation methods and forecast models (Cameron et al., 2019), and ....

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).*

    The citations were added both in the Introduction (see reply to minor point 14 above) and also in a new Table 1 that provides an overview of all the datasets used in this study.

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.*

    Thanks to the new structure of the manuscript, the description of the Sections does not include subsections anymore:

    In Section 3 we analyse the seasonal mean patterns of the TEM N_2O budget in each dataset and their differences. Sections 4 and 5 investigate respectively the mean annual cycle and the variability of the N_2O TEM budget terms, with a focus on the differences between the datasets. Section 6 concludes the study with a summary of our findings and possible future research.

19. *Page 4, line 102, "Data and methods". There is no "s" to "method".*

    Done.

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."*

    The sentence was rephrased as:

    We ran one realization of the public version of WACCM (hereafter WACCM4, Marsh et al., 2013), with a similar setup (e.g. lower boundary conditions) as the CTM experiments; the source code of WACCM4

is available for download at https://svn-ccsm-models.cgd.ucar.edu/cesm1/release_tags/cesm1_2_2cesm1_2_2.

21. *Page 4, line 104, replace "trasport (see Sect. 4)." by "transport (see Sect. 4 for detailed analysis)". The same remark for "dataset (see Sec. 2.3)".*

    Done.

22. *Page 4, line 119, the "... (Lin, 2004)." is not correctly reported in the reference.*

    The reference was corrected.

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)."*

    Done.

24. *Page 5, line 130, add coma after "In addition"*

    Done.

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"*

    The sentence was rephrased:

    Chabrillat et al. (2018) explain in detail the preprocessing procedure that allows the BASCOE CTM to be driven by arbitrary reanalysis datasets, and the set-up of model transport. As usual for kinematic transport modules, the FFSL scheme only needs the surface pressure and horizontal wind fields from reanalyses as input, because it is set on a coarser grid than

the input reanalyses, and relies on mass continuity to derive vertical mass fluxes corresponding to its own grid.

26. *Page 5, line 135, please add a comma before "which"*

    Done.

27. *Page 5, line 139-141, please add a comma after "but" and "In this way".*

    Done.

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."*

    The sentence was rephrased:

    As for the WACCM experiment, we used the daily mean outputs from the BASCOE CTM over the 2005-2014 period.

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...*

    We do not mention the TEM N_2O budget at that stage anymore, and the sentence was rephrased:

    BRAM2 is the BASCOE Reanalysis of Aura MLS, version 2, which covers the period....

30. *Page 6, line 164, please remove this "Livesey, in preparation"*

    Done.

31. *Page 6, line 170, please the sentence after "temperatures," and start a new one.*

There is now a period after the temperatures (definition of $M^{(z)}$), and the new sentence starts with the definition of v^* and w^*:

$M^{(z)} \equiv$**.... .**

v^* **and** w^* **are...**

32. *Page 6 line 180, please add a comma after "Hence"*

    Done.

33. *Page 7, line 195, replace "hence retaining" by "while conserving"*

    The sentence was rephrased:

    Before any TEM calculation all the input fields are interpolated to constant pressure levels from the hybrid-sigma coefficients, that retain the same vertical resolution as the original vertical grid of each dataset (Table 1).

34. *Page 7, line 201, please a comma before "which"*

    Done.

35. *Page 7, line 202, add a comma after "Furthermore in WACCM"*

    Done.

36. *Page 7, line 206, replace "timestep" by "time step"*

    Done.

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."*

The sentence was rephrased:

The daily mean fields are interpolated from their native hybrid-sigma levels to constant pressure levels prior to the TEM analysis, leading to numerical errors in the lower stratosphere.

38. *Page 7, line 207, please add a comma after "For WACCM-CCMI"*

    Done.

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. Chabrillat et al., 2018) due to the biases in the temperature and wind fields (Kawatani et al., 2016, Tao et al., 2019). "*

    The sentence was rephrased as suggested.

    The four dynamical reanalyses used in this study provide overall consistent temperature and winds in the stratosphere, but can lead to a different representation of large-scale transport (e.g. Chabrillat et al., 2018) due to the biases in the temperature and wind fields (Kawatani et al., 2016; Tao et al., 2019). Note that the TEM quantities are not directly constrained by observations, especially the upwelling velocity $\bar{w}^*$, that can vary considerably in the dynamical reanalyses, as it is a small residual quantity (Abalos et al., 2015).

