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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#2 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

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Discussion paper

We thank the reviewer for his/her useful comments. 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.

However the manuscript should be highly improved in structure and wording! I have the feeling that in some sections the text lacks an organized structure. E.g. when describing the figures, the text jumps from one figure panel to another and it is really hard to follow. I recommend publication after carefully reading over the text again and rephrasing where it is necessaire

As recommended by both reviewers, the structure of the manuscript was changed: the Sect. 3 was merged with Sect. 4, and the manuscript was restructured as follows:

Section 1. Introduction

Section 2. Data and Method

Section 3. Latitude pressure cross sections

Section 4. Climatological seasonal cycles

Subsection 4.1 Polar regions

Subsection 4.2 Middle latitudes

Subsection 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 Sect. 3, and to better follow the text by latitude band when discussing the figures (especially for Fig. 5 and 6). The change in the manuscript structure led to a chage of the layout of the figures as well. We separated them by latitude bands of each hemisphere (one figure for the polar regions, one figure for the surf zones and one for the tropics) in order to better follow the flow of the Section 4 of the revised manuscript and its subsections.

The Introduction was revised as well. Every major concept now gets his own paragraph(s), and some of them were improved, e.g. reanalyses and CTMs, while the paragraph about long-term trends of the BDC was de-emphasized, because this manuscript investigates only climatologies and inter-annual variabilities but not long-term changes.

All these structure changes, together with the reduction of the descriptive parts, intend to improve the wording/phrasing of the manuscript.

Specific comments/questions.

1. *-page 1, line 2: reword: " ... from well-mixed tropical troposphere to polar stratosphere....": This is a bit too short, here one has the impression, that tracers are transported directly from trop. troposphere to the polar region.*

   The sentence was rewordedas follows:

   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 impor-

tant implications for chemical tracers distribution, stratospheric heat and momentum budgets and mass exchange with the troposphere.

2. *-page 1, line 7: insert "in " − > .... in a chemical reanalysis*

   Done.

3. *-page 1, line 10: .... have not been compared ....* before.

   Done.

4. *- page 1, line 14: Please clarify, I do not understand the sentence: "....reflecting the large diversity in mean AoA obtained with the same experiments." The present study does not look at AoA with CTM experiments.*

   Here we referred to the study from Chabrillat et al., (2018). They used the same configuration of the BASCOE CTM as for the current manuscript to do Age of Air calculations. Anyway, the sentence was not clear and it is rephrased:

   ....reflecting the large diversity in the mean Age of Air obtained with the same CTM experiments in a previous study.

5. *- page 2, line 27: include that you compare interannual variability* between the different datasets.

   Done.

6. *- page 2, line 33: reword and clarify this sentence to e.g. "The Brewer Dobson Circulation is characterized by upwelling of tropospheric air to the stratosphere in the tropics, followed by .... " . Note however that the BDC includes both residual circulation (net mass transport) and two-way mixing. Moreover the downwelling takes not only place in the high, but also in the mid-latitudes (change to − > extratropical downwelling) and not only in wintertime, although in the respective winter hemisphere it is much stronger.*

The sentence was re-written as follows:

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. For tracer-transport purposes the BDC is often divided into an advective component, the residual mean meridional circulation (hereafter residual circulation), and a quasi-horizontal two-way mixing which causes net transport of tracers, not of mass (Butchart, 2014).

7. *- page 2, line 46: Why should mixing be limited to a specific latitudinal region of the winter stratosphere? In the surf zone mixing is only stronger. (see e.g. Fig. 1 in BÃÂűnisch et al. 2011)*

The sentence was modified and the reference was added:

The two-way mixing is stronger in a specific latitudinal region of the winter stratosphere, the "surf zone" (McIntyre and Palmer, 1983), and in the subtropical lower stratosphere all year round (e.g. Fig.1 of BÃÂűnisch et al., 2011).

8. *- page 2, line 51: change to:"... due to the increase in well mixed greenhouse gases (e.g. Butchart et al 2014,...) and due to increased ozone depleting substances (e.g. Polvani et al. 2018 ...) "*

Done.

