

***Interactive comment on* “Trans-Pacific transport of Asian dust and CO: accumulation of biomass burning CO in the subtropics and dipole structure of transport” by J. Nam et al.**

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Response to referee #1 Comment 1. Figure 4 and Figure 5. In these figures and the discussion of model bias for AOD it is not clear what simulations the author use. Do they use the ‘standard simulation’ or the simulation with SE asian BBx8? Please clarify in the text. Response: In the section, “Accumulation of CO over the Central Pacific”, the authors increased SE Asian BB emissions to investigate the sensitivity of column CO over the Central Pacific to this emissions. The manuscript showed the simulation results with SE Asian BBx8 only in this section (Figures 2 and 3) and the ‘standard simulation’ results in other sections (including Figures 4 and 5). In the new version of

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the manuscript, this has been clarified.

Comment 2. I am not sure that I follow the reasoning concerning a potential bias in the dust source location. It seems that dust bias exists for only 1 of the 3 events examined. Aren't the dust sources the same for all 3 events? Why would the dust source location be wrong for one event and not for the others? The meteorological argument makes more sense to me, as variations could occur among the 3 events. Response: The authors chose the first event since (both) dust signal (magnitude) and model bias are larger than the other two events (as indicated in p. 1365, lines 17-18). The dust source location is same for the three events, but dust emissions are a function of meteorological fields such as surface wind speed and soil moisture. Dust emissions simulated in the model were much less during the latter two events than the first event. For this reason, we did not rule out the possibility that dust locations may have problems.

Response to referee #2 General The conclusions state, with respect to AOD, “ The resulting impacts on the simulated source-receptor relationship of pollutant transport from Asia to North America are large” but I don't see this demonstrated or quantified. What is the impact of including the improved S.E. Asian CO emissions in the model on air quality in the U.S? What are the implications on aerosol concentrations in North America or elsewhere for that matter, of a bifurcation point in the North Pacific flow? Is this typical of the springtime flow over the Northern Pacific? Presumably, whether North America experiences an Asian dust episode or not could depend crucially on such bifurcation in the large-scale flow over the Northern Pacific. The paper brings up some interesting issues, but the limited time period considered hinders broader conclusions. To what extent are biomass burning emissions in GFED problematic beyond April 2003, and S.E. Asia? What do the authors recommend to address the “challenge on the modeling capability to simulate the transport pathways of pollutants across the Pacific” (p. 1369, lines 17-18)? Does the chaotic nature of large-scale transport place an inherent limit on our ability to simulate long-range transport (likely) or are high-

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quality met. Fields the answer? Response: With the paragraph (p. 1369, lines 14-16), the authors tried to stress the importance of the location of the dipole structure in the source-receptor relationship. The paragraph has been modified to clearly state this point by changing the verb from “are” to “could be”. The impacts of including the improved S.E. Asian CO emissions on column CO in the Pacific and the U.S. can be seen in the fourth column of Figure 2. Per suggestion by C. McNaughton in his Short Comments, we looked at the impacts on simulated CO at an ESRL surface site (Trinidad Head, at 41°N). The enhancement by the improved CO inventory was less than 2 ppbv. This is because the biomass burning plume from S.E. Asia affected mostly the southernmost part (30°N) of the U.S (Figure 2). In May 2003, the bifurcation of the flow is observed, although the bifurcation point occurred different latitude. As the referee mentioned, the location of the bifurcation point determines whether the Asian dust affected U.S. or not (Canada or Mexico), which is reflected in our qualitative statement on its impact on the source-receptor relationship. We used biomass burning emissions of CO, OC, and EC in GFED for spring 2003. Only CO emissions during April near S.E. Asia appeared to be problematic, although this is not a study focusing on evaluating the quality of the GFED inventory. The emphasis on the CO section is to explain the enhanced CO in lower latitudes in May. The reviewer raised very good questions on the broad implications on this study. We think there are two areas. We showed that model simulated trans-Pacific transport must be evaluated (if possible) before one can begin to understand how transport from East Asia may affect air quality in the United States. In this study, we did not do as well in the second area, which is how often the bifurcation occurs and for what reasons. One issue is if satellite observations can be applied to assess the climatology of transport pathways. We stated in the introduction the reasons why we only focused on the events in which we believe that the signals in satellite measurements are unmistakable. It would be a different study to try to quantify the limits in which satellite observations can be used and it is only the first step to address the reviewer’s questions. Although we did not do such analysis, it appears to be an obvious extension from this work for the readers. The questions that

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the reviewers raised are important. They are related to the long-term (e.g. interannual) variability of trans-Pacific transport, which we know is large. They cannot be addressed quantitatively in this paper.