40. *Page 7, line 213, add a comma after "In the rest of the paper"*

    Done.

41. *Page 7, line 214, replace "BASCOE reanalysis BRAM2" by either "BASCOE reanalysis" or "BRAM2 product"*

    "BASCOE reanalysis BRAM2" was replaced with "BRAM2 product".

42. *Page 8, line 217, add a comma after "n Figs. 1 and 2"*

    The sentence was rephrased:

    Figs. 1 and 2 show the....

43. *Page 8, line 219, replace "the strongest" by "stronger ...". In addition DJF & JJA can be term as boreal winter and summer season.*

    The whole sentence was removed from the manuscript.

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.*

    "time der" was replaced by "X_t". The y-scale was widened so M_y could appear properly in the all panels. The horizontal bold line (i.e. the zero line) was removed.

45. *Page 8, line 225, please rephrase "In the northern tropics the N2O 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.*

    The whole sentence was indeed confusing. The discussion of Figs. 1 and 2 was reduced because it was repetitive and it aims to describe only the most important points.

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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 in consistent between WACCM and the reanalyses at all latitudes.*

    The discussion about Figs. 1 and 2 was changed, see our reply to major point 4.

47. *Page 8, line 226-229, please rephrase this sentence "At the higher latitudes the main terms contributing to the N2O TEM budget are the positive horizontal mixing term in the N2O increase, and the negative vertical advection and vertical mixing terms for the N2O decrease in all the datasets, with negligible contributions from the other terms." It's not clear and poor.*

    The discussion about Figs. 1 and 2 was changed, see comment above.

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.*

    As mentioned before, Figs. 1 and 2 are not meant to discuss differences in the datasets (this is left to the next Sections), but only to show how the terms of the TEM budget balance each other. The discussion about Figs. 1 and 2 was changed, see comments above.

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".*

    We agree with the reviewer, but, again, we do not wish to compare the datasets at this point of the manuscript. The discussion about Figs. 1 and 2 was changed, see comments above.

50. *Page 8, line 233-234, the term "Ay" also contribute in the mid lat.*

The discussion about Figs. 1 and 2 was changed, see comments above.

51. *Page 8, line 235, please replace "is affected mostly" by "is mostly affected..."*

    As the paragraph was largely changed, this is not included anymore.

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?*

    As mentioned above, Figs. 1 and 2 are meant only for illustrating the various terms of the TEM budget, and how they balance each other at different latitudes. This is now explicitly stated in the discussion of Figs.1 and 2. The differences between datasets are discussed in detail in Sect. 3,4 and 5 of the revised manuscript.

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 men wind fields.*

    We thank the reviewer for the comment, but we chose not to show the residual advection and not remove the full zonal wind because we think that the full zonal mean wind is useful for showing the polar jet, as it is related to the discussion of Fig. 4, and the addition of the arrows of v^* and w^* would make the panel rather difficult to interpret.

54. *Page 9, line 245, add a comma after "CCMI"*

    Done.

55. *Page 9 line 250, add a comma after "During the DJF season" and before "but"*

    Done, and we replaced "DJF season" by "boreal winter".

56. *Page 9 Why the colorbars in figures 3 and 4 have a different scales?*

We now use the same color scale [-2,2] ppbv/day for both the figures.

57. *Page 9 Why the differences between summer and winter term are not discussed?*

Those differences are discused in the revised manuscript (Sect. 3). See our reply above to major point 7.

58. *Page 9, line 259, add a comma after "In the JJA season""*

Thanks to the new manuscript structure, this paragraph was removed.

59. *Page 9, line 259-267, why the large "My" term from BRAM2 is not mentioned?*

This is now discussed in the fifth paragraph of section 3:

In the austral winter, over the Antarctic Pole and below 30 hPa, M_y agrees remarkably well in all datasets (Fig. 4). Closer to the vortex edge and above 30 hPa, the wintertime decrease of N_2O in the middle stratosphere is mainly due to downwelling in WACCM-CCMI, while the reanalyses, especially BRAM2, show that the horizontal mixing also plays a major role (Fig. 4).

60. *Page 9, line 262, replace "very positive values" by "large positive values"*

With the new manuscript structure, this paragraph was removed.

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.*

In the revised manuscript, the discussion of the Figs. 5 and 6 (now merged into Fig. 5) is separated in subsections organized by latitude band (Polar region, middle latitudes, Tropics), rather than by hemisphere. This allows to better describe similarities/differences between the hemispheres, and to avoid repetition whenever the patterns are similar.

