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2. Reviewer #3

This manuscript investigates the effect of downward planetary wave coupling (DWC) events on Northern Hemisphere polar stratospheric ozone in MERRA-2 reanalysis data and WACCM simulations. The authors analyze the DWC modulation of O3 via a direct effect through changes in the residual circulation and transport, and an indirect effect, through changes in polar temperature and chemistry; and show that the direct effect dominates in explaining the changes in O3 during DWC events. Finally, the authors analyze the seasonal impact of DWC events (reflective Winters). I find this study interesting and adequate for publication in ACP after some minor revisions. In particular, reorganization of Figures, improvement of the comparison of model and reanalysis results, and better description of the results linking them to the direct and indirect effects as discussed in the Introduction. Detailed comments are listed below.

We thank the reviewer #3 for her/his constructive comments and very close reading of our manuscript. We have made substantial modifications that we hope have clarified our paper.

2.1 Specific comments

1. L. 104 and L. 108. Please explain a bit more what is the correcting tendency term.

The analysis term (i.e., correcting tendency term) is part of the Incremental Analysis Update (IAU) (Bloom et al, 1966), which is used in the GEOS5 model and is an additional forcing to constrain the model to the observations. We added this information in the text (**see P3. L31**).

2. L. 113. The period is not so clear as in line 91 it says 1980-2013 and in line 94, it says 1978 to 2004. I understand it is 1980-2013 but it would be better to clarify.

MERRA-2 assimilates satellite observations from the SBUV from 1980 to 2004, and from October 2004 from OMI and MLS (Bosilovich 2015). We have clarified this in the text (**see P4. L8**).

3. L. 135. Please add a bit more detail on the simulation of volcanic eruptions in CESM1 (WACCM), see for instance Marsh et al. (2013).

Observed volcanic eruptions of the twentieth century are included by prescribing a monthly zonal-mean time series of volcanic aerosol surface area density (SAD), identical to that used in the CCMVal2 REF-B1 simulations (**see P4. L25**).

4. L. 155. Define total. Is this the climatology plus anomaly?

Yes, the total is defined as the climatology plus anomaly (**see P5, L16**).

5. L. 157. How the results compare with DJF? I wonder if for the upward events, the coupling is larger in DJF than in JFM.

We focus our analysis during JFM because it represents the dominant time period of maximum downward planetary wave coupling in the Northern Hemisphere (Shaw et al., 2010, Lubis et al., 2013). Qualitatively similar results are found using the extended DJF and NDJFM winter season (consistent with ***Dunn-Sigouin and Shaw 2015***).

6. L. 164. It would be good to add a sentence on the comparison of the frequency of events, which is actually pretty similar between the reanalysis and the model.

We have now explicitly mentioned it in our manuscript. The frequency of DWC events in MERRA and CESM is similar, about 6 events per decade. (**see P5. L24**).

7. L. 170. Can you please explain briefly the Monte Carlo test? , so that the reader does not neccesarily need to check the references?

We have now included this in Appendix C.

8. L. 187. I notice Fig.S1c and d are the same as Fig.1a and d. In addition, I consider the results on v*T* and the divergence of the EP flux are important enough to be in the main figures (not in the supplementary material). Please include those panels in Fig.1 and then remove Fig.S1 from the supplementary material.

We have now modified the figure and the text accordingly (see our general comments in the first page).

9. L. 198. Which levels are the authors referring to? For each day the gray areas in Fig. 1a and 1b?

The levels refer to the region where the signals are statistically significant (i.e., between 100-1 hPa, gray areas in Fig. 1a and 1b). We have now clarified this in the text (**see P7. L9**).

10. L. 211-212. I don't see this transition in Fig. 2 a. I see the change from positive (day -5) to negative (day 0) but both maximum and minimum are at the same altitude (around 10hPa), so I don't see the change from the upper stratosphere to the lower stratosphere.

We apologize for this oversight. We agree that the positive ozone tendency subsequently changes sign and reaches its minimum value at 10 hPa, around day 0. We have revised the text accordingly (**see P7. L13**).

11. L. 220-223. Again, here changes in ozone tendency due to dynamics are discussed between the mid-lower stratosphere and the mid-upper stratosphere. I don't see that.

For clarity, we have included the pressure levels into the text **(see P7. L8-9*).***

12. L. 225-226.' . . . is evident in the upper stratosphere'. This is actually only true in the days before the DWC.

We apologize for this oversight. We agree that the contribution of the chemistry to the total ozone tendency is evident in the upper stratosphere before the mature stage of DWC, from days -10 to -5. We have revised the text accordingly (**see P7. L14**).

