

Atmos. Chem. Phys. Discuss., community comment CC1  
<https://doi.org/10.5194/acp-2021-149-CC1>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.



## Comment on acp-2021-149

Susan Solomon

---

Community comment on "Effects of enhanced downwelling of NO<sub>x</sub> on Antarctic upper-stratospheric ozone in the 21st century" by Ville Maliniemi et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-149-CC1>, 2021

---

Review of the paper by Maliniemi et al.

This paper uses WACCM CMIP6 simulations of the 21<sup>st</sup> century to examine the future behavior of ozone in the upper stratosphere using different scenarios. The primary conclusion is that while a cooler stratosphere leads to ozone increases over much of the upper stratosphere, an exception occurs in the Antarctic, where the authors argue that increased downwelling within the strong polar vortex deposits greater abundances of NO<sub>x</sub> from EEP, which deplete ozone. The paper makes some interesting points but requires revisions before publication. My comment are as follows:

1. The paper doesn't show what happens to the total ozone column, which is a key quantity. Because much of the ozone loss is in the lowermost stratosphere (especially in the Antarctic), I would suspect that Antarctic total ozone still undergoes recovery, perhaps even super-recovery. Please add a figure showing what happens to the global total ozone as well in this model.
2. The use of the term 'recovery' or 'super-recovery' in this paper raises the question of recovery from what – it should be recovery from ozone depletion due to CFCs if this term is to be used. Figure 1 shows values of average ozone mixing ratios from 1-10 hPa and from 10-100 hPa in different months and regions. These are very broad swaths, and the ozone depletion will occur only where chlorine is important; it may be small or even near zero at some of these levels. Also, the mixing ratio at 10 hPa is much larger than at 1 hPa, so this quantity is heavily weighted towards lower altitudes and is hard to interpret. The problem is even worse in Figure 2, where ClO<sub>x</sub> abundances for 1-100 hPa are presented. I suspect that there was not much ozone depletion from 1960-2000 (if any) at for example 1 hPa, so it's not clear that super-recovery is the right word there...it may not have been substantially "depleted" in the first place. Please provide an additional figure showing contour plots of the changes in ozone and ClO<sub>x</sub> from -90 to 90 degrees, 1000 to 0.01 hPa, for 1960-2000 and clarify where depletion occurs versus where the ozone changes of interest here occur. If there are regions or months where little or no ozone depletion occurred from 1960-2000 in the first place, then recovery or super-recovery is the wrong language and should be altered. These plots should probably be in percent units rather than mixing ratio, so that we can see the extent of changes everywhere.
2. Please elaborate what is in the model with regard to solar proton events, and whether those could modify the picture. See Stone et al., GRL, 2018.

3. Figure 6 shows that the maximum NO<sub>x</sub> increase occurs in August. It is essentially gone by October. Please explain why this occurs. I would have expected a longer residence time. Is it being mixed out of the vortex? Chemically destroyed? Or?

4. There are many mistakes in basic English in the paper. I note that at least one of the authors is a native speaker of the English language and request that attention be paid to proper English to make the paper more readable. "The" is missing in many places, for example, and those mistakes should be fixed by the authors, not the reviewers.

5. The title seems unclear, for the reasons noted above. Please rephrase. Something like "Effects of enhanced downwelling of NO<sub>x</sub> on Antarctic upper stratospheric ozone in the 21<sup>st</sup> century" might be suitable.

6. Line 22 and later (e.g., 106). Garcia and Randel 2008 was a modelling study; Butchart et al. 2014 was a review but stated what while models showed BDC increases, the data was unclear. The statement that the strength of the BDC is increasing should be edited if it is just based on models; if it's based on data then please provide an appropriate reference to back it up.

7. Line 27. Where in WMO, 2018, and does this pertain to ozone in the upper stratosphere?

8. Line 28-29. How can you be sure that EEP are the main cause? Please note the existence of other sources of NO<sub>x</sub> (SPE, but also simply downward transport from other sources besides EEP). Please clarify how it is you know that the NO increase you calculate is indeed due to EEP (versus e.g., photolysis and photoionization reactions at higher altitudes, transport from higher altitudes but originating from lower, sub-auroral latitudes, etc.); also are you referring to auroral electrons or others? If you can't be sure that EEP are the cause of the NO<sub>x</sub> change, please use different language. You could say upper mesospheric/thermospheric NO<sub>x</sub> for example, if transport from higher altitudes and lower latitudes is important.

9. Line 55. Briefly describe what was done in the CMIP6 recommendations on solar activity that are relevant here.

10. Line 63. Why 31 years?

11. Figure 1. I don't understand why these figures are so spikey if they are treated with the 31 year mean/LOWESS approach as described. Please explain.

12. Figure 2. Can you explain why the variability is greater in the future than the past?

13. Line 97. The increased ozone throughout the upper stratosphere is likely caused mainly by colder temperatures, not just at the equator.

14. Lines 103-104. I don't think this statement is quite right. While it seems likely that HO<sub>x</sub> production is important in the mesosphere, a key point is that mesospheric ozone loss chemistry is much less temperature sensitive than that at the stratopause – different reactions with different energies of activation are involved. Please rephrase.

15. Lines 130-135. Here the authors raise some doubt about how much of the changes referred to are indeed due to EEP and NO<sub>x</sub> versus other causes (e.g., dynamical). This is quite concerning since it's the main conclusion of the paper! Can dynamical contributions be checked by looking at other quantities in the model? for example, you might use SF<sub>6</sub> as a tracer for downward mesospheric transport and see how percent changes in SF<sub>6</sub> compare to the percent changes in ozone, for example. A clearer analysis is needed to

support the paper's conclusions, or the points made earlier and in the conclusions need changes.

16. Presumably if the NO<sub>x</sub> changes are caused by increased downwelling, then they should smoothly increase with time through the time of the simulation. Please show that. Also show what is happening at higher levels (e.g., 100-120 km) and lower latitudes to address my earlier comment on the potential importance of poleward and downward transport.

17. Line 140. Earlier, you said the equatorial lower stratospheric changes were dynamical.