Atmos. Chem. Phys. Discuss., 15, C10463–C10466, 2015
www.atmos-chem-phys-discuss.net/15/C10463/2015/

Atmospheric Chemistry and Physics Discussions

ACPD

15, C10463–C10466, 2015

Interactive Comment

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

Anonymous Referee #1

Received and published: 15 December 2015

This work addresses the consequences of proscribed ozone changes on middle atmospheric temperature and wind fields. Specifically, it is considering ozone changes in the context of energetic particle precipitation (EPP) which might cause polar NOx enhancements. It is an interesting and relevant topic for this journal. My main concern, and it is a very serious one, is that the assumed ozone perturbations are demonstrably unrealistically large. Typically, they are well in excess of observations, in some cases of the wrong sign, and thus the effect is to dramatically overstate the importance of EPP to middle atmospheric composition and structure. This work needs to be reconsidered until more realistic assumptions are made.

Despite all the above, the work has potential value because it casts serious doubts on the reality of published correlations between surface temperature and geomagnetic

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

activity (comment #4 below). But before that they need to reconsider much of what they have done.

Specifically

1a) The text states that the ozone perturbations are guided by the Fytterer et al (ACP, 2014) study. However, that study only looked at the Antarctic; the present study applies this to the Arctic which is not valid. Arctic NOx enhancements (and ozone reductions) in the stratosphere are rare- to date only the 2004 spring can be considered a reliable detection (cf. Natarajan et al., 2005; Randall et al., 2005) although the spring of 2013 (Bailey et al., GRL, 2014) could be another candidate. The other year with significant mesospheric descent was 2009 and studies of that year have failed to find significant NOx descent into the stratosphere (Salmi et al., ACP 2011; Siskind et al GRL, 2015). Thus we have, at best, two years out of 10 and nothing for the other 8. At best, their assumed ozone reductions for the Arctic could be characterized as the extreme case.

1b) As far as the Antarctic, there is greater evidence for recurring stratospheric NOx enhancements (and ozone reductions); however, the maximum depletion that Fytterer show is 20%, not the 30% assumed here. Furthermore, the sign of the perturbation reported by Fytterer differs at some altitudes than what is assumed here- in the lower stratosphere they report a positive correlation between Ap and ozone, not the negative effect assumed here.

1c) The authors refer to papers such as Rozanov et al 2005 and Baumgaertner et al 2011; however, this reviewer would argue that those papers also overestimate the phenomenon of EPP NOx production. Randall et al (JGR, 2007) discuss how their observations are lower than Rozanov's simulations. For Baumgaertner et al, figure 6a of that paper shows over 30 ppbv of NOx in a deep layer from 40-50 km in January to represent an "exemplary" Northern Hemisphere winter. But reality for an extreme Northern Hemisphere winter is given by Figure 1 of Bailey et al [2014] (i.e. absolutely nothing in January and a narrow layer in March which dissipates in April).

ACPD

15, C10463–C10466, 2015

Interactive Comment

1d) Finally, Figure 2 shows a 30% depletion originating in the 0.1 – 0.01 hPa layer and gives the impression that this propagates downward. I think this is misleading. An ozone perturbation at these altitudes is due to HOx chemistry, not NOx and is known to be short lived. The many works of Jackman show that ozone-HOx perturbations dissipate in a few days and do not propagate down into the stratosphere.

1e) It's actually not obvious what Figure 2 really means. Do they change the perturbation in a discontinuous fashion from month-to-month? Or are they initial conditions which propagate downward of their own accord.

I think that this work needs to be reconsidered in light of what actually happens in the upper stratosphere and lower mesosphere. The perturbations between NH and SH are quite different and overall, smaller than what the authors assume (much smaller in the NH, somewhat smaller in the SH). They are also focused on a much narrower altitude range than assumed here (mainly between 1.0 and 20 hPa). I expect the resultant effects to be less (but more realistic). I think it eventually should be publishable, but only if it adheres to what is observed.

2) The question of "self healing" of ozone is not addressed, but should be. This is the idea that ozone loss at a higher altitude allows for greater ozone production below. This might be the cause of the positive O3-Ap relation that Fytterer observe, and they speculate as much.

3) Finally there is a question for the rationale for the O3-TS simulation. This presumably is a solar effect from photons, not particles. So why is it included in a paper entitled "EPP-ozone changes"? It seems out of place. But it need not be. I suggest that if they want to keep this simulation (there is nothing fundamentally wrong with it), they should consider these two suggestions. a) Change the title of the paper to something like "On the relative roles of photons and energetic particles to middle atmospheric temperature and dynamics". b) They should additionally include the 20 km perturbation to ozone that Soukharev and Hood reported.

ACPD
15, C10463–C10466, 2015

4) Even with the overestimated perturbations, and certainly upon revision downward, the lower tropospheric perturbations fall well short of those reported by Seppala et al (2009). The present paper barely gets a 1K perturbation into the tropopause region (Figures 4-5). To me, this casts serious doubt as to the reality of Seppala's correlations. At a minimum, this discrepancy needs to be discussed here. Ultimately this may be the real value of this paper (i.e. proving that Seppala's results are theoretically difficult/impossible to explain).

5) I have refrained from commenting in detail on their dynamical diagnostics because they will likely change significantly once the initial perturbations are done more accurately.

Editorial: The abstract is pretty qualitative, overly so in my opinion. It gives no numbers and as a result is not helpful for someone looking for a quick order of magnitude estimate.

Grammar: While I expect this sentence to be significantly modified (or deleted) in a revision that more accurately characterizes the ozone perturbations, the sentence on lines 890 should read "... at least be comparable to) ....

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

ACPD
15, C10463–C10466, 2015