13. L. 228. '. . .are relatively small. . .' They are actually not significant except for those around day -7. I think the description related to Fig. 2c needs to be improved.

We have revised the description related to Fig. 2c (**see P7. L14**).

14. L. 231 and L. 248 (and description of Figure 3c and 7c). L. 231 says that the same conclusion can be drawn by assessing the instantaneous correlation between for upward and downward heat flux events. I don't see this conclusion from the 3 panels in Fig. 3. I think it is obvious that whatever relationship between w* and O3 is going to be associated to the dynamical term in equation [2] and not with the chemistry effects (which are related to production and loss). Am I missing something? So I do not see the point in showing panel c in Figures 3 and 7. I would keep these figures with 2 panels each.

We have now clarified this in our text that the instantaneous link between ozone and extreme wave-1 heat flux events is more dominated by the dynamical process, consistent with the results from Fig.2 where the transient changes of ozone during the life cycle of DWC is mainly due to changes in ozone transport (**see P8, L4)**.

15. L. 259 and others. I don't fully understand what the authors mean by 'reversible or irreversible' throughout the life cycle. Can you explain in the manuscripts what are the consequences of having a reversible or irreversible impact? Reversible means that even though the impact is e.g. negative, it can become positive in the future? Please explain.

A wave packet passing through a medium will induce EP flux convergence at its head and EP flux divergence at its tail, and the time integrated EP flux divergence will be zero (a reversible effect of the waves on the mean flow), assuming there is no dissipation or no nonlinearities (the non-interaction theorem). Thus, we expect the effect of a wave which propagates to the stratosphere and then gets reflected back down will be more reversible than that of a wave that gets absorbed in the stratosphere via nonlinear wave breaking and a cascade to small scales which get dissipated. With this in mind, reversible means that the effect of DWC on ozone is canceled out over the life cycle of the wave, as indicated by the time tendencies of ozone that change from being positive to negative. Thus the overall effect of having more DWC events in winter is to have lower ozone levels in the polar stratosphere (i.e., DWC weakens the typical increase of ozone induced by upward wave propagation). On the other hand, the effect of upward wave event on ozone is irreversible over the life cycle, with the time tendencies not reversing during the life cycle. This means that increased upward wave events result in increased ozone concentration in the polar stratosphere due to stronger transport. We have clarified this in our text (**see P8. L14, P8. L25).**

16. L. 264-265. I am not sure this sentence is right. I think it would be right if the time integration of ozone over the life cycle for DWC was negative, so it would balance the positive during upward events. But because Fig. 4 shows a time integration of ozone over the life cycle close to 0 for DWC and positive for upward events, I think what it means is maybe to 'minimize' or ' decrease' the increase in ozone, but not to 'prevent'.

As stated in the manuscript, the impact of DWC events on ozone is transient and involves *a positive to negative total ozone tendency evolution*, where the total net effect (as shown by time integration) is nearly zero. This means the impact of DWC on ozone is reversible. Therefore, increase DWC events in winter weaken the typical increase of ozone induced by upward wave events. We have now used the correct word for this in our text (**see P8. L29**).

17. L. 289. There are quite large differences in the values of Fig. 5c and 5d compared to 1c and 1d, with larger values in the model compared to MERRA. These differences are not mentioned in the text. Please discuss them.

We agree with the reviewer that there are significant differences in the values of the temperature tendency between model and reanalysis. However, the differences in the residual circulation anomaly between model and reanalysis are relatively small. The reason for the discrepancy in temperature is due mainly to bias in modeled temperature in WACCM. In particular, WACCM still exhibits a bias in the stratospheric westerly jets and polar temperatures in the NH winter, where the largest biases in the stratosphere are in the location of the maximum of the NH westerly jet (Marsh et al., 2013). This bias, however, could be reduced by increasing non-orographic gravity wave drag, but at the cost of a less realistic mesopause (see Richter et al., 2010 and Marsh et al., 2013). We have discussed this in the text (**see P10. L28**).

18. L. 325-334. Discussion of Fig. 8. I find interesting the differences in the evolution of total column ozone tendency the days previous to day 0 (significant blue lines in the figures), values about 1 DU/day in the model versus 5 in MERRA-2. Please discuss these differences and the possible reasons for them in the text.

We agree with the reviewer that there are differences in the time evolution of ozone tendency prior to mature stage (day 0) of DWC between model and MERRA2. The positive ozone tendency values prior to DWC event persist longer in the model compared to MERRA2, which is consistent with the persistent poleward residual circulation anomalies. We have now discussed this in the text (**see P11. L5**).

