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Interactive comment on "Impact of Intercontinental Pollution Transport on North American Ozone Air Pollution: An HTAP Phase II Multi-model Study" *by* Min Huang et al.

Anonymous Referee #2

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This paper represents a huge task, assembling and comparing the results from the multi-model HTAP2 study. It is a brave undertaking.

However, I do have some concerns about what was learned in the process. I believe the stated goals of the paper are not well met or met in a cursory fashion. There are a number of inferences stated as fact but not in fact proved. In some cases more analysis seems to be needed. In other cases a clearer explication of what has been learned would be helpful. Recommendations about future work are succinctly summarized, but the paper needs to be stronger in detailing what was learned and in justifying the methodology used.

Major Comments: I) The stated paper goals are to address: "1) the differences in O3

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Discussion paper

sensitivities generated from the HTAP2 and HTAP1 experiments to help address how the LRT impacts on NAM changed through time; 2) how the multi-model approach, as well as the refined model experiment design in HTAP2 can help advance our understanding of the LRT impacts, especially the benefits of increasing the global models' resolutions and involving the regional models; 3) the usefulness of satellite observations for better understanding the sources of uncertainties in the modeled total O3 (e.g., from the emission and regional models' boundary condition inputs) as well as for reducing the uncertainties in some of these model inputs via chemical data assimilation."

As the paper stands it is not clear if it achieved its goals. The answers to these questions should be clearly articulated in the conclusions and in the body of the paper itself. In particular:

1) Between HTAP1 and HTAP2 models have changed, emissions have changed and the transport has changed. So it is not really clear how the sensitivity changed through time. The authors suggest many of the changes are due to the changes in emissions, but this remains to be proven. The authors could determine if the changes in the sensitivities are consistent with the change in emissions by using the HTAP1 emissions and the current sensitivities (delta O3/delta emissions) to determine if most of the changes from HTAP1 are consistent with emission changes. However, as it stands the first goal of the paper cannot be met without substantially more analysis.

2) It is not clear how this study enhanced our understanding of LRT nor is it very clear how changes in model resolution impacted the solutions. The STEM model resolution is 60x60 km, actually rather comparable to a global model of 1o resolution (about 85 km at 40o N). While there is a wide range of different resolutions in the global models it is unclear how this paper really explored the impact of resolution on the results. What aspects of LRT did the paper enhance? This should be clear in the paper.

3) The usefulness of satellite data is essentially a "motherhood" statement. It is somewhat unclear how this paper further showed this usefulness. This is especially true

since the case study using satellite data was presented in a rather cursory manner.

II) It is not clear what the goal of using the STEM model is here. As pointed out above the resolution is not that much higher than some of the global models that give the boundary conditions. Differences between the STEM results and the boundary condition model could be due to the different chemistry in the two models or due to the differences in transport. Driving the models with different meteorological datasets also risks an inconsistency in the boundary conditions (e.g., chemical plumes transported in the jet in the parent model might be mismatched with the jet in STEM). At any rate the rationale for the use of the STEM model should be clearly articulated. What did we learn by coupling the global models with the STEM model?

III) The case study is rather thin. What are the goals of this section? This section should either be expanded or dropped.

Specific Comments: 1. L42. The sentence beginning is rather awkward. Consider rewording.

2. L48-49, "This indicates. . ...". This has to be proven. As is well known interannual variability of the atmosphere is substantial.

3. L175. Starting here the manuscript goes into considerable detail about how the simulatons are set up. This does not work well in the introduction, but belongs in the methodology section.

4. L202, Section 2.1. The manuscript parses the emissions between East Asia, MICS Asian regions and south Asian countries. The domains of each these regions is not clear.

5. Table 1. All abbreviations should be defined. Also the table headings need to be reformatted.

6. L250-253. This notation is should be improved: the left hand side of the equation has a percentage sign, but not the right. I would suggest something like EASALL(-

20%) on the right to distinguish this from the R(O3, EAS, 100%) where presumably all EAS emissions are reduced by 100%.

7. L290 and following paragraph. A long discussion is presented concerning STEM lightning emissions, biogenic emissions and VOC speciation. How were these emissions parameterized in the other models, the same as STEM or differently? Please specify more thoroughly differences in emissions between STEM and other models.

8. L394. "less sensitive" – less sensitive to what?

9. L420. "de-stripped" – the meaning is unclear.

10. L459. "suggesting that using". This seems rather speculative. There are many possible explanations.

11. L471-472. "overall there does appear to be a positive bias". This seems to be a rather strong statement considering the previous sentence. It would be better to say satellite is consistent with a positive bias.

12. L478-479. Can you provide a reference why co-emitted species are likely to be biased in the same way as NOx. It is not at all clear to me that emission factors would be all biased in one direction.

13. L509-510. "mainly due to". Maybe. It would be better to say consistent with.

14. L556-557. Did you show this? Probably better to say "consistent with".

15. L567-568. This is an interesting result: that R in HTAP2 is larger than in HTAP1. However, the reasons for this have not been clearly shown. Certainly the difference is consistent with emission trends but the authors need to establish that this is the case (see general comments above)

16. Figure 9. I think this is a scatter plot of R(MDA8,EAS,20%) and R(O3, EAS, 20%). Please address the notation.

17. The point of section 3.3 is not clear. Some of the figures panels in this section seem to be referred to in a very cursory manner or not at all (e.g., Figure 11). This section needs to be much better developed or not presented.

18. Figure 7 caption. I assume (a), (b), and (c) refer to the first three rows. Better to say row 1, row2 and row 3 or label all panels with letters.

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