Details: 1. A number of times when the manuscript gets overly wordy, e.g. p. 1360, lines 16-21. Also the content of remainder of the paragraph, p. 1360, line 24 – p. 1361, line 5, could be stated more simply – the model shows a low bias with respect to MODIS, which is likely due to a known high bias for MODIS in the presence of dust (Levy et al., Chu et al.). Also, I don't think it's a good idea to artificially reduce the values of observations. Identify the bias, and move on. Response: The sentence (p. 1360, lines 16-20) has been separated into two shorter sentences. The sentences (p. 1360, line 24- p. 1361, line 1) have been revised as suggested. The authors show the original MODIS AOD (without reduction) in the new version of the manuscript.

2. p. 1364, line 12: How do you use MODIS to “constrain” the dust transport? Asses, yes, but constrain? Response: The authors changed the word from constrain to assess.

3. p. 1359, line 26: “We employed the mobilizations scheme by Zender et al. ...” This sentence seems out of place. It needs to be integrated at line 15 where you are talking about dust emissions. Also, are the geographic distributions of dust sources used different from one scheme to another? Please be clear about this. I reviewed the earlier version of this paper, and noted that by default GEOS-Chem uses the GOCART dust source function, even when using the Zender et al., scheme. Response: The sentence (p. 1359, lines 26-28) was moved to line 15. We used GOCART dust source function for both simulations using Zender's scheme and Ginoux's scheme and we added this in the new version of manuscript.

4. p. 1360, line 13, wording. I suggest “during three events, on 1-8, 10-17, and 21-28 May 2003” Response: The authors revised the phrase as suggested.

5. p. 1361, lines 14-15. How can you compare simulated CO with observed AOD? This

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is a very odd thing to do. You can compare simulated with observed AOD, and point out that there is clearly a difference in the first event. You can identify differences between simulated and observed CO in Fig. 2. I don't think a similar pattern of simulated CO and observed AOD is a good basis for an argument. I think I know what you're trying to say, but it does not come over well. This is also a problem at p. 1365, line 4. Observations show different spatial (latitudinal) distributions of CO and AOD (Figs. 1, and 2). How can you talk of "consistency" between observed AOD and simulated CO, and at the same time talk of "large difference in transport pathways of dust and other aerosols or CO" (lines 8-9). Response: The authors removed the paragraph (p. 1361, lines 13-18) and the CO distribution (third column) from Figure 1. In the new version of the manuscript, we compare simulated and observed AOD in Figure 1 and move on to Figure 2 to compare simulated and observed CO. The paragraph (p. 1365, lines 4-15) has also been revised so that it does not compare distributions of AOD and CO, but instead compare dust and other aerosols.

6. page 1365, lines 19-20. Are you referring to the improved CO simulation when you talk of "transport pathway of CO is consistent with the observations (Fig.2)"? It's not clear. Response: The authors clarified this in the new version of manuscript.

7. p. 1366, lines 18-19. In my last review of this paper, I expressed surprise that "suppressing vertical transport" in the model had no affect on the dust transport. I still think that very surprising; the authors talk of upward flux over the dusty regions, and differential advection in transport over the Pacific. Are the authors confident, they did this right? What happens if they eliminate vertical advection altogether? Still no effect? Let alone the fact that artificially suppressing vertical advection (on its own) violates constituent mass continuity. Response: The authors checked again and we did everything correct. The transport pathway is not sensitive to uplifting of dust. We had the same surprise and had tried eliminating vertical advection altogether before the paper was submitted. Dust without any uplifting settled down very quickly and was not transported anywhere.

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8. p. 1368, line 7. I would leave out “more active in dust release than existing deserts.” The paper would benefit by showing a map of simulated dust emissions for May 2003 to justify or otherwise this statement. I think you will find the principal desert regions still dominant. Response: The phrase has been removed as suggested. The paper shows surface dust concentration during 1-2 May 2003 in Figure 7 as a proxy of dust emissions (We checked spatial patterns of simulated dust surface concentrations and emissions match very well). Principal desert regions are dominant in GEOS-Chem. We also found typos in caption for Figure 7 and corrected them.

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