

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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Review of Nam et al., ACPD, 2009

The authors evaluate transpacific aerosol and CO transport in the GEOS-Chem model using MODIS AOD and MOPITT CO for May 2003. They employ sensitivity tests with the model to determine the possible roles of transport and emissions distributions in contributing to differences between observed and simulated transport. They attribute differences in observed and simulated CO to underestimated biomass burning emissions from Indochina in April, 2003. They attribute differences in AOD to dust transport in the model, and discuss the possible role of errors in the wind fields and source locations in explaining these differences.

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I think this is an interesting piece of work. I find the discussion and analysis of the impact of CO emissions on the simulated transpacific transport persuasive. I find the analysis and discussion of differences in the dust transport less persuasive. The authors give equal plausibility to potential inadequacies in the advecting wind fields and dust source locations. I think anomalies in dust transport, found in the upper air, well out over the Pacific, are much more likely due to inadequacies in the transporting wind fields, rather than to inadequate representation of source locations (see below). A key finding shown are observed differences between GEOS-4 and NCEP winds. The dust reaches high altitude via lofting by the wind fields. It is strange that the authors find little or no impact on the dust transport by suppressing vertical transport in the model (see below). Also, deficiencies in dust source locations identified by Chin et al. (2003) are not relevant here, since the dust source function used in the model has been updated since then to reflect Chinese desert areas, and the changes applied by Chin were targeted to resolve boundary-layer outflow.

Some correctable deficiencies and clarifications can be made to the paper. I also think that the discussion and comparison of a similar period in 2006 does not add much, and could be cut without losing the theme. Given these corrections, I think the paper will be suitable for publication in ACP.

Specific comments:

p. 12903, lines 13-19. The authors should refer to the paper by Fairlie et al. (2007) who describe the implementation of both GOCART (Ginoux et al., 2001) and DEAD (Zender et al., 2003) dust mobilization schemes in GEOS-Chem, compare and contrast the dust emissions distributions using the two schemes separately, apply the model to transpacific transport of dust in 2001.

p. 12906, lines 11-24. I think this whole discussion and comparison to 2006 (and Fig. 3) could be cut. I don't see it as necessary to the theme of the paper, which is explaining differences in observed transport in May 2003. Go straight into showing Fig.

4 to illustrate the improvement with adjusted CO emissions.

p. 12908, line 14. The authors should make clear when they use the DEAD dust scheme (Zender et al., 2003) if they are using the seasonally dependent DEAD source function. The default in GEOS-Chem is to use the GOCART (time-independent) source function even when the DEAD mobilization formulation is used (Fairlie et al. 2007). If the source functions used are the same (and I suspect they are), then differences between simulations using the DEAD and GOCART schemes will be primarily a matter of magnitude and dust size distribution; the distribution of potential dust sources will be the same. In which case, it should not be necessary to show results from both schemes.

p. 12910, lines 1-2. It's hard to understand why the dust transport is insensitive to suppressing vertical transport, since clearly it is vertical transport that is responsible for lofting the dust to high altitudes, and the authors note differential advection with altitude in the Pacific high circulation (p.12910, line, 17). If the dust is veered too far south, this suggests that it is transported at too high an altitude, and should respond to changes in vertical advection.

p.12910, line 3. The authors mention a “third sensitivity experiment,” to study the impact of convection, but merely say they don't expect deep convection over arid source regions. This sound like an argument for discounting convection without having tested it. Did they suppress convection in the model, or not?

I find the argument that biases in the dust transport are associated with inaccuracies in the GEOS-4 transport wind field much more persuasive than that this is due to inadequate dust source locations. I'm certain the dust sources are not perfect by any means, but I'm not persuaded that missing source locations have a “magnitude . . . comparable to those from the Taklimakan and Gobi deserts.” (p. 12911, lines 15-16). The GOCART source function was updated in GEOS-Chem, since the ACE-Asia study by Chin et al. (2003), to reflect latest Chinese desert maps. This is certainly true

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for the version v7.03.06 of the model the authors are using. Moreover, the additional sources introduced by Chin et al. (2003) were focused on resolving inadequate simulation of boundary-layer outflow, rather than high-altitude outflow, which the current study shows. So I don't think the argument emphasizing inadequate sources holds in this case. The authors point to differential advection in the vertical associated with the Pacific high circulation, with more pronounced southward displacement at higher altitudes. I suspect that the altitude at which the dust is being transported may be the issue, which points again to the GEOS-4 wind fields, both for transpacific transport and for vertical lofting closer to source regions (see comment above on suppressing vertical advection).

Detailed

p.12900, line 24 – p 12901, line 5. This is a bit vague. If you mean that transpacific transport occur preferentially in spring, then say so. If there's an increase in background concentrations due to transpacific transport, that suggests the transport is continuous rather than 'once a year'

p.12901, line 17. The authors say they make use of 7 years of MODIS data, but discuss only 1 month. 7 years of MODS data doesn't seem that relevant

p.12908, Figs. 6, and 8. I think that aerosol extinction would seem a more natural quantity to show than "AOD density." Alternatively, the authors could show the aerosols as a mixing ratio.

Reference

Fairlie, T. Duncan, Daniel J. Jacob, Rokjin J. Park, The impact of transpacific transport of mineral dust in the United States, 2007, Atmospheric Environment, 41, 1251-1266.

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