Atmos. Chem. Phys. Discuss., 12, C9166–C9169, 2012
www.atmos-chem-phys-discuss.net/12/C9166/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interactive ozone and methane chemistry in GISS-E2 historical and future climate simulations" *by* D. T. Shindell et al.

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Received and published: 9 November 2012

This is an interesting paper that both evaluates a chemistry climate model, and evaluates the impacts of ozone and methane changes on historical and future climate change.

The climate impacts experiments are novel and scientifically important (assuming they are robust). In fact I would have liked to seen these discussed further. The authors do nicely link the evaluation and experimental sections through the radiative forcing. Even so, they might want to consider splitting the paper. I worry that the really interesting later parts get a bit lost amongst all the evaluation.

The evaluation is very comprehensive and covers many aspects. However it is not

always easy to tell what the overall message is. It would be useful if the model performance could be related to others too, maybe using some of the CCMVal diagnostics and ACCMIP comparison (e.g. Young et al. submitted). This would help add some perspective. e.g. where a disagreement with observations is found, is this the same for most models or is E2 an outlier? Hopefully over time the community will converge towards some common diagnostics for model evaluation papers. Are there aspects of the evaluation that are particularly new and innovative? If so, maybe these could be emphasised more.

Section 2.1: I'd suggest removing the description of E2-H as it is hardly used in the evaluation.

Section 2.2-2.3: It would be useful to have slightly more description of how the chlorine and bromine compounds are derived from the single CFC tracer.

Page 23518, line 23. Note HadGEM2 has not submitted any of the data from interactive methane runs.

Page 23519. In tuning the wetland emissions, the authors are implicitly assuming that all other emissions and loss rates are correct. This should be made clear.

Section 3.1.4. The authors could compare the ozone metrics and budgets here against previous studies (particularly multi-model ones).

Page 23534, lines 1-14: Do the NO2 biases (both low and high) correlate with ozone biases? If so, does the NO2 cause the ozone bias, or do both have a common cause (e.g. transport issues).

Page 23534, line 25-28: In the next section the suggestion is that it is VOC oxidation rather than methane that contributes to the upper trop CO. It would be useful if the authors could diagnose the main sources of upper trop CO in their model

Page 23435, lines 3-6: The NO2 might also be contributing to the ozone bias.

Page 23538, lines 17-23: It would be useful to see these trend comparisons in a graph rather than a table. Are the results here similar to Lamarque et al. (2010)?

Page 23539, line 3: "For comparison with other studies ..." it would be useful if this section presented some comparison with other studies here rather than leaving it as an exercise for the reader.

Page 23539, line 27- page 23540, line 2: Has this difference in variability between coupled and uncoupled ozone been seen before? If not it may be worth emphasising this more.

Page 23541. How similar are the setups for the GISS and NCAR RTMs? The descriptions read very differently, but I assume they are doing similar calculations?

Page 23545, lines 4-6. This sentence needs rephrasing. I assume this means that the changes in background state increase the iRF by 0.05 W/m2?

Page 23547, lines 1-3. Does this RF/iRF ratio also apply separately to the LW and SW? If not, is there a reason to believe the total forcing ratio is more appropriate than the separate LW and SW components?

Page 23548, lines 14-15. I don't quite understand the relevance of comparing iRFs between the current version and AR4.

Page 23548, lines 22-24. The cooling might speed up ozone loss in the polar regions (in the absence of circulation changes).

Page 23549, lines 20-22. Why are the MAGICC results different for 8.5? Does this imply a different sensitivity of ozone production to methane concentrations?

Page 23549, lines 24-26. I assume the polar superrecoveries are due to increased transport.

Page 23556. The heterogeneous and non-local responses discussed here, seem in contrast to the smoother patterns discussed in Shindell et al. 2010. Does this imply

that the 2010 conclusions have been superseded? If the ozone RTPs from Shindell and Faluvegi 2009 were used to calculate zonal mean responses, would they agree with the full model calculations (as in Shindell 2012)?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23513, 2012.