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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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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.

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 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.

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

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 refered 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.

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 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?

5. A further mis-understanding of the literature (at least in my reading) is on p22024 l1. 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.

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