

Interactive
Comment

Interactive comment on “Tropospheric temperature response to stratospheric ozone recovery in the 21st century” by Y. Hu et al.

Anonymous Referee #1

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General comments:

The subject of this paper is quite interesting; the possible impact of ozone recovery in the stratosphere to the temperature trend in the troposphere. It is argued that stratospheric ozone recovery likely strengthens tropospheric warming which is primarily driven by GHG increase. While it is plausible as discussed by Grise et al. (2009JCLI; see also references therein), the current study presents somewhat unexpected result that ozone-induced warming in the troposphere is not limited to the SH high-latitudes, where ozone recovery is maximum, but also would occur in the tropics and the NH extratropics. It is further shown that NH warming is likely stronger than SH warming (e.g.

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Fig. 3). This result is hard to believe. If this is true, it will change our understanding on the global warming. Here are some of my major concerns.

1) Is the trend difference significant? Most figures show temperature difference between models with and without ozone-recovery forcing. However, none of them show whether the difference is statistically significant. Based on error bars in Figs. 1-3, I suspect the difference is not significant at all. In other words, the difference between the AR4 models with prescribing ozone recovery and those without it might result from model bias instead of any physical process. In fact, Fig. 2 of Polvani et al. (2010JCLI, in press) shows that AGCM integration with prescribing ozone depletion does not make any significant difference in the temperature especially in the tropics and northern extratropics. I strongly encourage authors to perform significant test at the first place. Without it, any relationship of causality cannot be established.

2) Model sensitivity test might be helpful. Extending the above comment, the comparison among different model sets is not quite clean. This is particularly true if the signal is relatively weak. To get a better insight, author may want to perform sensitivity test using a single CCM. In the experiment, one can prescribe time-varying or fixed ODS. By comparing these two experiments, one can have a clearer picture. It will also help for authors to identify the relevant mechanism(es).

3) More analyses are needed Although authors attributed tropospheric temperature change to the radiation (e.g., O₃, CO₂ and H₂O) and dynamics (e.g., Brewer-Dobson circulation), no evidence is presented. Using CCMVal-1 models, one can at least plot time series of chemical species as a function of pressure and latitudes. It can be used to support authors' argument. As a possible dynamical forcing, Brewer-Dobson circulation is also discussed in the paper. It however cannot change temperature in the low troposphere which is commonly shown in the paper. Are there any other dynamical processes which modify tropospheric temperature over the whole globe?

Specific comments:

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P22020 L2-8: Abstract is unnecessarily long. I suggest authors to cut first 3 sentences.

P22020 L3: Cite the CCMVal-2 report.

P22022 L6: This is true only in the SH.

P22023 L14: It is worth to note that details of prescribed ozone are not documented.

P22024 L25: is around 300 hPa -> is found around at 300 hPa

P22025 L20: Is it a global mean temperature trend?

P22025 L23: It depends on the time period of analysis.

P22026 L9- : It should be noted that Figs. 4 and 5 are already shown by Son et al. (2009, see their Fig. 8).

P22028 L17: This difference should not be attributed to the interactive ozone chemistry as indicated by Son et al. (2010JGR).

P22028 L21: The temperature response in the NH is most interesting part. But, it is not analyzed at all. . .

Figures: Many figures can be combined. It would help readers. I suggest authors to combine Figs. 2 and 3, Figs. 4 and 5, Figs. 6-7, and Figs. 9-10.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22019, 2010.

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