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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Stratospheric ozone change and related climate impacts over 1850–2100 as modelled by the ACCMIP ensemble" *by* F. Iglesias-Suarez et al.

Anonymous Referee #3

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This study examines the stratospheric ozone concentrations in the past and future climates as simulated by the ACCMIP models. The results are then compares with the CMIP5 and CCMVal-2 models. Although these experiment sets have different configurations (i.e., prescribed surface boundary condition and no tropospheric chemistry in the CCMVal-2 models, prescribed surface boundary condition but interactive tropospheric chemistry in the ACCMIP models, and fully interactive surface conditions and atmospheric chemistry in the CMIP5 CHEM models with some notable exceptions as described in the manuscript), reasonably similar TCO climatology and trends are found in all experiments. However, as described in the manuscript, there are also non-

negligible differences in the regional trends. These differences are related with the GHG-induced stratospheric temperature changes, BDC changes, and photochemistry.

This study would be helpful to better understand the uncertainty of SPARC ozone data and the projected changes of the stratospheric and tropospheric ozone in a warm climate. Inter-model comparison, i.e., ACCMIP, CCMVal-2, and CMIP5, could be also applicable to other modeling project such as ongoing CCMI project. However, the present study is missing the detailed explanations. In many places, the authors argued that such differences or discrepancies are "likely"caused by photolysis and stratospheric circulations without presenting any supporting evidences. I understand that the main purpose of this study is to evaluate the ACCMIP simulations. However, this paper could become more exciting paper if additional analyses and figures that can support their arguments are presented. For example, intensification of the BDC and its differences among the models are repeatedly stated. But, no figures are shown for the equatorial upwelling or BDC. Since the computation of w* is not difficult, it could be evaluated at least for the ACCMIP models. The results could be compared to the tropical upwelling in the CCMVal-2 and CMIP5 high-top models which are presented in Butchart et al. (2010JCLI) and Charlton-Perez et al. (2013JGR).

Followings are some specific comments that could be taken into account when authors revise the manuscript.

1. Evaluation as a function of latitude In all figures, latitudinally-averaged quantities are presented. But, I think the latitudinal profile of annual-mean TCO or the latitude-month plot of monthly-mean TCO is much more useful. Such figure would be especially important to evaluate the extent of the polar vortex and its trend. I suggest authors to evaluate the climatological TCO (1980-2000) as a function of latitude (instead of the one presented in a small box in Fig. 1). Likewise, authors can present long-term trends of TCO, stratospheric O3 and tropospheric O3 as a function of latitude and season.

2. BDC To explain the biases of the tropical and NH midlatitude O3 concentration, au-

thors mentioned the importance of the BDC. Such influence could be simply illustrated by a scatter plot of tropical O3 and NH midlatitude O3 for all ACCMIP models. For example, if the modeled BDC is stronger than observation, negative relationship between the two would be stronger. Based on Fig. 5, I suspect that 50 hPa in the tropics (decreased O3 by the intensified upwelling) and 150 hPa in the extratropics (increased O3 by the enhanced downwelling) would be reasonable choice for the scatter plot. This scatter plot would also reveal the relationship between the mean biases and trends of tropical O3 and those of extratropical O3.

3. Interactive and zonally asymmetric stratospheric ozone. It is argued that "eliminating zonal asymmetry may lead to a poor representation of stratospheric and tropospheric climate trends in the SH". This point is repeated raised in the manuscript. However, no evidence is presented. In fact, Gerber et al. (2013BAMS) documented that, based on the inter-comparison between the CMIP5 (prescribed ozone) and CCMVal-2 models (interactive ozone), the response of the SH circulation is NOT dramatically sensitive to the interactive or zonally asymmetric ozone. Such sensitivity might be true in a single model (e.g., Waugh et al. 2009b). However, its impact is likely within the uncertainty in the multi-model framework (Gerber et al.). This issue should be clearly re-investigated in the revised manuscript.

4. Others - Fig. 1b versus Fig. 2b: Fig. 1b shows comparable TCO trends to observations. However, a large difference is found in Fig. 2b especially in the tropical UTLS. Are they consistent? BTW, it would be helpful if zero line is included in Fig. 2.

- Fig. 4: In the introduction, definition of tropopause used in the study is extensively discussed. However, in Fig. 4, tropopause is simply set to 150 hPa. Is there any reason to choose 150 hPa? This pressure level is certainly the upper troposphere rather than the lower stratosphere.

- Too many references: I am not sure whether that many references, over 6 pages, are necessary for the present paper.

- Typos: This paper is technically well written. But there are still several typos. I believe authors can easily correct them when revising the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25175, 2015.