....due to the increase in well-mixed greenhouse gases (Butchart et al., 2010; Hardiman et al., 2014; Palmeiro et al., 2014) and ozone-depleting substances (Polvani et al., 2018),....

9. *-page 3, line 56 and line 63: Here the study of Fritch et al. 2019 (https://www.atmos-chem-phys-discuss.net/acp-2019-974) is interesting.*

The mentioned study is now included in the manuscript:

The difficulty to derive observational trends in the BDC can be partly attributed to the spatial and temporal sparseness of the observations, together with its large dynamical variability and the uncertainty of trends derived from non-linearly increasing tracers (Garcia et al.; 2011, Hardiman et al., 2017; Fritsch et al., 2019).

10. - *page 3, line 60: """* observational *trends in the ...*

    Done.

11. - *page 3, line 65: Say why is it important to do this separation?*

    This sentence and the previous one ("Furthermore the observational datasets cannot discriminate....") were removed from the revised manuscript as this paragraph was de-emphasized.

12. - *page 3, line 72: Could you write more about the study of Tweedy et al. 2017, as they are also looking at the N2O TEM continuity equation in GEOSCCM!*

    A sentence about Tweedy et al., 2017 was added:

    In the tropical lower stratosphere, the distinction between vertical and horizontal transport is important, as they impact differently the seasonality of N_2O in the northern and southern Tropics (Tweedy et al., 2017).

13. - *page 3, line 75: In Abalos et al. 2013 the stratospheric N_2O buget isn't shown.*

    The reference to Abalos et al. (2013) was removed.

14. - *page 3, line 85: change to: ...four different dynamical reanalyses are used here to drive simulations ....*

    The paragraphs about the reanalyses and the CTM were changed. Now the mentioned part states:

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:.....

15. *- page 3, line 88: Please clarify: Is only WACCM4 compared to BRAM2?*

    Both WACCM and the CTM experiments are compared to BRAM2, this is now explicitly stated:

    WACCM and the CTM experiments are also compared....

16. *- page 4, line 93: Are there studies with CTMs driven by reanalyses that studied tracer transport in TEM framework?*

    To our knowledge, a few studies were performed using CTM in the TEM framework, but they used dynamical fields obtained from CCMs and not from reanalyses (e.g. Strahan et al., 1996). Hence they were not deemed relevant to this work and we did not include them in the manuscript.

17. *- page 4, line 107-118: You explain the differences of WACCM-4 and WACCM-CCMI by model development. But are there also differences in the setup of the simulations (e.g. different SSTs, ...)*

    The model setup of WACCM4 was as similar as possible to the CTM experiments, to allow fair comparison. This is now stated in the manuscript:

    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;...

18. *- page 4, section 2: I recommend to include a table to give an overview over the different simulations (CCM, CTM with diff. reanalysis).*

    The table in now included (Table 1).

Dataset name	Reference	Dynamical Reanalysis	Chemical reanalysis of	Model grid	Top level
WACCM4	Marsh et al., (2013)	none	none	2.5x1.9, L66	$5.1x10^{-6}$ hPa
WACCM-CCMI	Garcia et al., (2017)	none	none	2.5x1.9, L66	$5.1x10^{-6}$ hPa
ERAI	Chabrillat et al., (2018)	ERA-Interim (Dee et al., 2011)	none	2.5x2, L60	0.1 hPa
JRA55	Chabrillat et al., (2018)	JRA-55 (Kobayashi et al., 2015)	none	2.5x2, L60	0.1 hPa
MERRA	Chabrillat et al., (2018)	MERRA (Rienecker et al., 2011)	none	2.5x2, L72	0.01 hPa
MERRA2	Chabrillat et al., (2018)	MERRA2 (Gelaro et al. 2017)	none	2.5x2, L72	0.01 hPa
BRAM2	Errera et al., (2019)	ERA-Interim (Dee et al., 2011)	MLS (Livesey et al., 2015)	3.75x2.5, L37	0.1 hPa

Table 1. Overview of the datasets used in this study.

19. - *page 5, line 132: WACM $->$ WACCM*

    Done.

