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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The sensitivity of CO and aerosol transport to the temporal and vertical distribution of North American boreal fire emissions" *by* Y. Chen et al.

Anonymous Referee #1

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The sensitivity of CO and aerosol transport by Y. Chen et al.

This paper very carefully documents the sensitivity of CO and aerosol transport to the specification of emissions from the forest fires in Alaska and Western Canada during 2004. It is very nice and thorough study. The figures are well chosen and provide a good synopsis of the different simulations. The explanations are clear.

Comments:

My primary concern with this paper is that the model data comparison needs to be more quantitative. The biases, root-mean-square differences or correlations should be given where appropriate to distinguish between different simulations. Statistical tests

should be conducted where possible. Sometimes the authors state one simulation is better than another – but often this is not obvious from the figures. These statements should be backed up quantitatively. It would also be helpful to point out those situations where the data does not discriminate between the simulations. The following gives some instances where the model-data comparison could be improved. This is not a complete listing.

-Page 1171, line 13-15. While biomass burning emissions improves the simulation in the upper troposphere to be fair you should mention that the inclusion of biomass burning emissions degrades the simulation in the lower troposphere.

-Page 1171, line 23: It is not clear from inspection that in general the uniform distribution deviates more from the measurements. Does a statistical analysis support this statement? I suspect the CO measurements cannot discriminate between the different emissions scenarios. Please back this statement with a more statistically rigorous analysis if you wish to include it.

-Page 11972, line 15. Please see above comments. Also, in contra-distinction to the text, in the upper troposphere the uniform emissions appear to underestimate the mean measured BC concentrations (Figure 9a) but appear to compare best with the measurements. Since you are presumably showing the model mean shouldn't this be compared with the measured mean?

-Page 11973, line 2, "Applying temporal constraints. . .." This is not obvious from the figure. Could you back this up by giving correlations, rms errors? Also, line 17 – this is not clear from inspection and should be backed up quantitatively.

-Page 11973, line 29: "except the monthly". Can you show this quantitatively? I'm not sure the differences are significant.

-Page 11975, line 23, "improves the timing". Please be more specific or back up statistically.

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This paper does not account for variability due to changes in emission factors. One might expect significantly different emissions of CO and BC during smoldering and flaming conditions. Presumably, one could introduce these into the emission parameterization without too much trouble. It might be worth mentioning that this variability is not included. I assume it might be important.

In the conclusions it would be good to more of an indication as to which conclusions apply to the particular fires studied (for example, the relative insensitivity to emission height might apply to this particular fire) and which conclusions might generalize to other situations.

Minor Comments:

-Page 11954, line 15: Isn't "arbitrary assumptions" a bit strong? Maybe reword to "the injection height used in these simulations lacks strong observational support".

-Page 11956, line 23: It is not clear what you mean by "CO-only and offline aerosol simulations". Aren't all simulations using this model "offline"?

-Page 11957, line 24 and 25: What are these scaling factors based on? -Bottom page 11958 and top 11959: It is not clear how the ABBA satellite products are used to construct the diurnal cycle.

-Page 11960 line 19: "is different from". Do you mean in a fundamental way? "Regular" convection over a polluted boundary layer would also be expected to loft high concentrations of pollutants. I'm not sure I follow your meaning here.

-Page 11962, line 4, "may underestimate". The logic here is not altogether clear: do you mean it may underestimate the plume height in some fires, or that that due to a correlation between intense fires, high emissions and high plume heights this parameterization will generally underestimate the emissions to the higher altitudes.

-Section 4 and lines 8-21 page 11961. The extent to which plume heights were adequately sampled with MISR is not clear. What percent of fire pixels were you able

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to establish plume heights for - what percent did you resort to the PDF approach for MISRind?

-Table 1: I find that this table is not particularly easy to read. Putting the various parameterizations along the top of the table, then 'x-ing' the process used in each simulation would provide the reader with a quick and transparent summary of the various simulations.

-Page 11964, line 9: Have CH4 retrievals been realized from MOPITT?

-Figure 7, please put in figure caption which column is for CO versus BC. The labels are not immediately obvious.

-Figure 8, this figure is legible, but barely. Please make sure the line size of the caption matches the lines on the figure.

-Figure 9. This figure is really very difficult to read. I would recommend putting parts (a) and (b) in a separate figure and enlarging. For example, the synoptic line for CO in 9a is not visible at all.

-Discussion: Note that Pfister et al (2005) suggests that their inverse calculations are not dependent on the vertical distribution of fire emissions, in general agreement with this study. For what it is worth the synoptic estimate of emissions in this study is roughly consistent with the inverse calculation of Pfister et al.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11951, 2009.