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

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I found the paper to be on an interesting topic although exploring issues quite close to others in the published literature. Although I encourage the authors to continue their study, I do have some concerns about the robustness and significance of some of the results which they present. In particular, to my reading of their plots they focus on differences between runs which could not be said to be significantly different and may result simply from sampling variation. Until the authors consider the sampling variation more thoroughly the paper is not suitable for publication.

We thank the reviewer's very helpful reviews, which are very important to improve our

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paper.

All three reviewers pointed out that statistical significance tests have to be shown for temperature trends and trend differences. We agree with the comment. Statistical significance is tested, and all figures are re-plotted. Temperature trends and trend differences with student t-test values greater than 2.0 (significance levels higher than the 95% confidence level) are marked with dots. Figures 1-3 attached here show that statistically significant trend differences are dominant (they correspond to Figures 5, 7, and 10 in the manuscript, respectively). Trends and trend differences in Figures 1-4, 6, 8, and 9 are also significant (not shown here). Wish these could clarify the key concern by reviewers.

Figure 1 (corresponding to Figure 5 in the manuscript). Global and annual zonal-mean temperature trend differences between AR4 models with and without ozone recovery (a) and between CCMVal-1 and AR4 models without ozone recovery (b).

Figure 2 (corresponding to Figure 7 in the manuscript). 300 hPa temperature trend differences between AR4 models with and without ozone recovery (a) and between CCMVal-1 models and AR4 models without ozone recovery (b).

Figure 3 (corresponding to Figure 10). SAT trend differences between AR4 models with and without ozone recovery.

Major concerns:

1. Expanding on the point above, consider Fig. 1. Here multi-model trends for two periods from the three model sets are considered. Apart from a few regions, error bars for all three models sets overlap. What is the reader to make of this, given the rather upbeat commentary in the text. Could the coincidence of the red (CCMVal-1) and blue (AR4 with O3 dep) be due to chance or is it a real physical effect? What would happen if a different sub-set of models was used for the calculation? While I appreciate the physical reasoning and previous work in this area may support the hypothesis of

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the authors, to my mind the evidence presented for a significant difference is weak at best. This concern applies to all of the analysis in the document including the vast majority which shows little or no calculation of sampling uncertainty (Fig. 4 onwards). In particular, once the authors begin to consider zonal, seasonal mean trend differences, noise must be quite large and the error bars on trend estimates subsequently much larger than in previous figures.

As shown and addressed above, significant trend differences are dominant in all figures. As for the CCMVal-1 results, we think that their consistency with AR4 models with ozone recovery is not by coincidence. In fact, we have calculated trends from 13 CCMVal-2 models. They are highly consistent with trends from both CCMVal-1 and AR4 models with ozone recovery. Because CCMVal-2 data has not been released to public, we do not show CCMVal-2 results in this paper.

2. Fig. 4 is quite mis-leading since red colours expand into the negative trend range. This must be corrected before publication.

It is changed.

3. My reading of the introduction reveals a significant mis-understanding of the authors since they combine the analysis of Ramanathan and Dickinson (1979) with the more recent work of Chen and Held (2009). A key difference here is that Ramanathan and Dickinson consider the direct radiative effects of Ozone changes on the surface energy budget whereas the accelerated Westerly winds referred to at the end of the paragraph are thought to be related to a dynamical link between the stratosphere and troposphere (caused by the radiative changes to the stratosphere). Although a small point, it is very important that this is clarified in future versions of the paper.

We agree with the comment. Changes will be made to address the issue clearer.

4. Methods. There is very little description of the methods used in the paper. One example where increased clarity would be beneficial is p22023 l11 'In temperature trend

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calculations, all available ensemble members are used for each model' What does this mean, is an ensemble mean calculated first and then trends estimated or are trends estimated for each ensemble member before calculating a mean trend. How does this effect the error estimates given that some models have more than one ensemble member while others do not?

We agree that the description of our methodology is not clear. Our method is: we first calculate ensemble average for each model. Second, we calculate ensemble average for each group of models. Then, we calculate temperature trends. Thus, the error bars in Figures 1-3 indicates variations among different models, rather than among ensemble members of simulations. This is the common way used by others, such as the series paper by Son et al.

5. A further mis-understanding of the literature (at least in my reading) is on p22024 I1. Son et al. make the point that the tropospheric trends simulated by stratosphere resolving CCMVal models and CMIP3 models with ozone recovery included are different. Representation of stratospheric dynamics is key.

Thanks for pointing out this. Son et al. (2010 JGR) actually first pointed out that their results from CCMVal and AR4 models are comparable (see their last sentence in the fourth paragraph of the discussion section). Then, they discussed the insignificant differences between the two types of models in the fifth paragraph. Thus, it is probably better for us to say these models show qualitatively consistent results.

