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Interactive comment on "Inconsistencies between chemistry climate model and observed lower stratospheric trends since 1998" *by* William T. Ball et al.

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The motivation for this study is as follows: While CCMs show tropical O3 decline over the past 20 years that is likely driven by increases in tropical upwelling, models do not produce the (observed) decline in midlatitude O3; rather, they show an increase. The authors use a fixed dynamical heating model to estimate the impact of the negative O3 trend on the temperature trend. The result is that the observed temperature trend is consistent with the observed O3 trend. The conundrum the authors find is that in spite of the disagreement between model and obs midlatitude O3 trends, the models and obs get similar temperature trends – this is not the physically expected response. The authors propose that the explanation for this is the models have stratospheric water vapor trends that are opposite to those observed, thus creating a dynamical heating term

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Discussion paper

that opposes that of the simulated O3 trend, leading to fortuitous agreement with the observed temperature trends. A sort of ‘two wrongs make a right’. This lack of agreement between the CCMVal-2 models and observations motivates this study, whose intent is to explain what’s wrong with the models and thereby help improve them and increase confidence their O3 projections.

Much of the knowledge regarding model problems that is so ‘urgently needed’ is already available in the SPARC (2010) CCMVal evaluation. It has over 400 pages of detailed analyses showing why these models don’t match observed O3, temperature, trace gases, variability and more. Chapter 2 is very useful because it details how each model differs in its representation of radiative, chemistry, and dynamical processes, boundary conditions, etc. Chapter 3 is all about radiative processes and the information presented reveals much about how these models can be expected to respond to changes in radiatively active trace gases, i.e., O3. Chapter 5 reports on the transport issues affecting the credibility of their ozone simulations, in particular how well they represent tropical ascent and mixing out of the subtropics – topics so very relevant to how models’ circulations will be respond to increasing GHGs and alter future ozone distributions. Chapter 8 examines simulated O3 variability and whether models have the necessary processes to simulate that variability (spoiler alert: they don’t). This report may be 9 years old but there is much about models and the physical processes requisite for simulating ozone that must be understood before undertaking an investigation of why CCMs don’t do a particular thing right.

The authors justify their use of the CCMVal2 simulations rather than the CCMI runs because these models (in some ways?) are ‘still representative of the state-of-the-art’. I think it is a mistake not to use the CCMI runs because although some models may be the same as in CCMVal2, others have made improvements. (Shouldn’t the goal be to improve the current state of modeling?) Whether you use CCMVal2 or CCMI simulations, the conclusions of the SPARC (2010) report still provide a relevant starting point for a study like this.

Using these CCMs' transport behavior to support the interpretation of observed O3 trends in terms of ascent and mixing is problematic given the many model transport problems diagnosed in Ch. 5 of the SPARC report. See in particular Figure 5.20 that evaluates tropical ascent and meridional mixing. Most of the models used in this paper did rather poorly here. When a model does a poor job at representing a physical process, it should never be assumed that the simulated process will (magically) respond in a physically meaningful way to changes in forcing (e.g., increasing GHGs).

Furthermore, given that these CCMs have many different radiative, chemistry, and dynamical problems, using the multi-model mean (MMM) is a bad idea. The MMM is a mash-up of correctly and incorrectly simulated (or missing) processes. This manuscript works from the assumption that something physically meaningful can be derived from their analysis of the MMM. That's not possible. As an example, here are some relevant conclusions from the Ch. 3 summary of the models' radiation evaluation that speak to what you get with a MMM:

"...5 out of 18 CCMs show biases in their [temperature] climatology that likely indicate problems with their radiative transfer codes." "Problems remain simulating radiative forcing for stratospheric water vapour and ozone changes with a range of errors between 3% and 200% compared to LBL [line by line] models." "The stratospheric water vapour forcing has errors of over 100% between the models (Figure 3.12)."

Did you know water vapour in some of the models can't respond to climate change because a water vapour climatology was used? I also read in the report of a situation where the MMM quantity (I don't remember which one) actually got a higher grade than the models did individually on a particular evaluation. Bottom line: the MMM is not a quantity from which you can derive physical meaning.

I'm curious why the authors excluded 6 of the 18 CCMs models for the MMM. The excluded models (AMTRAC, EMAC, E39, GEOSCCM, UMetrac, and UMUKCA-Meto) all provided O3, H2O, and T outputs.

On a different topic, the authors report that lower stratospheric ozone has negative trends, a conclusion not broadly agreed upon by the community. See for example Steinbrecht et al. (2018), WMO (2019), and the LOTUS report (2019). None of these publications finds statistically significant O3 trends below the upper stratosphere. Steinbrecht et al. note that the search for ozone trends is complicated by ozone variations not caused by declining ODSs, such as variability in or changes to the Brewer-Dobson circulation. The role of circulation variability on ozone trends is acknowledged by the authors, yet they seem not to recognize that the CCMs' inabilities to provide anything close to observed interannual variability in stratospheric composition is one of the greatest shortcomings that interferes with the ability to simulate credible O3 projections. The QBO is the greatest source of stratospheric composition variability and these models either have no QBO or an unrealistic one. This is the elephant in the room with respect to 'why don't the CCMs get a negative or flat O3 trend over the past 20 years'. See Chapters 4, 5, and 8 in the SPARC report.

The overarching motivation of this paper – to improve the credibility of CCMs– is fine. The analysis of individual model behavior, especially in a model with vetted radiative, chemical, and transport processes, may produce useful insights into CCM needs. However, the problem stated by the paper's title – inconsistencies between chemistry climate model [sic] and observed lower stratospheric trends – cannot be solved with the chosen approach. The results presented rely on analyses of the physically meaningless MMM, which is fundamentally not a valid approach; for this reason I do not recommend publication.

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