

***Interactive comment on “Transpacific transport of ozone pollution and the effect of recent Asian emission increases on air quality in North America: an integrated analysis using satellite, aircraft, ozone sonde, and surface observations” by et al.***

**Anonymous Referee #1**

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This paper gives a very nice overview of the impact of the effect of Asian emissions on U.S. ozone levels using a number of measurement platforms and the GEOS-chem model. It makes a nice contribution to the literature and should be published. The comments below should be addressed before the paper is published.

1) Simulated ozone off the west coast of N. America seems to underestimate the aircraft measurements by about 5 ppbv, which the paper attributes to an underestimate in

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the input of stratospheric ozone. This is probably a reasonable explanation, considering that the model uses the Synoz parameterization. Model underestimates of 10 ppbv are seen at Richland. However, GEOS-Chem seems to reproduce the ozone record at Mt. Batchelor (MBO) without any significant bias. The discrepancy between the model-measurement comparison with the ozonesondes and aircraft measurements versus Mt Batchelor is not discussed. Presumably if the STE of ozone was increased in GEOS-chem (to reconcile the model with the aircraft and ozonesonde measurements) this would significantly impact the simulation over MBO. I would guess as a result GEOS-chem would significantly overestimate the ozone at MBO. This would seem to imply the estimated 9 ppbv contribution from Asia is too high. If the authors understand how to reconcile these set of measurements it would be helpful and would strengthen the paper. If not, this discrepancy should be stated and the strong conclusion that Asian pollution contributes 9 +/- 3 ppbv of ozone at MBO should be explicitly tempered by the fact that model underestimates ozone by 5-10 ppbv in other nearby locations.

Other Comments. 1) Page 8152, line 25: *495 Tg ozone y-1* should read approximately *495 Tg ozone y-1*, as Synoz only constrains the ozone flux to the specified stratospheric input over a long-term time average. Interannual variability may occur, although its amplitude is constrained.

2) I found the discussion on emission changes rather confusing and had to reread it to make sure I understood what is going on. I think the following points would help to clarify the discussion and interpretation. 1) I think it would be helpful, if the authors would explicitly name the emission inventory that you are using, e.g. S2006(prime); (Then the various emission inventories could be explicitly named on the bottom of 8152). 2) Then, if I understand correctly, S2006(prime); is the same as S2006, but with the NO<sub>x</sub> emissions equal to twice those of S2000 over East Asia (20-50 N and 100-150 E). It would help to explicitly state this on page 8154, as well as the countries over which you are doubling the anthropogenic emission inventory (i.e., at least over China, Japan and S. Korea). Then if I understand correctly you are attributing the factor of 2

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emission increases over China with growth in emissions, but over Japan and S. Korea to low emission estimates in S2000. 3) Thus, only approximately 80

3) Page 8154, line 23: Are the results of Jaegle and Wang consistent with your assumptions? Their estimates suggest a 30-50% emission increase is reasonable; the author's emissions are 2-3x what they report. Can the authors clarify?

4) Page 8155, line 15: *likely because of an OH overestimate*. This seems a bit presumptuous. Certainly your emissions of CO are also rather uncertain. *consistent with an OH overestimate* would be a better way to phrase it.

5) Page 8156, line 5. You seem to be assuming an error in the measurements because NO/NO<sub>2</sub> in Geos-Chem is in close agreement with the NASA Langley photochemical model. Is it not just as likely that the photochemical model is also missing some important process or input? Or does the 15% error in HO, HO<sub>2</sub> in that NASA model lead you to believe this is unlikely. Please clarify.

6) On the bottom of page 8156 you suggest the model error in ozone is due to an underestimate in STE. Yet, by filtering out stratospheric air one might think you have minimized this error. Please clarify?

7) Page 8157, line 20: Is the decreasing trend in CO during April and May also attributable to the increase in OH?

8) Page 8157, line 26: was event 2 also seen at MBO?

9) Page 8158, line 12. Is the correlation of 0.5 significant?

10) Page 8158, line 23. This north-south split in the plume has also been noticed in transport to Hawaii (e.g., Hess and Vukicevic, JGR, 2003).

11) Page 8160, line 4: *The ozone production in the southern branch is relevant for impact on the United States*. It might be better to say something like the *direct impact*. Much of the southern branch gets shunted into the subtropical boundary layer where

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it is subject to rapid photochemical destruction; it may be that slow subsequent ozone production from the Northern Branch might be indirectly important on longer temporal and spatial scales.

12) Figures 11 and 12 show two maximums in ozone production, ozone and NO<sub>x</sub>. The maximum in the central Pacific was examined by Hess and Vukicevic (JGR, 2003) in relation to the MLO experiment on Hawaii. They also showed the importance of PAN decomposition in descending anticyclonic air and the subsequent ozone production.

13) The increase in ozone attributed to increases in Asian emissions of 3-5 ppbv is likely too high by 20% (assuming S2000 is 20% too low in their estimated NO<sub>x</sub> emissions over Japan and S. Korea). Thus, an estimate of 2.4-4 ppbv is probably more reasonable. This should probably be pointed out.

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