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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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The authors use the GEOS-Chem chemical transport model to interpret satellite observations of CO and aerosol transpacific transport for May 2003. They demonstrate improved transpacific transport of CO when enhanced emissions from S.E. Asia are included. They illustrate a discrepancy between simulated and observed AOD, which they attribute to dust, and argue that transport pathways across the Pacific are sensitive to the location of bifurcation points in the meteorological fields. The paper focuses on explaining differences between observed and simulated CO and aerosol distributions. Identification of inadequate sources in S.E. Asia is important. Showing that transpacific transport is sensitive to bifurcation points in the flow is also important, since it illustrates that large-scale transport is subject to chaotic attributes in the large-scale flow. What

the paper lacks is an assessment of the impact of these observations, beyond identifying model deficiencies (see general comments below).

General

1. 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-quality met. fields the answer?

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.

2. p. 1364, line 12: How do you use MODIS to “constrain” the dust transport? Assess, yes, but constrain?

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.

4. p. 1360, line13, wording. I suggest “during three events, on 1-8, 10-17, and 21-28 May 2003”

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

6. page 1365, line 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.

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.

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1355, 2010.