20. - *page 5, line 137: ... as input...*

    The sentence was changed:

    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.

21. - *page 6, line 161: What do you mean with situation of interest?*

    "Situation of interest" was indeed misleading, a more appropriate wording would be "regions of interest". BRAM2 has been evaluated in several regions of interest in the middle atmosphere as defined in the BRAM2 paper (Errera et al., 2019): the middle stratosphere (MS) the tropical tropopause layer (TTL), the lower stratospheric polar vortex (LSPV) and the upper stratosphere polar vortex (USPV). The chemical species were only evaluated in some relevant regions, and BRAM2 N_2O was evaluated in MS, LSPV and USPV. The text was rewritten more clearly:

    BRAM2 N_2O has been validated between 3 and 68 hPa against several instruments with a general agreement between 15 $\%$ depending on the instrument and the atmospheric region (the middle stratosphere or the polar

vortex, see Errera et al., 2019). .

22. *- page 6, line 182: "N_2O balance" $->$ In this section you use tracer X to explain the TEM diagnostics, but here you change back to N2O. Perhaps you use N2O instead of X in the entire section?*

    We now use χ in all the formulas, and "N_2O balance" was changed to "tracer balance". Furthermore, we stated explicitly that χ represents the N_2O concentrations in the revised manuscript:

    ...where χ is the volume mixing ratio of N_2O,...

23. *- page 7, line 200: Can you be a bit clearer, please: You are giving the causes of the non-zero residual for WACCM, but what about the residuals in the CTM, and the chemical reanalysis? Is it only the timestep in BASCOE?*

    Regarding the reanalyses, the reason for the large residual could be the coarser resolution compared to their input reanalyses (especially for BRAM2), impacting the numerical errors in the the horizontal and vertical derivatives that are involved in the TEM analysis. For this reason, a new reanalysis of Aura MLS is planned (BRAM3) with the same horizontal and vertical resolution as in the CTM. The unresolved mixing can also play a large role, as discussed in Sect. 3 of the revised manuscript. Taking into account these two factors, the text was rewritten:

    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. The possible causes of the residual in all the reanalyses are discussed in more detail in Sect. 3

24. *-page 7, line 205: "...while ..." $->$ "...even though ..."*

    Done.

25. *- page 7, line 209: Note that Tweedy et al. 2017 looked at N2O TEM buget at 85 hPa in the tropics.*

In the revised manuscript, it is stated more clearly that they looked in the tropical lower stratosphere:

In order to validate our N_2O TEM budget, we reproduced the findings reported in Tweedy et al. (2017, Fig. 7) with WACCM-CCMI in the tropical lower stratosphere, and we noticed similar results (not shown).

26. *- page 7, line 213: Why does w^* vary in reanalyes data? Perhaps you can add one sentence more about Abalos et al. 2015.*

The sentence was slightly modified to include the main physical reason of the disagreement:

The upwelling velocity $\bar{w}^*$ can vary considerably in the dynamical reanalyses, , as it is a small residual quantity (Abalos et al., 2015).

27. *-page 8, line 219: delete "the" $->$ .... are strongest ...*

The sentence was removed from the revised manuscript.

28. *- page 8, line 220: You motivate the choice of the 15 hPa level with large differences between the CCM and CTM simulations in this region. Where do you see this? I suppose in Figs. 3 +4. And why isn't it interesting to see what is going on in the lower stratosphere?*

Indeed those differences can be seen from Figures 3 and 4. We didn't look at the lower stratosphere because the vertical range of validity for BRAM2 is limited to 3-68 hPa (Errera et al., 2019).

29. *- page 8, line 16: The terms, "vertical advection", "horizontal mixing" and their abbreviations Ay and My are mixed within the manuscript, even between one*

sentence these terms are mixed (e.g. page 8, line 225). Can you please use the terms consistently?

In the description of Figs. 1 and 2, we kept using the full names and their abbreviations (e.g. the vertical advection term A_z) as we explain the methodology in that section. In the rest of the manuscript we use the abbreviations A_z and M_y.

