Atmos. Chem. Phys. Discuss., 12, C12583–C12589, 2013
www.atmos-chem-phys-discuss.net/12/C12583/2013/

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.

D. T. Shindell et al.

dshindell@giss.nasa.gov

Received and published: 12 February 2013

We thank the reviewer for their constructive comments on our paper. Those comments are in italics below, while our replies follow in standard font.

Anonymous Referee 2

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 emphasizes 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.

We have greatly revised the conclusions to remove some of the generalities and provide a clearer summary of many of the key results from the paper, including quantification of model improvements, as suggested.

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.

Figure 22 has methane chemical lifetime, and in the discussion of that we have added values for the changes in OH over time, as suggested. We have also added additional reference to the paper of Voulgarakis et al on future OH trends. In particular, that paper explores the sensitivity of OH changes in the GISS model to a variety of factors, and

presents time evolution of OH, stratospheric ozone, and J(O1D) and discusses those in some detail, which we reference rather than repeat here.

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.

This was indeed supposed to be two sentences, thank you. It is now corrected.

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.

This is correct, and we have added this to the text.

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.

This is a reasonable suggestion, and we intend to move to that dataset in the near future, though at present we would like to maintain the current analysis as it allows us to compare easily with our older model versions as documented in previous papers. The Tilmes et al dataset uses largely the same sonde measurements as in the Logan et al and Thompson et al datasets we use, so we would not expect vastly different results, but the regional grouping developed in Tilmes et al could be informative, complementing the regional comparisons we present against satellite data.

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

We have updated the text to compare with the recent comprehensive evaluation in Prather et al. (2012) that is based on multiple sets of observational evidence (including the methyl chloroform data).

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

We agree that this paragraph could be improved by clarifying these points earlier, so have added a statement that these forcing values are not diagnosed relative to either preindustrial or zero ozone but are the forcing bias due to model/TES ozone differences. We have also added the total iRF from tropospheric ozone for context with the errors.

p. 23545, l. 22-26. The last sentence "Hence : : :" I don't follow how that sentence follows from the previous sentences.

We've revised the last sentence of this paragraph to more clearly follow from the previous text.

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.

We have revised this description to clarify that the colder temperatures slow down ozone destruction outside the polar regions, and that the extrapolar ozone burden dominates the total stratospheric amount (in response to both reviewers' comments). Polar ozone is discussed further at the end of this section.

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

While it is difficult to determine exactly what relationships between changing temperature, water vapor and constituents were included in MAGICC, it seems likely that the differences in RCP8.5 ozone may stem from differences in the methane concentration

itself (see figure 22) as well as potential differences in ozone production chemistry at large methane concentrations. But as we are not certain about this, having no diagnostics of the MAGICC simulations used in the RCP forcing estimates, we would prefer not to speculate on this in the paper.

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.

Agreed, we've added a statement on this in the text.

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 temperature change.

We have added the OH changes in each RCP to the text, as suggested. We also include values for the influence of climate change (temperature as well as changes in water vapor due to warming), and having quantified these changes we now show that warming causes decreases in lifetime that are outweighed by the reduction in OH seen in the RCPs (except for RCP6.0, under which these effects are roughly in balance).

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.

We thank the reviewer for raising this point. We had used the IPCC TAR methane feedback factor, but as the feedback factor in the GISS-E2-R simulations did indeed vary in time we now use the actual value for 2100 from the GISS simulations (as diagnosed from a methane perturbation simulation described in Voulgarakis et al., ACPD, 2012). That is now stated in the text, and reported values are somewhat higher as the feedback strength increased over time.

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.

We described in section 2.3 on anthropogenic emissions that N2O concentrations are prescribed at the surface level following past observations and RCP projections for the future. We have added a comment that the Ramaswamy forcing calculation includes the effects of changing N2O on methane forcing. We've also added a comment at the start of section 5.3.1 noting that while projected N2O increases would reduce future ozone, the effects of projected decreases in chlorine and bromine containing substances is greater.

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

We agree, and will try to work with the journal to ensure that the final figures are in a larger format when possible.

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

The point of the figure is to show the correspondence in time between the ozone and temperature responses and the imposed forcings. This is easier to do when showing the solar and volcanic forcings on different scales, as the solar is much smaller but has a substantial influence on the stratosphere (as there is enhanced variance in the UV). Hence we believe it could be confusing to add third and fourth vertical axes to the figure or to add just one more but have to scale one of the forcings, and the added value to the reader would be minimal.

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

We now start the discussion of variability in section 4.2, where Figure 13 is introduced,

by stating that we use all ensemble members for the model, so that we maximize our statistics, and simply available years for the observations.

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