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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelled thermal and dynamical responses of the middle atmosphere to EPP-induced ozone changes" *by* K. Karami et al.

Anonymous Referee #2

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General Comments:

This paper uses EMAC to study the effects of prescribed polar ozone anomalies on temperature, winds, and wave driving and propagation, as well as the impact on sudden stratospheric warmings. The prescribed ozone anomalies are intended to represent the effects of energetic particle precipitation. This is a potentially interesting result that is certainly relevant for ACP. However, I have several major concerns about this paper and cannot recommend publication until these are thoroughly addressed. First and foremost, sections of this paper that are taken word-for-word from previously published journal articles and not cited must be rectified. Second, it's not at all clear how the ozone anomalies are introduced. Without this knowledge, it's just not possible to even evaluate the results since the entire paper hinges on this.

Assuming that the ozone anomalies are introduced in a discontinuous step-wise fashion as Figure 2 suggests, a major concern is that there is no justification given for why the authors choose what seems to me to be a very unphysical way of representing ozone loss due to energetic particle precipitation. The discontinuous ozone anomalies seem to be (again, this is not entirely clear in the paper) introduced instantaneously, which has repercussions that are not discussed at all. One first step in validating the method would be to show the time-altitude contours of temperature and wind and their respective anomalies to show that they are indeed realistic. Without this the reader is left with serious concerns about the validity of this method. Is this method established in other papers, and if not why was this method chosen over previously established methods of studying the effects of energetic particles in models in which they are not explicitly represented?

Another major concern is that ozone anomalies descending with time is characteristic of the EPP Indirect Effect, which is important in the stratosphere. So it seems a bit unrealistic to me to have the ozone anomalies of -30% descending in an EPP IE like manner from 0.01 hPa.

Specific Comments:

The authors say that the Fytterer paper guides the ozone anomalies for this study, but don't explain this any further than with a figure. I think more detail would be helpful for the reader about why the 30% was chosen uniformly for all altitude levels and months–except September/March. Why is September/March not included? Are the ozone anomalies based on the model results or the satellite results of Fytterer? I can only assume they are based on the model results because there is no evidence for EPP-induced month-long O3 changes anywhere near 30% above 50 km in May in the satellite data they presented. Indeed this is what we expect since HOx is the main EPP-induced ozone loss driver in the mesosphere, and HOx is short-lived there. Even the model O3 changes, which the authors of the Fytterer study say are larger than the satellite O3 changes, are not even close to 30% (more like 10-12%, and their scale only goes up to 20%). The authors should probably also mention that the Fytterer study was for the SH, and justify why they are using it for the NH as well. For example, the Fytterer analysis looked at the O3 depletion in the SH polar vortex, for which 60-90 degrees is a decent approximation. However, in the NH this approximation is not very good.

Ozone anomalies descending with time is characteristic of the EPP Indirect Effect, which is important in the stratosphere. This is also very evident in the upper right panel of Fytterer Figure 5, where the descending ozone anomalies start below 50 km. Therefore, it seems a bit unrealistic to me to have the ozone anomalies of -30% descending in an EPP IE like manner from 0.01 hPa. The EPP IE is a NOx-driven phenomenon, whereas the ozone depletion above the stratosphere is mainly HOx-driven and sporadic in nature (i.e., not usually lasting an entire month).

I think Figure 2 needs much more explanation. It isn't clear whether the ozone anomalies are introduced each month in the new altitude range, or whether they are done once at the top. The phrase "ozone anomalies move downward with time" (in the abstract and in Section 2.3) suggests to me that the ozone anomaly is moved by the model rather than forced anew each month. Although, the constant 30% anomalies and step-wise nature suggests that it is forced each month. Anyway, I think it is essential to clarify this more.

I also think it would help the reader to explain the choice of -4% for O3-TS a little more. As it stands there are two sentences for this. It would be helpful to say a few words about the Soukharev and Hood paper so that the reader doesn't necessarily have to go digging in another paper just to understand why -4% was chosen. Also, the O3-TS results are not mentioned in the abstract and seem like kind of an afterthought in the paper.