30. *-page 8, line 226: "higher latitudes" $-$ $>$ I can see this mainly in the northern higher latitudes.*

The description of Figs. 1 and 2 was largely reduced in order to remove purely descriptive sentences such as this one (lines 226-229).

31. *page 8, line 232 (and also line 229):"... especially in the reanalyses Az and the residual play a minor role": I wouldn't say, that this effect is "minor"!*

The whole paragraph was re-written, see comment above. Figures 1 and 2 are now described and discussed 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.

32. *- page 8 line 238: spelling: reanalyses*

    Done.

33. *- page 9, line 253: You only show thee reanalyses here, not four.*

    "...in the four reanalyses" was replaced by "...in the other reanalyses".

34. *-page 9, line 266: middle stratospheric $->$ middle stratosphere*

    Done.

35. *-page 9, line 257: "(Fig. 3(f), (i), (l))" $->$ right columns of Fig. 3*

    Done.

36. *-page 9, line 269: Motivate why you are choosing a single level in the middle stratosphere (15 hPa). What about the lower stratosphere?*

We tried several levels in the middle stratosphere and found that the differences between the datasets were most visible at 15 hPa while other levels did not bring added value to the intercomparison. With respect to the lower stratosphere, see reply 28 above.

37. *-page 9-11, description of the climatological seasonal cycles: In my opinion this section is very hard to read, as the SH and NH are separated into two pictures. I recommend to merge Fig.5 and 6 to one Figure and then describe first the tropical, mid-latitude and polar N2O (upper raw), second the vertical advection Az (middle row) and third horizontal mixing My (bottom raw). Thus it is easier to see the differences in NH and SH, the text is better structured and you do not have to repeat patterns that are similar.*

In order to follow this comment and another major comment by the first reviewer, Figs. 5 and 6 were re-organized into three figures, each of them covering both hemispheres. The revised Fig. 5, 7 and 9 show respectively the polar regions, mid-latitudes and tropics and are discussed in sections 4.1, 4.2 and 4.3 respectively. This new structure avoids any repetition while showing simultaneously, for each latitude band, the N2O cycle and the two main terms contributing to its TEM budget. Fig. 9 was also split into latitude regions and inserted as revised figs. 6 and 8, to contribute to the interpretation of our results in the polar regions and mid-latitudes. The tropical regions of Fig. 9 were moved to the Supplement.

38. *- page 9, line 278-281: What do you mean with uncertainty - the 1 sigma standard deviation?*

Yes indeed, as stated in Errera et al., (2019). This sentence was moved to the caption of Fig. 6 following a comment from Reviewer 1.

39. *- page 9, line 282: " We first investigate the N_2O mixing ratio in the SH. In the tropic (Fic 5c and 6a)...." $->$ Fig. 6a is not in the SH!*

After the rearrangement of the sections explained above, this sentence is not limited to the SH any more:

In the tropical regions the N_2O mixing ratio (Figs. 9(c) and (d)) in WACCM-CCMI ....

40. *-page 9, line 283: Please point out here more clearly, that BRAM2 is used as reference, and that this is the case for the entire section.*

This is pointed out more clearly after the structure rearrangement:

In the following, we will consider BRAM2 as the reference when comparing N_2O mixing ratios between datasets.

41. *-page 10, line 286: change to: ...is smaller than in BRAMS in all simulations.*

Done.

42. *-page 10, line 284-288: You missed to describe the mid-latitudes....*

With the new manuscript structure, the middle latitudes are now discussed in a dedicated Sect. 4.2.

43. *-page 10, line 287: You wanted to talk about N_2O, not about Az and My...*

That paragraph was confusing indeed. Now the discussion of the middle latitudes is put together in Sect 4.2. It starts with the N_2O mixing ratio in both hemispheres, and continues with A_z and M_y for each hemisphere.

44. *-page 10, line 300: "...expect for JRA55" $->$ expect JRA55*

Done.

45. *-page 10, line 305: "It is yet comparable..." $->$ What? The uncertainty.*

The sentence was removed from the revised manuscript, as it did not add any relevant scientific point.