19. L. 333. Again, I think 'prevent' is not the best Word here.

We agree with the reviewer. We have modified this sentence and others by replacing "to prevent" with "to weaken" (**see P11. L10)**.

20. L. 343. How is m computed?

We have clarified this in the text **(see P11. L21)**

21. L. 345. Is sigma the sigma for JFM or which one?

The classifications are based on the vertical wave numbers (m) and zonal-mean zonal wind at 30 hPa (U30) in winter months (JFM). We have clarified this in the text (see. **P11. L25**).

22. L. 358. I see this tilt from 500hPa to about 100hPa but not up to the middle stratosphere.

We apologize for this oversight. Indeed, the eastward phase tilt with heights of the wave-1 structure is visible from the mid-troposphere to the lower stratosphere (**see P12. L10**).

23. L. 394 and discussion of Figure 12. I think it would help to add contour labels to the colors in the plots. It seems to me that the Dynamical and chemical terms cancel each other at the polar latitudes in Fig. 12b and c and 12e and f. Also the green contours mentioned in the caption seem black. Maybe better just to draw them in black.

We have now modified this figure by adding the contour labels, changing the intervals of the contour line and shading, and using another color table with better gradation (**see Fig. 11**). For a better comparison with the model, we have now combined this figure from MERRA2 with the figure from the model simulation. We do see that the CHM (DYN) terms are dominated the ozone tendency in REF during mid winter (late winter).

24. Regarding Fig. 12, I wonder how symmetric or linear the response is between REF and ABS winters, otherwise it's hard to know if the negative signal in Fig. 12a comes from positive anomalies in ABS or negative anomalies in REF. Can the authors discuss how the individual signals (ABS and REF) are to make sure the description of the differences make sense?

Yes the responses are symmetric. We have now discussed this in the text (**see P13 L1, P13 L12 and P13 L20**). The responses of ozone and ozone tendency in REF and ABS winters are symmetric with respect to climatological mean, so that negative ozone anomalies during mid winter or early spring indeed come from negative anomalies in REF.

25. L. 400, should it be 'in the upper polar stratosphere? Also, L. 401 talks about the signal at 10hPa but neither fig. 12a nor 12 d show significant signal at 10hPa. Please focus the description on the significant signals.

We have modified these sentences by focusing the interpretation of the results on the significant regions only (see **P13. L15**).

26. Section 5. Conclusions. I think the first 4 points could be combined. My understanding is that the direct effect described in the Introduction is the one related to transport and w*, while the indirect effect is that associated with the chemistry and their dependency on temperature. If this is correct, then it would make more sense to arrange the first four conclusions putting together these results on w* on one hand, and the results on chemistry on the other. I miss the link between the direct and indirect effect discussed in the introduction with the actual results of the paper.

We have modified our conclusion by focusing it into 4 key results: **(1)** The impact of DWC on the residual circulation and the temperature over the wave life cycle. This is important starting point to elucidate the direct and indirect impact of DWC on ozone. **(2)** The direct impact of DWC on ozone through residual circulation (*w* or PSI*). **(3)** The indirect impact of DWC on ozone through the temperature changes (*dT/dt*). Finally, we close the conclusion by stating the cumulative (seasonal) impact of DWC on ozone in mid winter and early spring. We removed the point 2 in the old manuscript, since it is already included in the current point 2.

We think that our current conclusion has encapsulated the important key results found in our study.

27. L. 445. Shouldn't be a positive divergence anomaly drawn in panel a of Figure 14, analogous to the negative anomaly in EP flux divergence in panel b?

Yes, we have added a blue shading in panel (a), indicating a positive divergence anomaly (see **Fig. 12**).

28. L. 471. I think also a better understanding on stratospheric conditions, right? As it was shown here that the wave geometry in the stratosphere matters.

We have clarified this in the text (**see P15. L18**).

29. L. 481. Figures 1a and 1b should be Figures A1a and A1b.

Corrected.

30. L. 493. Figures A1c.

Corrected.

31. Figures: Please indicate in Fig captions 1 to 4 that those are with MERRA2 data.

We have now added this information in Fig captions 1 to 4.

32. Figure 9. Add in the caption which line is which (shaded or line plot).

We have now modified the caption. The shaded (line) indicates the *m* (*U30*).

33. Figure 10. What is the author referring with wave geometry in the caption of Figure 10?

We have now clarified the caption. The wave geometry configuration is referred to stratospheric configuration where a vertical reflecting surface (*m*) bounded above by a well-defined high-latitude waveguide (*l*).

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