

Interactive comment on “Simulations of preindustrial, present-day, and 2100 conditions in the NASA GISS composition and climate model G-PUCCINI” by D. T. Shindell et al.

W. Collins (Referee)

bill.collins@metoffice.com

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General Comments

Many studies have been published on the effects of climate change on tropospheric chemistry, and to a lesser extent on stratospheric chemistry. These papers often end with a comment to the effect that further research in this area needs to be carried out with a combined tropospheric-stratospheric chemistry model. I am very pleased that this paper is addressing this issue.

The impact of STE on radiative forcing is dismissed in this paper, which rather suggests that STE is important only to enhance our technical understanding. I would have liked

more evidence to support this lack of radiative impact.

It was not entirely clear why two different scenarios (A1B and A2) were needed, and what scientific questions were answered with which scenario.

More discussion of how the recognised model deficiencies might effect the scientific conclusions is needed in the paper. This is particularly important for STE since in the high latitudes, both the ozone concentrations and the transport seem to have large errors.

The encouragement to come up with a standard definition of STE, and for modellers to present the tropical and two extra tropical regions separately is very welcome.

I recommend that this paper be published after corrections.

Specific Comments

Page 4796, lines 25-27: Should explain that the A1B and A2 scenarios here include changes in composition and emissions of ozone precursors and depleting substances as well as climate change.

Page 4796, lines 27-28: How does the dry deposition change? Does the deposition velocity change, or does the flux change with a constant velocity due to increases in surface concentrations?

Page 4797, lines 3-4: Is the RF dominated by emissions? In the A1B scenario, climate change removes three-quarters of the RF due to composition.

Page 4808, line 20: In what way is the depth of the ozone hole over Syowa reproduced reasonably? The amplitude of the seasonal cycle is much greater than the observations, and the absolute annual minimum in the simulation is off the scale of the plot.

Page 4813, line 20- page 4814, line 14: Some comment on the disagreement with the two TRACE-A measurements needed here.

Page 4814, line 28: Is the global source of lightning NO_x tuned in this model?

Page 4815, line 8 - page 4816 line 3: This section on nitrogen deposition is not needed as nitrogen deposition does not feature in the rest of the paper. The N-deposition cannot be used as a validation of the wet deposition scheme, since the liquid trace budget was not used for the comparisons listed here and might give very different (not necessarily better) results.

Page 4816, lines 15-22: This paragraph on chlorine and bromine nitrate is not necessary.

Page 4817, lines 1-4: How long was the model methane spun up for, and has it reached equilibrium? Have any offsets been applied to the model methane values to compare them in figure 12?

Page 4817, lines 5-24. This level of detail isn't necessary. Something like "Model predictions of CO and CH₃O₂ have been shown to agree well with observations in Shindell et al. 2003 and 2005." would suffice.

Page 4818, lines 10-26: Section 3.6 is not needed as aerosols are not discussed further.

Page 4819, lines 10-11: How have the biomass burning emissions in the pre-industrial been derived?

Page 4819, lines 11-13: It seems inconsistent to increase tropospheric methane concentrations in A1B, but not ozone precursor emissions.

Page 4823, line 20: The authors should explain why the OH and H₂O distributions are different.

Page 4825, line 21: The authors should explain how the different components were separated for figure 20.

Page 4826, lines 22-24. How does wind speed affect the dry deposition, does v_d

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depend on the wave height? The r_a term in the deposition velocity is not very sensitive to wind speed in the high wind regime, and with such high surface resistances, v_d is not very sensitive to r_a anyway. Ice and sea have comparable deposition velocities, so the change in sea ice should not affect the overall deposition around Antarctica. Are the authors sure the decrease in ozone is due to dry deposition?

Page 4827, lines 15-18: Why is figure 21 so different from fig 17? The chlorine loadings are similar. The caption on fig 21 should say "composition and climate" not "emissions and climate".

Page 4827, lines 18-26. This section states that the upper stratospheric ozone recovers to more than the 1979 or PI values, but then says the ozone layer fails to return to its 1979 or PI state. Are the authors making a distinction between the upper stratosphere and the ozone layer? This section needs re-writing to make it clearer.

Page 4829, section 4.3.4: There is confusion here about what is fixed in A1B. Comparison between A1B composition and A1B composition and climate tells us nothing about the effect of emissions (line 9 and lines 18-20) since emissions are fixed at present day. Similarly comparisons between A2 and A1B are not appropriate since ozone precursor emissions don't increase in A1B (although all the precursor concentrations will be different as the background methane change will affect all chemical species).

Page 4821, line 3: "... including simplified lower stratospheric chemistry".

I don't think this is correct. I believe Zeng and Pyle simply applied their tropospheric chemistry in the stratosphere. They certainly didn't include halogens or PSCs, so their lower stratospheric ozone concentrations don't include any halogen-catalysed depletion at all. The same is true for Sudo et al.

Page 4832, lines 5-9. Since the A1B scenarios include ozone recovery and A2 climate-only doesn't, this surely explains the difference.

Page 4832, lines 17-19: This doesn't agree with table 6 which gives a decrease in the

A2 climate-only tropical flux, and decreases between the A1B climate composition and A1B climate in the tropics and southern extra-tropics.

Page 4834, lines 2-4: This doesn't agree with table 6 which shows a tropical upwelling change of -45 when going from A1B composition to A1B composition and climate, compared to +7 from PI to PD.

Page 4835, lines 20-21: What effect do these deficiencies and biases have on the STE calculations?

Page 4838, line 10-12:"future ozone recovery" - To be exact Zeng and Pyle (and Sudo et al.) don't include all of the present day ozone depletion since they simulate their own ozone up to 50 hPa without halogen chemistry. So it is partly "recovered" by default.

Technical corrections:

Page 4800, lines 6-10. There are two "along"s in this sentence, please simplify.

Page 4803, line 2. Delete ", if not all,". The Met Office aerosol model has included this for at least 10 years.

Page 4808, line 7: "send"->"seen"

Page 4851, table 6: "A1B emissions"->"A1B composition"

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