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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 #1

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This paper examines a few episodes of trans-Pacific transport of CO and aerosols in the GEOS-Chem model to diagnose possible contributors in the model's inadequacy in an accurate representation of these transport events. I have several questions on how the simulations were conducted and compared with the satellite measurements (listed below in details). An appropriate simulation set-up and comparison methodology is essential in the validity of identification of the model bias, which is the basis of this paper. These issues needed to addressed before the paper is re-submitted to ACP again.

Specific comment # 1. Page 12903, Line 5-7: The authors state here that a simu-

lation of aerosols and CO with a 2-month spin-up was conducted for the analysis of the transport events. I am seriously concerned about the 2-month spin-up time for an aerosols and CO simulation. While the atmospheric lifetime of aerosols is on the order of a month and that of CO is $\sim$ a couple of months, the local lifetime of aerosols (CO) are highly variable, ranging from a few days (weeks) to several months. Springtime CO at many observation sites in the Pacific is affected by long-range transport of emissions from multiple regions, in particular midlatitude outflow from E. Asia, as well as high-latitude transport that are originated from Europe (Duncan and Logan, 2007) where lifetime of CO can be several months. Therefore, a 2-month spin-up time is not adequate for CO/aerosols to reach steady state. This might significantly affect the simulated CO concentrations, therefore alter the presented results.

Specific comment # 2. Section 4. In this section, the authors conducted a sensitivity simulation by increasing biomass burning emissions in the Indochina Peninsula by a factor of 4 (or 8), which yields a better agreement with the MOPITT CO for a single transport event. Thus they conclude that this indicates the GFED biomass burning emissions in the Indochina Peninsula during April 2003 is too low. I am not sure this is a convincing methodology. Here a few of my reasons.

i) A model's ability in accurately reproducing individual long-range transport events of CO depends on many factors, i.e. emissions, transport uncertainties, and OH fields. In particular, lofting of biomass burning emissions in SE Asia is mostly associated with deep convection. An accurate representation of the location or intensity of deep convection remains an active yet challenging research topic. An important question that the authors need to address is how good is the GEOS-4 convection? Was the lifting mechanism for these events well captured by the driving met fields?

ii) While from an averaged perspective, biomass burning effluents from Southeast Asia might occur at lower latitudes than Asian anthropogenic pollution, the transport latitude of an individual biomass burning transpacific transport event can occur in a vast range of latitude band. There a relatively low transport latitude alone does not indicate the bias must be caused by an underestimate in biomass burning emission from SE Asia. Many previous studies have shown that the Asian anthropogenic emissions and biomass burning from SE Asia are commonly mixed in the outflow (e.g. Carmichael et al., 2003; Ma et al., 2003; Bey et al., 2001). Have the authors tried to increase the Asian anthropogenic emissions, which might be a possible alternative candidate in accounting for the model bias?

iii) Figure 3 shows extensive biomass burning in SE China at the same time. I would speculate that if the emissions from SE China were underestimated in the GFED inventory, it would have a similar impact. Have the authors tried to increase the biomass burning emissions in SE China and see how does that affect CO in the Pacific?

I would suggest the authors take a more in-depth approach to investigate the causes of this model bias, by using tools such as back trajectories, the GEOS-Chem tagged CO simulation to examine what was the origin of the plumes, its lofting mechanism, was transport too diffusive, etc. In addition to Guam, there are more surface observation sites in the Pacific that can be used to help this analysis. Also I don't really follow the idea of presenting a 4-day averaged field. This makes it difficult to figure out the origin and pathway of a transport event.

Specific comment # 3. Section 5-6. The details of the comparison between the GEOS-Chem AOD and MODIS AOD were not explained clearly in the text. MODIS usually does not report data under cloudy conditions, while a lot of the Asian outflow happens in the cloudy sector of frontal systems. Did the authors filter the model appropriately for a meaningful comparison between the simulated and observed AOD? It would be necessary to compare daily maps of MODIS AOD with GEOS-Chem AOD to see if the 5-8 May dust transport event happens in the cloudy sector of a frontal system to understand if it is indeed due to model overestimate or it is just that no simultaneous MODIS retrievals were available at the corresponding location. Showing a 4-day average of wind vectors (Figure 9) is not an appropriate way to explain what meteorological systems are involved during this transport episode because this does not offer accurate

information of the relative position of the plumes with respect to the pressure systems.

Specific comment # 4. Figure qualities. Figures 1, 2, 5, 6, 7, and 9 seem to have a resolution problem when they are inserted into the manuscript, which impairs their clarity. Some of the figures were too small, which made it difficult to confirm what's discussed in the manuscript with what's shown in the figures.

Minor comments:

1. Page 12900, line 24-26: Trans-Pacific transport has been well-studied in the past decade and many previous studies showed that trans-Pacific transport has an episodic nature and occur more frequently than once a year. For example, Yienger et al. (2000) shows that 3-5 Asian plumes impact the west coast of the United States' boundary layer between February and May. Liang et al. (2004) suggests that long-range transport of CO from Asia to the northeastern North Pacific region occur year-round every 10, 15, and 30 days in the upper, middle and lower troposphere. None of the three papers cited here by the authors claim that these transport events occur on average only once a year. Table 7 in Jaffe et al. (2003) lists $\sim$2-3 transport events identified during spring 1999 and 2001. The relatively smaller number of events in the earlier years is a result of limited observations available when long-range trans-Pacific transport was not an active research topic. Heald et al. (2006) identified 4 aerosol transport events in a single spring.

2. Page 12902, line 12-16: How do you reach the conclusion that may 2003 was the period of largest enhancements of trans-Pacific aerosols as well as CO? This was not clearly explained in the text. What was the magnitude of enhancements with respect to the background during this month? What was the average enhancement level? Please give quantitative information.

3. Title and Page 12910. The importance of the dipolar structure in the pressure/wind fields in the eastern Pacific in regulating the trans-Pacific transport has been noted in several previous studies, Holzer et al. (2005), Liang et al. (2005), and Liu et al. (2005),

which should be properly accredited in this study.

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