-Page 33285, line 23: "Although the UV radiation is only a small proportion of the total incoming solar irradiance, it has a relatively large 11 year Solar Cycle (SC)

variation...UV variations of up to 6% are present near 200nm where oxygen dissociation and ozone production occur and up to 4% in the region of 240–320nm where absorption by stratospheric ozone is prevalent."

This is almost word-for-word from Gray et al., 2010 (Reviews of Geophysics), starting at paragraph 10: "Although the UV absorption composes only a small proportion of the total incoming solar energy, it has a relatively large 11 year SC variation, as shown in Figure 3 (bottom). Variations of up to 6% are present near 200 nm where oxygen dissociation and ozone production occur and up to 4% in the region 240– 320 nm where absorption by stratospheric ozone is prevalent."

The authors should put this in their own words and cite the previous work appropriately.

-Page 33287, line 8: Shouldn't it be Fytterer et al. 2015 instead of 2014?

-Page 33292, line 5: It's not clear to me what the authors mean by "statistically significant". There is no mention of any statistical test that was performed or significance level given. If they are just referring to the difference between the perturbed O3 run and control run being larger than 1-, 2-, or 3-sigma of the control run, I think it would be better to say something like, "the differences are significantly larger than the internal variability of the control run". As far as I can tell there hasn't actually been any statistical test performed.

-Page 33292, line 13: The authors say that the temperature responses in middle and late winter are not statistically significant. I think it would help to qualify here what is meant by mid and late winter, but later in the paper they say that mid winter is December 16 through February 15. In terms of sigma levels, the January response is perhaps the most significant response, so I don't understand the statement that it's not significant.

-Page 33294, line 10: EP-flux diagnostics. There is no justification given for why the authors are using the quasi-geostrophic approximation for the EP-flux diagnostics. The discontinuous way in which the ozone anomalies seem to be introduced is essentially shocking the system with drastic, unrealistic temperature changes. This has the potential to generate small-scale waves, which would not be accounted for in the quasi-geostrophic approximation.

-Page 33300, line 21: “In contrast to the occurrence of the SSW events (0.6 events per year; Charlton and Polvani, 2007), SFWD take place every spring in both hemispheres and hence are more frequent than SSW.”

This is basically word-for-word again from another previously published work without being cited.

Hu J G, Ren R C, Yu Y Y, et al. 2014. The boreal spring stratospheric final warming and its interannual and interdecadal variability. Science China: Earth Sciences, 57: 710–718, doi: 10.1007/s11430-013-4699-x

Page 711: “Compared with the frequency of the SSW events (0.6 events per year (Charlton et al., 2007)), the SFW takes place every spring in both hemispheres (Black et al., 2006).”

This is word-for-word but with conflicting citations. How do the authors explain this?

The authors further say on page 33300, line 24: “Following Charlton et al. (2007) a SFWD is defined as the final time when the zonal mean zonal wind at the central latitude of the westerly polar jet drops below zero and never recovers to a specified positive threshold value (with thresholds of 5 and 10ms-1 of the NH and SH, respectively) until the subsequent autumn.”

Whereas Hu et al., 2014 page 711 say: “Recently, Black et al. (2006, 2007a, 2007b) defined an SFWOD as the final time when the zonal-mean zonal wind at the central latitude of the westerly polar jet drops below zero and never recovers to a specified positive threshold value (with thresholds of 5 and 10 m s-1 of the Northern and Southern Hemisphere, respectively) until the subsequent autumn.”

Again this is word-for-word except for the difference in the reference given, and I don’t

think the reference given by the authors is correct. Charlton et al. 2007 used the criterion that the zonal mean zonal winds return to westerly for at least 10 consecutive days to exclude final warmings from their analysis of sudden stratospheric warmings.

The authors should rectify these discrepancies and certainly cite the Hu paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 33283, 2015.