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. You previous argument was that it's level of better assimilation of meteorological observations according to Manney et al. 2003. Please correct that.*

    The sentence was removed as the same statement was already in Sect. 2.4.

63. *Page 9, line 274, add a comma after "For WACCM-CCMI"*

    Done.

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"?*

    The part from "The color codes..." until "remain cautious" was moved to the caption of Fig. 5 of the revised manuscript. Regarding BRAM2, it is constrained by N_2O observations, which is not the case for the CTM nor for any of its 4 driving reanalyses. We do not consider BRAM2 as the "truth" more than we would consider an observational dataset to be the "truth". A whole paragraph explains this in section 2.4, both in the ACPD and revised versions, with the revised version stating:

    In the rest of the paper, we will assume that the BRAM2 product provides the best available approximation of the TEM budget for N_2O, at least where the residual is smaller than the vertical advection and horizontal mixing terms. This assumption relies on the combination in BRAM2 of dynamical constraints from ERA-Interim with chemical constraints from MLS (Errera et al., 2019)

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Furthermore the caption of Figure 5 in the revised manuscript states:

BRAM2 is depicted with a black line and symbols, as usually done for observations, because it is constrained by both dynamical and chemical observations.

65. *Page 9, line 282, replace "We first investigate" by "First, we investigate..."*

    Done and moved to Sect. 4.1 page 12 line 376.

    First, we investigate the N_2O mixing ratio...

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?*

    The physical reason behind the underestimation of N_2O in ERAI compared to JRA55 is the faster upwelling in JRA55 (evaluated by Chabrillat et al., 2018 through mean AoA) compared to ERAI (because of the inverse relationship between N_2O and mean Age of Air). Unfortunately, we did not have mean AoA output from WACCM to draw similar conclusions for the CCM. This is discussed in Sect. 4.3 of the revised manuscript:

    In the tropical regions, the N_2O mixing ratios in WACCM-CCMI agrees well with the reanalysis of Aura MLS, while the CTM results show large differences in the N_2O abundances depending on the input reanalysis (Fig. 9(c) and 9(d)). In regions where the mAoA is less than 4.5 years and N_2O is greater than 150 ppb, i.e. in the tropical regions and lower stratospheric middle latitudes (Strahan et al., 2011), the N_2O mixing ratio is inversely proportional to the mAoA, because faster upwelling (younger air) implies more N_2O transported from lower levels, decreasing its residence time and resulting in a limited chemical destruction (Hall et al., 1999; Galytska et al.,

2019). The dynamical reanalyses also produce large differences in mAoA at 15 hPa: MERRA delivers the oldest mAoA and MERRA2, ERAI and JRA55 progressively show younger mAoA (Fig. 4(b) in Chabrillat et al., 2018). Hence the large discrepancies in N_2O mixing ratio can be explained by the large differences in AoA, while M_y and A_z contribute to rates of change of N_2O.

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, ..."*

    Indeed, the structure was confusing. As mentioned before, we changed the layout of the manuscript, merging the Sections 3 and 4. In the revised manuscript, the Sect. 4 "Climatological seasonal cycles" is divided in three subsections by latitude bands: Polar regions, middle latitudes and Tropics.

68. *Page 10, line 289, replace "We then investigate" by "Second, we investigate..."*

    Done, and moved to page 15 line 464.

69. *Page 10, line 322-323, the sentence is not clear and can be split into 2 sentences and formulated clearly.*

    As a result of the structure of the manuscript, this sentence does not mention anymore the middle altitudes:

    A_z is positive all year round showing the effect of tropical upwelling, and agrees very well in the reanalyses (Figs. 9(i) and (j)), as a result of the good agreement in the tropical upwelling velocity at 15 hPa (Fig. S7 bottom row), and also as depicted by mAoA diagnostics (Fig. 4(d) in Chabrillat et al., 2018).

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70. *Page 10, line 326, add a comma after "Finally". Same after "In the Tropics from Novermber to April (Fig. 6(g))", same after "In the middle latitudes (Fig. 6(h))", same after "In the arctic region (Fig. 6(i))"*

    With the new manuscript structure, these parts were removed, or moved to the correct places and corrected.

71. *In this section 3.2, differences are reported but there is no physically explained attempt.*

    As mentioned before, we merged the Sections 3 and 4 to address this problem. We reduced the purely descriptive parts, and we moved (and enhanced where possible) the relevant scientific discussion to where it belongs for each figure.

