

## ***Interactive comment on “Transpacific pollution transport during INTEX-B: spring 2006 in context to previous years” by G. G. Pfister et al.***

### **Anonymous Referee #3**

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The article titled “Transpacific pollution transport during INTEX-B: spring 2006 in context of previous years” is an important study that makes the INTEX-B special issue of ACP more complete by connecting the results from a short campaign to overall state of the atmosphere. Since aircraft campaigns deliver only sporadic data, it is important to assess representativeness in time and space of that data. This is what the article aims to do. The approach of the paper is very thorough, spanning aircraft, satellite and model derived CO concentrations. Below are my specific comments.

1. p17819: proper citation of past literature. There have been a lot of articles in recent years (after 2004) on transpacific transport and none are mentioned. Reviewer 1 suggested a few, but left out Zhang, L. et al. 2008, Zhang, L. et al. 2009 and at least few others.

2. p17820, line 9: “is destroyed by oxidation of OH” should be changed, since it’s not OH that is oxidized.
3. p17821, lines 14-15: “limit the MOPITT data to daytime retrievals”. Could you comment on what bias that introduces? Especially since the positive aircraft bias is so nicely addressed in the paper.
4. p 17823: It would be great to see the number for annual global total CO emissions, for reference. No need to add it to the table.
5. p17825, line 1: “monthly mean MOPITT averaging kernels”. I think the need for such coarse monthly approach should be explained in the text, since the default should be to use individual orbit averaging kernels.
6. p 17825, lines 12-17: I’m confused why isn’t the model sampled along MOPITT orbit and considered only where MOPITT data is available for monthly mean purposes mentioned here.
7. p 17825, line 24: Merritt et al. 2009 should probably be Deeter et al. 2009
8. p17826, line 12: “These are very similar to results when the satellite retrieval . . .”. I’m looking at Figure 3 and the variability of MOZART\_AK vs. MOZART\_noAK is opposite, so they don’t seem similar. Meanwhile, in Figure 3, the CO burdens in all cases look the same. Is this what is meant by “results”? It would be very helpful if the scale on CO burden was changed to be more meaningful. Alternatively, if the burdens are in fact all the same, then maybe there is no need to show them.
9. p17826, line 27-28: Is this range with or without the MOPITT drift?
10. p17828, line 27: “others” should be “other”

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