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

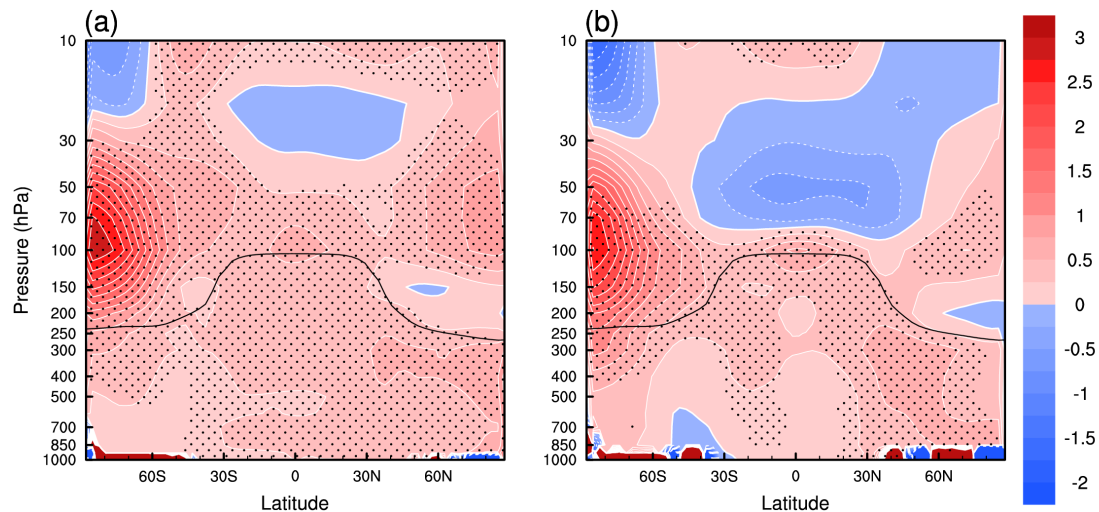
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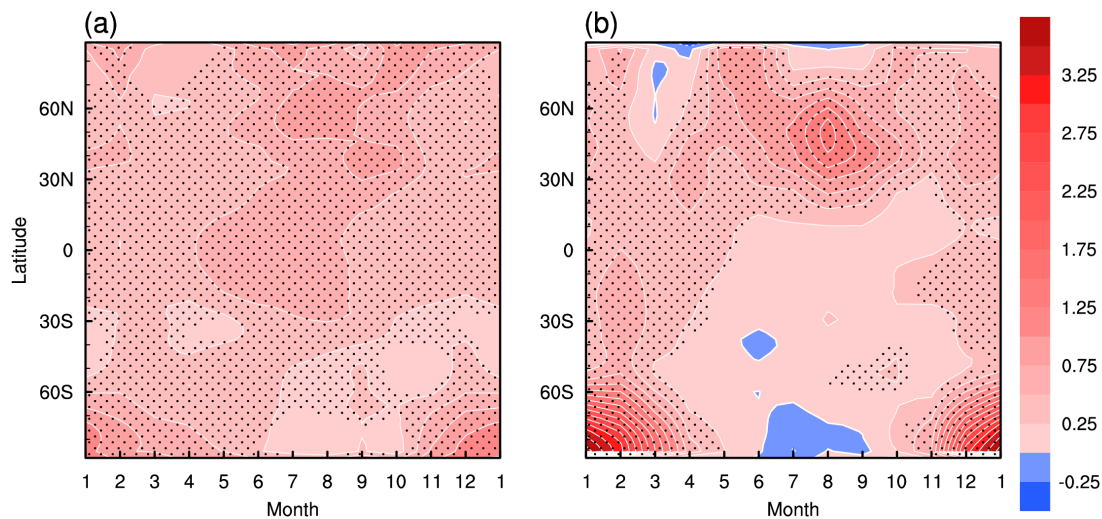
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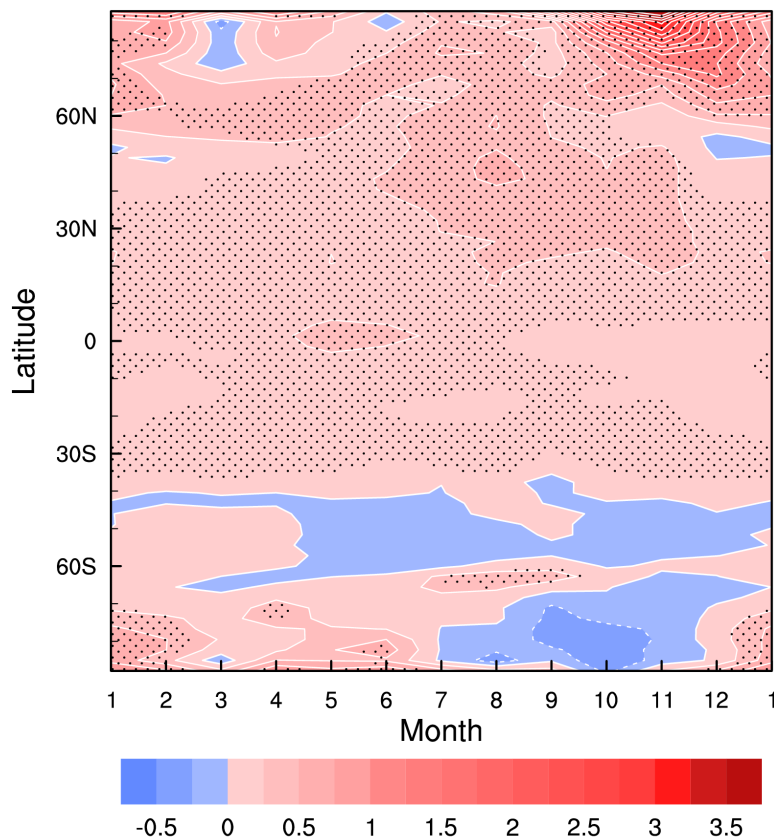
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