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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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 #2

Received and published: 6 July 2009

This paper describes a series of sensitivity analyses performed in order to better understand the importance of temporal resolution of the biomass burning emissions as well as their injection height on the CO and aerosol burden and transport. Therefore, the authors compare model simulations using different biomass burning emissions temporal resolution (monthly, weekly, incl. diurnal variations) and different vertical distributions with surface and satellite observations.
It is well written and provides a complete picture of the impact of these parameters on model performance, applied to the case of the large boreal fires of the summer 2004. Therefore, I recommend publication in ACP, provided attention is paid to the following comments.

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Interactive Discussion

Discussion Paper

I agree with previous comments that the discussion of previous and ongoing work on injection height study and parameterization should be more careful, and that comparisons should be more quantitative and precise.
In addition to these points, my main comment concerns the discussion of the impact of injection heights. In particular, the vertical distribution relies on the MISR product but there is no specific mention of the associated uncertainties, in particular due to its limited coverage. Could a comparison with identified events during this time period be undertaken (e.g. the one discussed by Damoah et al.?)
Finally, it would be helpful to the reader if the authors could provide recommendation regarding this problem: is it worth having specific injection heights or not in model simulations, or if mixing within the PBL gives good enough results. Do injection heights from MISR allow better results for long-range transport events?

Some other general comments, as well as more specific comments, are listed below.

General comments

Previous modeling studies (Turquety et al., Pfister et al.) on this time period agree that on average the injection height does not have a clear impact on the CO comparisons with MOPITT and in situ ICARTT observations on average over the fire season. As you state, these studies relied on averaged distributions so that the conclusions could not be as clear as in this complete study. However, several studies also found that injection in the UT could be important to simulate specific long range transport events, particularly for the transport towards Europe during ICARTT. You mention this in the discussion section but it is not clear how important this could be.

As mentioned in the general overview, the uncertainty associated with the use of MISR derived injection heights should be discussed more specifically. It would be useful to add some detail in Section 4 on this, particularly on the uncertainty due to limited coverage. Could you compare the MISR derived injection with documented events?

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For example, a case study has been published by Damoah et al. indicating that pyro-convection may have played an important role during this fire events based on the POAM observations:
“A case study of pyro-convection using transport model and remote sensing data” by Damoah et al., Atmos. Chem. Phys., 6, 173-185, 2006. More precisely, they discuss a strong event at the end of June 2007, with injection up to 3km above the tropopause. Did you check if MISR was able to prescribe this kind of high altitude injection?
Also, please provide the number of MISR heights used in this study, the number of fire pixels covered (or percentage), as well as the averaged/maximum/minimum injection height performed. I would find it useful to have the maximum and minimum profiles plotted on Figure 3, since the MISRind average profile is very close to the MISRpdf.

Specific comments

- Introduction: references to other work on parameterization of injection heights should be added (Trentmann and coworkers). There is also a study based on CALIPSO observations:
  Labonne M., F.-M. Bréon, F. Chevallier (2007), Injection height of biomass burning aerosols as seen from a spaceborne lidar, Geophys. Res. Lett., 34, L11806, doi:10.1029/2007GL029311.

- Section 2: you mention that the GFEDv2 emissions had to be scaled by a factor of 1.2: how was that scaling factor chosen? Should it always be applied in boreal regions?
  During the summer 2004, there were also quite large fires in central Canada (Turquety et al.). From the area provided at the end of section 2, I guess all of Canada is included in the inventory and not only Western Canada, right? If so, then correct the last sentence to state that the total is for Alaska and Canada (same thing p. 11960, l.2).

- Section 2 and 3: you chose here to use the GFEDv2 monthly and weekly inven-

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tories, and develop as 3days inventory with 3hourly coefficients to simulate the diurnal cycle based on GOES. How is the synoptic variability 'superimposed' on the diurnal inventory?
Previous inventories developed for this time period (Pfister et al., Turquety et al.) have daily temporal resolution. Could you compare your variability with theirs (even the general features compared to the published figures)?
I guess the motivation for the use of the GFEDv2 data was its availability for other time periods. Is that correct?

- Section 4: as mentioned in the general comments, is would be useful to add details on the uncertainty associated with the MISRind injection heights derived here. Could specific events be missed?
  P.11961, L.6: mention work on Calispo by Labonne et al.
  P.11962, L.13: why did you choose to use the PDF for plumes not detected by MISR and not the PBL one? Is it related to some detection limit of MISR? Also, even if the fires were very persistent, implying that a 8-days period on the emissions is realistic, but I am wondering if it is still realistic for injection height? Does the energy remain at the same level during 8days?
  L.20: The vertical distribution of the injection height in Turquety et al. and Leung et al. is constant but NOT uniform!! They apply different coefficients in the boundary layer / middle troposphere / upper troposphere. In Turquety et al., it is 40% in the PBL, 30% above up to 400 hPa, and 30% in the upper troposphere (400-200 hPa). Please modify this statement everywhere and state that you have chosen a simplified average configuration.

- Section 5: Could you motivate the choice of observation datasets used? In particular, why not use surface CO measurements? Why not use satellite AODs?

- Section 6: I agree with referee #1 that a more quantitative discussion is needed.
  Section 6.1.1:

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I guess it was expected that temporal resolution of the fire emissions is more critical than injection height but it would be good to know precisely what uncertainty can be expected for both parameters, i.e. how the model reacts to different choices.
Is there a difference in flux calculations (table2) with different injection heights? What is the impact further downwind over the Atlantic and Europe (what percent change)? this information would be useful for the long-range transport analyses.

Section 6.1.2:
You show the impact of injection heights on the 3 months average CO burden to be very small. Does this conclusion still hold for smaller time periods ?

Section 6.3:
P.11972, L. 20-21: I guess model interpolation on MOPITT a priori vertical levels also needs to be done before convolution with averaging kernels.
Could you elaborate on the necessity of using a threshold on the a priori contribution? Should retrievals with a priori fraction larger than 30% be removed because retrieval error is too large in these cases? If it is only a question of available information in the retrievals, my guess is that the convolution with the averaging kernels should account for this in order to have comparable model and observations...
In my opinion, the fact that the bias is larger with lower a priori contribution is not only due to smaller amount of comparison data but also to the fact that the smoothing on the model (and obs) is larger when the a priori contributes more. For very small information content, the model and observation are both close to the a priori, so that the a priori profile is compared to the a priori profile.
It would be interesting to discuss the differences (and/or similitude) of your comparisons with MOPITT with those of Turquety et al., who did similar exercise with the same model but only different emissions and injection heights.

- Discussion:

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Maybe the discussion on the MISR coverage could be mentioned earlier.
it would be interesting to also discuss rapidly the effect on the long range transport over the Atlantic towards Europe (one of the objectives of ICARTT).

Tables:
Table 1: line nobbNA; remove additional 's' on emissions.

Figures:

- Figure 1: the purple and blue colors are too close; it is difficult to distinguish the diurnal and synoptic lines.
- Figure 3: add maximum and minimum altitude injection height for the MISRind to give an idea of the range of variability during this period.

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