46. *-page 10, line 311: Replace differ to different.*

Done.

47. *-page 11, line 337: Do you use the 1-sigma standard deviation?*

Yes indeed. The text could be more precise, as implicitly suggested by the reviewer. The revised sentence now states:

... we compute for each month the 1-sigma standard deviations of the N_2O mixing ratio, M_y and A_z across the ten simulated years.

48. *-page 11, line 335-340: I think it is easier for the reader if you plot the standard deviation the same way as in Fig. 5+6. I do not see a real advantage of plotting the results in this order. And as recommended before it would be nice to have Fig. 7+8 in one plot and restructure the text accordingly.*

We merged Figs. 7 and 8 into Fig. 10 of the revised manuscript. The text was restructured accordingly, and according to the new sections layout.

49. *-page 11, line 343: Why does the variability in WACCM-CCMI strongly depends on the considered realization? Shouldn't the internal variability between these ensemble simulations be similar?*

This was a surprising result, as in the other latitude bands the internal variability of WACCM does not play a major role. Strong differences between ensemble members with respect to inter-annual variability indicate that the considered period is not long enough to explore the inter-annual variability in the northern mid-latitudes, and that the mean variability from this ensemble (with only 3 members) would not be representative of the internal variability of WACCM. Fortunately, our

study did not investigate the ensemble mean but showed instead the full range from the 3 WACCM realizations. This will be stated in the revised manuscript.

50. *-page 12, subsection "polar regions": The structure of this subsection was not clear to me during reading: you first write about the wintertime North Pole, then about the wintertime South, then you jump to the SH spring and to Antarctic and Arctic inter-annual variability. Perhaps you can give an introducing sentence of what you will discuss in this section.*

    With the new structure of the manuscript mentioned above, this subsection was merged with Sect. 3, and all the information (wintertime North Pole, wintertime South,...) were moved to the right places when describing the figures.

51. *-page 12, line 375: What do you mean with "Above the Arctic in the middle stratosphere ... (Fig.6)"? Do you refer to the 15 hPa level in Fig. 6?*

    Yes. This paragraph was moved to Sect. 4.1 of the revised manuscript.

52. *-page 12, line 376: I cannot see that N_2O abundance in polar regions (Fig. 6c) are in good agreement in WACCM and BRAMS in the wintertime ...*

    The reviewer is right, and sentence was modified:

    Above the Arctic in the middle stratosphere, the N_2O abundances simulated by WACCM agree with the BRAM2 reanalysis, except in December and January, and....

53. *-page 12, line 379: Compared to which reanalysis? To all? Before you were comparing with BRAMS.*

    Yes, we consider here all the reanalyses. The text was modified accordingly:

    Compared to the dynamical reanalyses and BRAM2,....

54. *-page 12, line 381: Replace "Fig. 6 bottom raw", to Fig. 6 g+h. And why are you talking about tropics and mid-latitudes here? In this chapter you wanted to discuss the polar regions.*

The references to Tropics and mid-latitudes were removed as a consequence of the manuscript structure. The sentence was re-written:

Compared to the dynamical reanalyses and BRAM2, WACCM shows in the Arctic a 2-fold underestimation of the N_2O changes due to horizontal mixing during winter.

55. *-page 13, line 383: Do you mean the aging by mixing term in the polar regions of Fig. 2 in DietmÃ¼ller et al. 2018? Moreover reword "Note that ..." This is a poor transition between the two sentences.*

Yes, we mean aging by mixing. The sentence was modified for clarity:

It should be emphasized that WACCM is among the CCMI models with the lowest contribution of aging by mixing to Age of Air (Fig. 2 in Dietmuller et al., 2018).

56. *-page 13, line 386: Include that TEM AoA buget was done in CCM simulations.*

Done.

Dietmuller et al. (2017) applied the TEM continuity equation to the Age of Air (AoA) in CCM simulations.

