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Interactive comment on "Global sensitivity analysis of GEOS-Chem modeled ozone and hydrogen oxides during the INTEX campaigns" *by* Kenneth E. Christian et al.

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We thank Prof. Kasibhatla for his thorough review and thoughtful suggestions for improving the manuscript. Below are our responses to his comments (*italics*).

The focus of this paper is on an analysis of the causes of discrepancies between modeled and measured O_3, OH, and HO_2 vertical profiles during the INTEX-A and INTEXB field campaigns. The analysis is based on a global sensitivity analysis approach, in which an ensemble of model runs (in which multiple variables are simultaneously perturbed) is used to construct sensitivity factors to delineate the

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relative importance of the various variables considered on modeled tracer fields. This is potentially an interesting approach to understand observation-model differences, but the paper seems to fall short in fully exploiting the power of this approach and in terms of the analysis presented. I discuss my specific concerns below:

1. Section 2.2 presents a relatively technical description of the global sensitivity analysis approach and gives the impression that the advantage of the approach (relative to a local sensitivity analysis) is to examine the uncertainty in model results due to the joint uncertainty in multiple model inputs. However, the paper focuses solely on the calculation and analysis of first order sensitivity indices, because of the computational burden associated with number of model runs needed to estimate higher order sensitivity indices. This raises the question as to whether the calculated first order sensitivity indices are in fact meaningful, or whether they themselves could be uncertain owing to the truncation of the polynomial function (eqn 1) that is being fit. It also raises the question as whether there is any advantage of using the global sensitivity analysis approach itself. Given that only first order sensitivity indices are estimated, wouldn't it have been more straightforward to use a local sensitivity analysis approach?

Response: As for calculating the model sensitivity to all the various inputs, there would be small if any benefit to performing local sensitivity analyses. With ~30-50 inputs, it would still take hundreds of model runs to create polynomials, or a sort of regression, relating the model inputs to the outputs. Not all the model inputs are described in simple linear functions, many of the component functions of the RS-HDMR analysis are 2^{nd} and higher ordered polynomials describing the model output response to perturbations in each individual model input. Also, through a series of local sensitivity analyses, we would lose somewhat the contextualization of the different sensitivities to the model inputs. It is true that we generally find non-linearities to contribute a rather small portion of the total model uncertainty. This means that modelers wishing to determine the sensitivity of these modeled oxidants to one individual factor can likely assume these

factor interactions are small to negligible compared to their separate effects. This point it made clearer in the conclusions (P14 L31-P15 L2). One of the strengths in this method is that we don't assume linearity between factors and factor-factor interactions are accounted for.

We have confidence in the model sensitivities calculated from testing the sensitivity of the sensitivity indices to varying the number of model runs included. For example, calculating the sensitivity indices using 512, 448, 384, 320, 256, 192, and 128 model runs. As noted on P4 L28-30, we find little difference in the sensitivity indices calculated. This insensitivity to increased number of model runs, especially from 256 higher gives us confidence in these first order sensitivity indices. This result is similar to Lu et al., 2013.

Changes: Added a couple sentences at the end of P14 (P14 L31-P15 L2) to make note of the generally small contributions by factor-factor interactions in the overall model uncertainty.

2. Another potentially important issue pertains to the treatment of uncertainty of individual variables. Let me illustrate by focusing on the assumed uncertainty for biomass burning emissions. Presumably, the authors assume that this is a systematic (as opposed to random) uncertainty so that in any given model run, the sampled uncertainty factor is applied uniformly in each and every grid cell of the model. Is this in fact appropriate? Or would it more appropriate to assume that some portion of the uncertainty is random? Also unclear is how inter-species uncertainty correlations are handled. For example, are CO and NO_x biomass burning emissions perturbed by the same scaling factor in every grid cell in a given run (which would occur if the uncertainty was solely due to uncertainty in burned area for example) or are the perturbation factors completely independent (which would occur if the uncertainty was solely due to uncertainty in emission factors for examples)? As another example, are the perturbation factors for the photolysis rates correlated or uncorrelated? The

authors should describe more clearly their approach in selecting perturbation factors and the justification for the approach they use - and discuss how their choice might impact their conclusions.

