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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.

Anonymous Referee #2

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This is a very comprehensive and worthwhile study. The authors have improved their climate model by including chemistry related to stratospheric and tropospheric ozone, and chemistry for methane, with natural emissions from wetlands responding to changes in climate. These are important improvements in moving their climate model toward a more fully coupled model. The paper describes in great detail the comparison of the model against observations, and then uses RCP projections into the future to view future radiative forcing and climate change, focusing on ozone and methane.

This is an excellent paper and I think that it should be published after minor revisions, responding to comments below. I will also mention that there is enough material here for more than one paper, if the authors wanted to focus separately on methane, strat ozone, and trop ozone, for example. But having these all together in one paper empha-

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Interactive Discussion

Discussion Paper

sizes how these are interrelated, and that is certainly acceptable – there is no need to rewrite.

A general comment is that while the whole paper is very well-written, I did not think the conclusions section was well written. The conclusions have several generalities that don't do much to summarize what was actually in the paper. The abstract is much more specific (and I think better). Given that the paper is so long, and addresses so many ideas, I think that the reader needs some guidance to interpret which are the more major points and how do they all fit together. In particular, I think it would be helpful to say what the major model improvements were, and quantify their importance. In this sense, I'd hope that the conclusions would read more like the abstract, but with more specific details.

A second general comment is that changes in OH or HOx in the past or future are not presented. This has bearing on the lifetimes of CH4 and O3. You might consider presenting these changes as a way of helping to explain some of the ozone and methane changes in the model.

Specific comments:

p. 23514, l. 10 – there is something wrong with this very long sentence at "long quantitative" it seems that there are two sentences here.

p. 23519, l. 28-30 – I assume that in the future no methane emissions are assumed from permafrost or methane hydrates. This limitation could be pointed out.

p. 23521, l. 18-22 – Many in the field are now using the ozonesonde climatology from Tilmes et al and the authors might think to compare their ozonesonde observations with those.

p. 23537, l. 5-7 – Is there a more recent estimate of methane chemical lifetime based on methyl chloroform or other method?

p. 23542, l. 8-22 – I was wondering here whether this ozone RF was with respect to the

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preindustrial or relative to no ozone. I was also wondering what total RF to compare the errors (in W/m2) to. The answers to both questions came on the next page, but consider reorganizing this section so that is clear up front.

p. 23545, l. 22-26. The last sentence “Hence . . .” I don’t follow how that sentence follows from the previous sentences.

p. 23548, l. 22-24. You say that colder stratosphere slows down ozone destruction. But we have an ozone hole over the Antarctic and not elsewhere because of cold temperatures and reactions on polar stratospheric clouds. So I would expect that a colder stratosphere would destroy ozone more quickly. I’m probably wrong, but it would help either to mention the reactions you’re referring to, or to reference other work that shows this clearly.

p. 23549, l. 20-21. If you know what the shortcoming of MAGICC is, it would be worthwhile to state it.

p. 23551, l. 5-10. I assume that when you calculate future wetland emissions, you are not assuming changes in land use / hydrology that might affect wetland area. That could be more explicit.

p. 23552, l. 8-22. Through here, discussion of OH and HOx changes would help. The authors speculate that temperature isn’t affecting methane oxidation much because it doesn’t show up at the end of the century. But that could be shown by looking at OH concentration and quantifying the % change in average rate constant due to the temperature change.

p. 23554, l. 20-23. The authors mention the methane/OH feedback, but should report the feedback factor they assume. Also, it is not clear to me that it is a good assumption that this feedback factor is constant through the next century.

p. 23555, l. 3-7. Use of the Ramaswamy equation here reminded me that CH4 forcing has a relationship with N2O forcing. I don’t remember that the authors made clear how

$N2O$ was changing in the model, and its possible effects on stratospheric ozone or methane forcing.

Figures – the text on many of the figures appears very small when printed in this format.

Fig. 12 – not clear to me why the vertical scale for forcings should be arbitrary.

Fig. 13 – These are standard deviations, but among what population of values? Is it the stdev among 10 annual values?

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