Atmos. Chem. Phys. Discuss., 11, C5286–C5288, 2011
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biomass burning contribution to black carbon in the western United States mountain ranges" *by* Y. Mao et al.

Anonymous Referee #1

Received and published: 22 June 2011

The GEOS-Chem model was used to simulate BC over the western U.S. for 2006. To understand the large low bias of model results, three meteorological fields from GEOS-3, GEOS-4, and GOES-5 were used to test the model sensitivity. Selected simulations were also done for 1998 and 2004 to compare with a previous study and to analyze a year of lower fire emissions, respectively. The paper is written well and is publishable in ACP. I will provide here some general comments, which will further improve this work.

The most interesting aspect of the paper for me is that more realistic meteorological simulations (boundary layer heights of GEOS-4 and GEOS-5 compared to GEOS-3 and precipitation of GEOS-4 compared to GEOS-5) degrade the quality of model BC simulations. The ramifications of this result for applying model simulations to source attributions of BC or other pollutants are very significant. I hope that the authors can

add more discussion on this aspect and state more clearly which model fractional attributions in this paper are reliable.

For the reason described above, the least appealing section to me is 3.6 (I should say that I didn't find any error in that section). I personally find the large fractions of pollutants attributed to Asian sources across the Pacific uninteresting. One can make as good a guess simply by looking at the emission inventory. There are three issues that I do think that the authors should think about and revise the paper accordingly.

It is clear that the model underestimates BC in fire seasons, which should be stated explicitly in the abstract and conclusions. The reason may be a large underestimation of fire emissions in the western U.S., which should be stated more directly in the paper if true. A more detailed analysis should be done by separating biomass burning sources. There is no reason to lump fire emissions over the U.S. with the rest of the world (see Zeng et al., 2011). While the 50% increase of global biomass burning is large, the effect comes probably only from the emissions over the western US (and some from western Canada). I think it would be very reasonable to increase the regional emissions over the western North America by a factor of 2 and see if BC simulations are improved. If not, the model probably has another very serious problem not recognized by the community. I think that this aspect of the paper is very interesting and scientifically significant.

When comparing to high-altitude sites, the model results are probably sampled at the corresponding altitudes. I think that this is problematic. The IMPROVE sites are "surface" sites. In daytime, a large fraction of fire emissions at lower altitudes can be transported along the slope up to the sites. This is a very different process from boundary layer mixing and then transport. In fact, one issue with the model comparison may well be that boundary layer mixing is instantaneous such that the BC gradient from the surface to the mixed layer cannot be simulated in the model. The IMPROVE sites from 0-4 km all sample this "surface" layer, not the mixed layer. It is less an issue for anthropogenic BC since the sites are located away from the large anthropogenic

sources.

I don't think that the horizontal resolution of the model is as important as the over-the-source vertical gradient. By comparing the 2x2.5 emissions in the model with the original GFED fire emission distributions, it would be useful to comment on how the spatial averaging of emissions may affect the BC simulation results.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13425, 2011.