Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-863-AC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global sensitivity analysis of the GEOS-Chem chemical transport model: Ozone and hydrogen oxides during ARCTAS (2008)" *by* Kenneth E. Christian et al.

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Received and published: 26 January 2017

gensymb We thank the referee for their thorough review and helpful comments. Below are our responses to the referee's comments (*italics*).

1. Number of parameters chosen: The authors state in the conclusions that 52 parameters have been explored. In Table 1 I count 51. Have I missed something? Can the authors please check this.

There were 52 factors included in the HDMR analysis and 51 in the table. EPA NH_3

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should have been included in the table and has now been added. Thank you for finding this discrepancy.

2) Model resolution: The current simulations have all been performed at 4° x 5° horizontal resolution with the justification that the authors found only small differences (∼10%) using higher resolution model simulations (2° x 2.5°). These latter higher resolution simulations still strike me as being very low resolution. Do the authors expect the same sensitivity to hold at say 0.5° x 0.5°? I ask as I have seen more and more simulations with GEOS-Chem at these sorts of high resolutions and so I think transferring the knowledge gained here to those studies is important.

Response: We would have preferred to run this analysis at the finest possible resolution but are limited by computational resources. The comparison to the 2° x 2.5° was intended more to illustrate how sensitive the modeled results are to changes in resolution. This comparison between 4° x 5° and 2° x 2.5° has been used in previous GEOS-Chem studies (eg., Fiore et al., 2002, Fischer et al., 2014). The expectation is that most of the findings of this paper would be applicable to other model resolution choices considering the small differences between the two resolutions we tested. While finer resolution (like 0.5° x 0.5°) studies are becoming more popular with GEOS-Chem, these studies are limited to a few regions–E Asia, Europe, and North America. In the case of the North American domain, the nested grid doesn't cover the ARCTAS domain.

3) Meteorological uncertainty in the model: This is out of interest, but how different is the uncertainty between the average monthly fields between GEOS-4 and GEOS-5 compared to the standard deviation of the meteorological parameters generated from the re-gridding from the native GOES-5 grid to the GEOS-Chem grid?

Response: By averaging over the month, some of the day-to-day and some of the spatial differences are muted between the resolution choices. From some back of the envelope calculations between the 2° x 2.5° and 4° x 5° resolution meteorological fields, we find differences less than those between meteorological models with the greatest differences coming around the edges of mountainous regions and the edge of the Antarctic continent. Going even finer to the native resolution would presumably further increase these differences to being around the same or perhaps greater than the differences between the GEOS-4 & 5 models.

4) The role of organic radicals: It's interesting to see that the uncertainty in isoprene emissions pops up as having an effect on ozone and HO_2. I was wondering if the authors considered the uncertainty in the organic peroxy radical reactions associated with isoprene?

Response: All the chemical reactions in the GEOS-Chem chemical mechanism were included in the Morris Method pre-screen test including those involving organic peroxy radicals. The isoprene peroxy radical reactions did not make the cut to be included in the HDMR analysis but some of the methane ones did as shown in Table 1.

5) NO_x: There is very little mention of the role of NO_x in the manuscript and I'm surprised that the authors did not include NO_x in the analysis and results. Clearly NO_x plays an important role in coupling HO_x and I would like to see how the current study impacts the NO_x partitioning. I think that this is something that many others would also benefit from seeing and I would suggest adding some plots to at least show the impact of the ensemble of simulations on the NO_x profiles.

Response: NO_x profiles were not originally included in the paper for a couple of reasons. First, this analysis didn't change the model treatment of NO_x (except for the perturbations to emissions and chemical rates) making most of that analysis a rehash

of previous research. Secondly, a few different NO_x emissions inventories were perturbed in the analysis leading to a large variability in their modeled concentrations between model runs, especially near the surface where the emissions sources are. As you note though, readers would be interested in the seeing at least the NO_x profiles. To address this, we have created a small supplement showing the median NO_x and CO profiles for both the spring and summer flights.

Changes: We've made plots showing NO, NO_2, and CO profiles for both ARCTAS A and B in a supplementary file (Figures S1 and S2).

6) Normalised sensitivities: It's not clear to me if the reason that HO_2 uptake is the most sensitive parameter is owing to the fact that it has the greatest uncertainty? Can the authors comment on the use of the method in distinguishing/determining normalised sensitivities?

Response: As far as the HO_2 uptake uncertainty, please refer to my response to referee # 1, general point # 3.

The HDMR method is not necessarily used to determine normalized sensitivities, however one could infer a qualitative sense of this comparing those factors in the pie charts to their respective sensitivities listed in Table 1. We touched on this in a peripheral sense noting the sensitivity of the oxidants to both the chemical kinetic rates (which have much lower uncertainties) and emission inventories (P3, L20-24). Due to the non-linearity of the chemical system, I have reservations with creating "normalized" sensitivity indices armed with the sensitivity indices and uncertainty factors though.

Technical corrections:

Page 2 line 4: I don't think Wu et al., 2007 is a great reference for making this point. A

better reference would be a multi model intercomparison study like one of the ACCMIP or HTAP papers.

Response: That's a good suggestion to use a multi model inter-comparison paper to make this general point. Instead of the ACCMIP or HTAP papers, we've edited this reference to the POLMIP paper as it's also Arctic focused.

Changed the reference to Emmons et al., 2015

Page 9 line 13: ppb should be ppt I think.

Correct. Changed as suggested

Page 9 line 22: Need to define HO_x earlier in the text.

HO_x was defined on Page 2, Line1. No changes

Page 10 line 10: ppb should be ppt I think.

Correct. Changed as suggested

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