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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global sensitivity analysis of chemistry-climate model budgets of tropospheric ozone and OH: Exploring model diversity" *by* Oliver Wild et al.

Anonymous Referee #2

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This is an interesting paper where "identifying and quantifying the main causes of diversity in current models" is attempted through Gaussian process elimination. Three models are analyzed for tropospheric methane lifetime and ozone burden: the FRSGC/UCI CTM, the Goddard Institute for Space Studies Global Climate Model, GISS GCM, and the Community Atmosphere Model with Chemistry, CAMChem. The GISS GCM shows a rather different sensitivity to methane oxidation than the other two models, although the reasons for this are never clearly articulated. The sensitivity in the different models can explain some of their different responses to global change over the next century.

Overall, while this paper demonstrates a unique and potentially powerful analysis tech-

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Discussion paper

nique. However, I was not altogether satisfied with some of the analysis or in the end with the scientific depth of the paper.

Major Comments:

1. How were the key variables selected for the Gaussian process elimination? The selection of variables should be justified. For example, Holmes et al. (2012) (No, I am not an author on this paper) found that OH is most sensitive to the ozone column (at least when examining interannual variability). This was not included in the present study. This choice needs to be justified.

2. The selected range of the sensitivity variables is very important for the paper particularly when comparing the sensitivity of one variable against another. The paper states the ranges are loosely based on studies in the literature, but do not give the studies. To me, at least it is not believable that cloud optical depth varies could vary by 100, or boundary layer mixing by a factor of 10,000. Of more concern is that humidity varies globally by 50%. This is in fact very large and colors the results and conclusions throughout the paper. In contrast, Holmes et al (2012) gives a variation of 3% in humidity. While the variability might be calculated differently in the two studies, a value of 50% seems huge. Would the range in variability be better quantified by comparing across the models? I also do not believe it is reasonable to take the variability from the smallest and largest values in the literature as this does not likely capture the likely error in model simulations. In any case, the sensitivity and ranges in variability need to be better quantified as this impacts much of the interpretation in the paper. An arbitrary specification of the range in these parameters would seem to imply that the resulting comparison of the different sensitivities is also arbitrary.

3. Comparing the sensitivities across models only makes sense if the model forcing is similar. (For example, the sensitivity of any one model might be very different when comparing between present day conditions and pre-industrial conditions). The authors need to show that the tropospheric forcing in the three models is roughly the same

(CH4, NOx emissions, tropical ozone column etc). In particular I am concerned about the tropical ozone column and perhaps more importantly the tropical photolysis rates. Might this explain the large difference in the methane oxidation rates between the different models? At any rate quantifying the mean differences (at least in a supplement) seems important in better understanding the results.

4. The paper does not really pinpoint some of the basic causes of the discrepancies between the models, in particular the different lifetimes of methane. As stated in the previous comment some of the mean fields in the models should be given, for example the global burden of CH4, atmospheric water vapor (perhaps with latitude?), etc. While the sensitivity tests between the different models is revealing evaluating the mean difference between the GISS-GCM and the other models may also be revealing. In a number of locations the authors hypothesize that the difference is due to differences in humidity (lines 177-178, Pages 8 and Page9) or due to differences in the formation/decomposition of PAN. Why don't the authors check? I'm not suggesting a lengthy analysis, but an inspection and comparison of some of the mean fields should be sufficient to check some of these hypothesis and possibly reveal some of the key model differences.

Minor Comments

1. Please state in the caption to Figure 1 that these results are over different years with different emissions and meteorologies. Also please reference Table 1 in the Figure caption where the references to the points can be found. Finally, please state what the dots refer to in the right hand panel.

2. The methodology behind the Gaussian process elimination should be explained in more detail in methodology section. While I don't expect the authors to go into detail, neither should it be necessary to reference the referenced papers to understand this analysis.

3. Does the sensitivity to boundary layer mixing simply involve changing the vertical

diffusion coefficient over the boundary layer depth, or something else?

4. The paper states: “The models differ in their sources of meteorology”, but certainly the sources of meteorology are important. Are the runs using specified dynamics or GCM generated meteorology? This might be quite important to how well the relative humidity is specified. In addition, using a single year might also introduce significant variability between the models. Please say something more specific about the meteorology used as well as comment on the possible importance of the interannual variability. My guess is the latter might introduce significant variability between the models.

5. Page 5, line 15. “but we scale”. It is not altogether clear how this scaling is used. Please specify. Given the non-linear response to NOx, does a linear scaling make sense?

6. Page 7, line 139-141 “discrepancies highlight that uncertainty in chemistry and transport processes not considered here may play a substantial role in governing the CH4 lifetime”. I’m not sure I understand this conclusion. It seems it might simply mean, for example, that the variability in water vapor is considerably over-estimated.

7. Page 7, line 149 “it is notable that humidity has not been prescribed in previous model intercomparison studies”. Possibly, but again the large response in water vapor is dependent on the large variation in water vapor.

8. Page 8. The explanation in the future responses of the different models is nice and given in several places in the paper. Personally, I think this might belong better in the conclusion section.

9. Page 9, line 180 “suggesting a saturation in OH formation in this model”. How does a saturation occur?

10. More generally, I am curious about the conceptual difference between the sensitivity to forcing parameters (NOX emissions, stratospheric ozone column) and internal model parameters. To what extent does it make sense to distinguish between the sen-

sitivity between these type of parameters?

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