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..."*

    Done.

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.*

    Done.

74. *Page 11, line 341, replace "We first consider" by "First, we consider"*

    Done.

75. *Page 11, line 342-343, in the [0° , 20°] at 15hPa, BRAMS N2O mixing ratio is more closer to the reanalyses at the first half of the year.*

    The sentence was rephrased for the [0° , 20°] latitudinal band:

WACCM-CCMI and the BASCOE reanalysis of Aura MLS BRAM2 show very similar variabilities, especially in the southern Tropics.

76. *Page 11, line 344, add a comma after "In the northern mid-latitudes (Fig.7(d))"*

    The middle latitudes were moved to the supplement:

    In the middle latitudes of both hemispheres, the inter-annual variability of A_z and M_y peaks in winter as its mean value (Fig. S8).

77. *Redo panel a) and b) of figure 8.*

    Done.

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?*

    In the revised manuscript, the Sect. 3.3 was merged with the relevant parts of Sect. 4 and the scientific discussion was improved, while some purely descriptive parts were removed.

79. *Page 11, line 347, add a comma after "In the middle latitudes (Figs. 7(e) and 7(h))"*

    This part was removed from the revised manuscript.

80. *Page 11, line 348, add a comma after "In the antarctic region (Fig. 8(c))"*

    This part was removed from the revised manuscript.

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?*

    The differences between the Arctic and Antarctic are discussed in Sect. 5 of the revised manuscript:

We now look at the interannual variability of A_z and M_y in the polar regions. Above the Antarctic, the inter-annual variability of A_z and M_y is maximum during spring (Figs. 10(e) and (i)), due to the large inter-annual variability in vortex breakup dates (Strahan et al., 2015). While the maximum variability of M_y is consistently reached in October in all the reanalyses, WACCM-CCMI simulates an earlier maximum (September) that does not correspond with the maximum in its mean values (Fig. 5(m)). The lower wintertime variability of both A_z and M_y would increase if a longer period was considered to include the exceptional Antarctic vortices of 2002 (Newman and Nash, 2005) and 2019 (Yamazaki et al., 2019). Above the Arctic, M_y and A_z are most variable during winter, reflecting the frequent disruptions of the northern polar vortex by sudden stratospheric warmings (SSWs, Butler et al., 2017). A case study of the effect of a SSW on the N_2O TEM budget was examined in Randel et al. (1994). They found a stronger A_z and M_y contribution (among the other TEM terms) during the SSW event than in the seasonal mean in the middle stratosphere in the Arctic. Thus, the large wintertime variability of A_z and M_y is explained by the occurrence of seven SSWs detected in the reanalyses for the 2005-2014 period (Butler et al., 2017). In the middle latitudes of both hemispheres, the inter-annual variabilities of A_z and M_y peaks in winter as do their mean values (Fig. S8)

82. *Page 11, line 349, replace "the vortex break-up," by "the breaking vortex period"*

    Done. The sentence was also rephrased:

    The variability of the N_2O mixing ratio increases in October i.e. during the breaking vortex period that is highly variable in time (Strahan et al., 2015).

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....."

This part was rephrased after the structure change of the manuscript.

The variability of M_y (Figs. 10(j) and (k)) is small compared to the extra-tropical regions, in agreement with calculations of standard deviations of the effective diffusivity within the tropical pipe (Abalos et al., 2016a). The reanalyses deliver a larger inter-annual variability in the northern tropics, during boreal winter, while in the southern tropics the variability of M_y is larger in the second part of the year. WACCM-CCMI does not reproduce this hemispheric asymmetry, with a rather flat profile in both hemispheres and a clear underestimation in the northern tropics, as shown for its mean values.

84. *Page 12, line 355, add a comma after "In the mid-latitudes"*

    This sentence was removed.

85. *Page 12, line 338, add a comma after "In the antarctic region (Fig. 8(e))".*

    This descriptive sentence was removed.

86. *Page 12, line 360 add a comma after "The Arctic (Fig. 8(f)) "*

    This descriptive sentence was removed.

87. *Page 12, line 360 add a comma after "Among the reanalyses"*

    This descriptive sentence was removed.

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.*

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This generic part of the Discussion was removed.

89. *Page 12, line 376, add a comma after " Above the Arctic in the middle stratosphere"*

    Done.

90. *Page 13, line 408, add a comma after "During the SH spring"*

    Done and "SH" was replaced by "austral".

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