

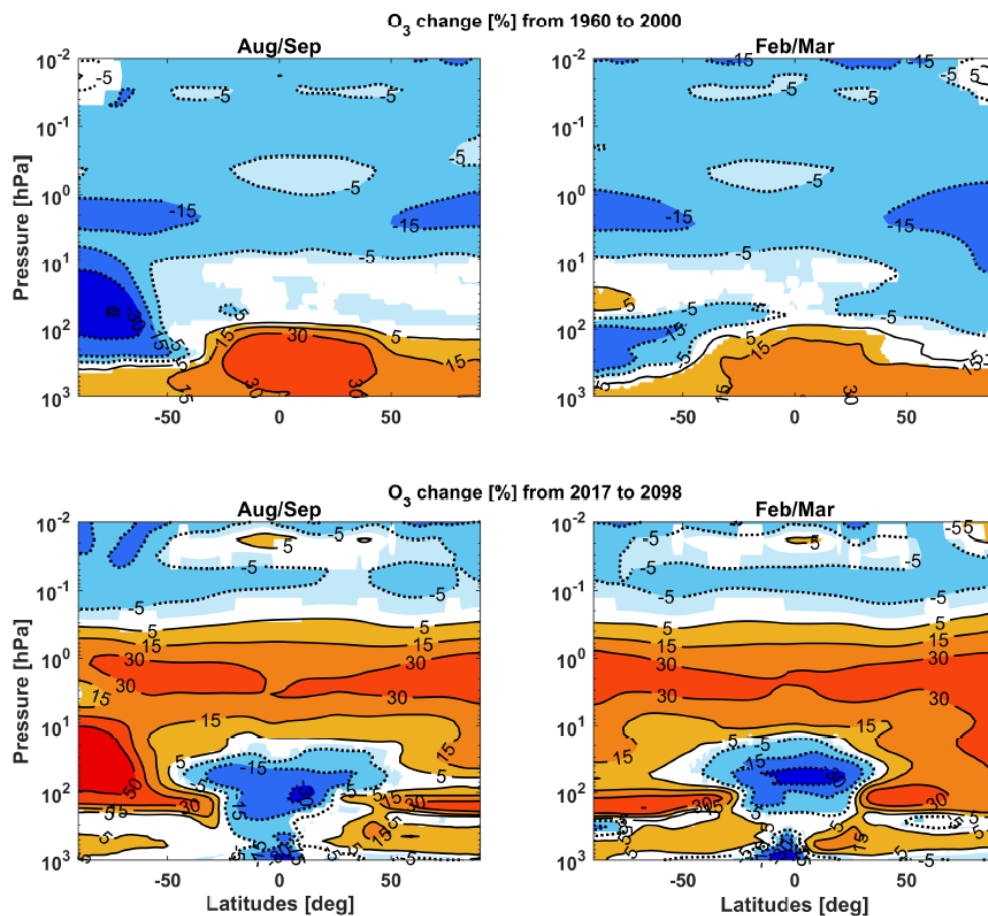
Reply to reviewer:

We thank the reviewer for reading of paper and pointing a few additional clarifications. For convenience, the original comments by the reviewer are indicated below in **bold blue font**. Our response to each comment is given in normal font.

Overall, the authors have done an excellent job responding to my comments and the paper is very much improved. I have only a few remaining concerns, only one of which is major.

1) Major. The authors have added some helpful figures on the vertical structure of ozone change, but I think there is still a need to see contour plots of the future ozone changes (latitude-height contour) that are the key point of the paper. I realize there are multiple scenarios, and am not suggesting they all be shown; perhaps . I think what is still needed is a plot for the 21st century change perhaps just SSP5. Figure 4 should be modified to do that. There is little point in showing the percent changes in ClOx in the bottom row. These should be replaced with contour plots for the percentage changes from 2000-2100 like those in the top row, so that we can see the full structure of the effect that is of interest here.

We modified Figure 4 as recommended. One can see the updated figure below. We also modified the part in the chapter 3.1 where Figure 4 is discussed.



2) Line 31-34. There were many papers on the topic of NO_x transport from the upper atmosphere to the stratosphere via the winter polar vortex and impacting ozone predating the ones given. The original references are, as far as I know, work by Solomon et al. in JGR in 1981 and Brasseur and colleagues around the same time. A very good early review is Garcia, Advances in Space Research, 1992.

We included citations to Solomon et al. (1981) and Garcia (1992).

3) Line 91, suggest “which is more notable in the northern hemisphere, as explained further below.”

The suggested phrase is now used.

4) Line 165. Do you really mean increased production? Or slower loss due to colder temperatures affecting the rate limiting steps in key catalytic cycles.

Yes, this was misleading. We now write it as: “Ozone super recovery in the upper polar stratosphere is thus mainly predicted due to the decreased ozone loss reactions in colder temperatures...” Similar changes are also made in other parts of the manuscript discussing this topic.

5) Closing statement, line 191-194. Changes in the polar vortex shown in this paper have been limited to the region above about the 10 mbar level. No connections from those levels to the tropospheric annular mode have been established by this paper or by other work, and I don't think it's appropriate to speculate about them here. The paper's conclusions section should reflect only what it has actually concluded and avoid speculation of this type.

We rephrased this part as: “Seasonal stratospheric ozone depletion due to the descending indirect NO_x has been also shown to influence stratospheric temperatures and the polar vortex (Arsenovic et al., 2016; Salminen et al., 2019; Asikainen et al., 2020). Thus, there is a great potential of improving future projections and seasonal variability of the polar stratosphere by implementing a more accurate solar forcing, including EEP to the earth system models (Matthes et al., 2017).