Response: We treat all the perturbations for each factor independently from the others. There are a few reasons for this. The uncertainties we use for the emissions inventories largely come from the differences among different studies. Thus, this uncertainty is more closely a measure of systematic uncertainty in these emissions inventories. In the case of biomass burning emissions, as the reviewer notes, there are uncertainties in parts of this process that would affect all the emissions similarly (i.e., area burned, elapsed time of burn, etc.) but there are also uncertainties in other parts of this process that would not affect all emissions similarly (i.e., land cover, fuel type, temperature of burn). We felt with these uncertainties in the uncertainty it made more sense to treat all the emissions factors individually and separately allowing for us to determine the specific species responsible for model uncertainty. While the uncertainty in these biomass emissions are largely a function of processes that affect all the biomass emissions, like area and elapsed time of burn, there are still uncertainties in the partitioning of these emissions into various specific trace gases (Andreae and Merlet, 2001; van der Werf et al., 2010). Treating the emissions separately also allows for us to determine the specific emissions or processes that are resulting in model uncertainty. Lumping the uncertainty would lose some of these insights. It is not immediately obvious how perturbing some of these emissions factors in concert with one another would change the conclusions of this study. We already conclude that emissions factors form Asia, North America, and biomass burning are responsible for considerable portions of the total model uncertainty so grouping many of them together would presumably only serve to group their effects into a bigger piece of the pie.

The perturbations to photolysis rates are treated individually and systematically as well. These uncertainties come from the combined cross-sectional area and

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quantum yield uncertainties in the JPL evaluation cited. As these uncertainties in the cross-sectional area and quantum yield pertain to the individual chemical species, we believe this is the appropriate way to express this given that errors in chemical rates would affect the global troposphere similarly worldwide. The inclusion of cloud fraction as a perturbed model input would contain some of this combined photolytic uncertainty (at least for INTEX-B).

Changes: Added a couple sentences (P4 L7-9) noting the independence of the perturbations and the short rationale for this.

3. There also seems to be a bit of a disconnect between the sensitivity indices shown in Figures 6-9, and the discussion of results in Section 3.3. For example, on page 13, line 1 the authors say that '. . .suggests that ..'. Why 'suggests'? Doesn't Figure 7 in fact make the case that uncertainty in lightning NO_x cannot solely explain the discrepancy in modeled O_3? More importantly, I am somewhat puzzled by the authors approach of using a subset of ensemble members to illustrate some of their points. Wouldn't it be more straightforward to make an additional set of model runs in which all the important identified parameters were appropriately perturbed (based on Figures 6-9) and to demonstrate that the the final configuration results in better statistical agreement with observations?

Response: In our analysis we try to be restrained in our language and conclusions. The reviewer is correct in noting that any way we perturb the lightning NO_x in the model fails to bridge the model-measurement gap, especially in the higher altitudes of the Houston INTEX-B flights (Figure 11). The reason we were not more confident with our language in this discussion is that there remains uncertainty in the way lightning NO_x is parameterized and handled, especially in global models like GEOS-Chem. We note in our conclusions that there are some different lightning NO_x parameterizations

and treatment in the works. Our tests only varied lightning NO_x within the context of the existing parameterization which leaves the possibility that lightning NO_x, or specifically its parameterization, could be still be the culprit.

Also worth noting is that model sensitivity does not necessarily mean that the default treatment of that factor is "wrong". In some cases, we have found sensitivity to a factor and the best matching ensemble members had values close to the default model values. Just because a factor takes up a large piece of the pie charts in Figures 6-9, doesn't mean that that factor is "wrong".

As for the second half of the point raised, we do not see a great value in creating runs containing all the best matching perturbations and think that this could be ripe for misinterpretation. The perturbations producing the best model-measurement agreement for one of the oxidant species studied do not necessarily produce the best model-measurement agreement in the other domains or among the other oxidants in the same domain. This lack of predictability limits the usefulness in creating these "improved" model runs. Rather the purpose of this section is to highlight processes that may be systematically misrepresented in the model (like gamma HO_2, lightning NO_x) and stimulate discussion for the others.

We included the model runs with lower NO_x emissions due to the persistence of better model-measurement agreement with lower NO_x emissions and to compare these results to those of the Travis et al. (2016) paper.

4. A minor comment - I think some thought needs to be put into making the presentation more appealing. Much of Section 3 describes in detail various aspects of the figures that are obvious by simply looking at the figures, rather than highlighting the most important aspects of the results.

We feel that we do highlight in these sections the important aspects of the results in

these sections. While it can be a bit repetitive in describing some results contained in the figures, we felt it was needed so the reader would know the model uncertainties in the profiles and specific sensitivity index values for the pie charts. Throughout the section we make note of the interesting/main picture elements and further expand upon these takeaways in the discussion of the results.

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