57. *-page 13, line 391: Can you explain, why the TEM formulation is different in this study?*

Our formulation was misleading. The differences arise only from the different nature of AoA and N_2O: AoA does not have chemical sources nor sinks in the stratosphere, while N_2O is destroyed in the tropical higher stratosphere. Since

the definition of the dynamical TEM terms does not change, we removed "with a different TEM formulation", and the sentence now reads:

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 Dietmuller et al., 2017).

58. *-page 13, line 392: "... agreement: our residual term is larger ..." But you are listing the differences here.*

The second difference ("with a different TEM formulation") was removed. The point of this paragragh is that we find qualitative agreement between their "aging by diffusion" and our residual term, since both are computed as the remaining of the respective TEM budgets. We hope that the revised sentence makes this clearer (see previous comment).

59. *-page 13, line 396: Perhaps change to "....SH winter". (Also in other parts of the paper)*

The sentence was re-written:

In the austral winter, over the Antarctic Polar cap and below 30 hPa, M_y agrees remarkably well in all datasets (Fig. 4).

60. *-page 13, line 397: Again: What do you mean with "above 30 hPa"? Do you mean the 15 hPa level (latitude band 60-80S), as you are refering to Fig. 5?*

We referred to Fig. 4 of the ACPD manuscript. This sentence was moved and adapted to Sect. 3 in the revised manuscript, where it still refers to Fig. 4; this is now clearer because Fig. 5 is introduced only in the next section.

61. *-page 13, line 399: You are talking about Fig. 4, not about Fig 5!*

Same reply as for the previous comment.

62. *-page 13, line 401: Are these studies are giving an explanation for the mixing inside the vortex. If yes, can you please give the explanation here.*

De la Camara et al., 2013 states that the Rossby waves breaking can contribute to the tracer mixing inside the polar vortex and occasionally across its edge. The sentence was re-written as:

The impact of horizontal mixing on N_2O inside the wintertime polar vortex is not negligible (e.g. de la Camara et al., 2013; Abalos et al., 2016a), as Rossby waves breaking occurs there as well as in the surf zone.

63. *-page 13, line 403: Make clear, that it is overestimated in WACCM ... (and overestimated according to what?)*

Garcia et al. (2017) compared the winds simulated by WACCM to the winds from MERRA. This is stated more precisely in the revised manuscript:

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).

64. *-page 13, line 404: Change to : ... (see black thin lines in Fig. 4).*

Done.

65. *-page 13, line 405: You do not show the residual terms in Fig. 5.*

The sentence refers to Fig. 4, as the residual terms were not shown in Fig. 5. The sentence was moved to Sect. 3 of the revised manuscript, and changed:

Yet, the differences in M_y and A_z above the Antarctic in winter should be put into perspective with the large residual term that points to an incomplete TEM budget (Fig. 4 right column).

66. *-page 13, line 408: Say, why you are now looking at SH spring.*

Indeed, that change to SH spring was confusing as it was not introduced. After the change in the structure of the manuscript, this part was moved to Sect. 4.1 of the revised manuscript, and now it follows the discussion of the wintertime M_y at 15 hPa over the antarctic.

67. *-page 13, line 409:"... better agreement ..." Better compared to what?*

After the change in the manuscript structure, this sentence was moved to Sect. 4.1 of the revised manuscript for the description of Fig. 6:

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.

68. *-page 14, line 418: Replace "reanalyses" with dynamical reanalyses. And why is BRAM2 not included in this comparison?*

The word "reanalyses" was replaced by "dynamical reanalyses". BRAM2 is not included in this comparison because it is dynamically constrained to the winds from the ERA-Interim reanalysis, and its results are nearly identical with those of the CTM simulation driven by ERA-Interim, i.e. these differences are only due to the coarser resolution of BRAM2 and they are negligible.

69. *-page 14, line 434: Please explain critical lines.*

This is explained in the revised manuscript as follows:

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).

70. *-page 14, line 448: vmr $->$ mixing ratio*

    Done.

    Comments to the Figures:

71. *- Fig. 1+2: Can you please replace "time der" to dN2O/dt in the legend.*

    Done.

72. *- You are showing different colorbars in Fig. 3 and 4!*

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

73. *-Fig 5+6, y-axis: Replace X with N_2O.*

    Done.

References

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