Reply to anonymous referee #2

We would like to thank the referee for the thoughtful and insightful comments. We have addressed all of the comments. Our responses are itemized below.

General comments:

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.

– Point well taken. We have included more quantitative analysis including correlation coefficients for comparisons.

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.?)

– More information on the uncertainties of MISR-derived plume height is now added.

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?

– We have added discussions on this in the summary.

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.

– It is likely that some trans-Atlantic transport events (such as those observed during ICARTT) are very sensitive to smoke plume injection height distributions. We did not seek to address this question in any depth, as the present study is already too expansive.

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? 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?

– We have added more discussion on the uncertainty of MISR smoke plume height and note that its validation is rather limited at present. The case of pyro-convection reported by Damoah et al. (2006) was not observed by MISR.

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.

– More information on the MISR plume height data is now included.

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.

– We now indicate the maximum and minimum, individual plume heights.

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.*

– Yes. Added.

- *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?*

– We have added more discussions justifying these scaling factors.

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).

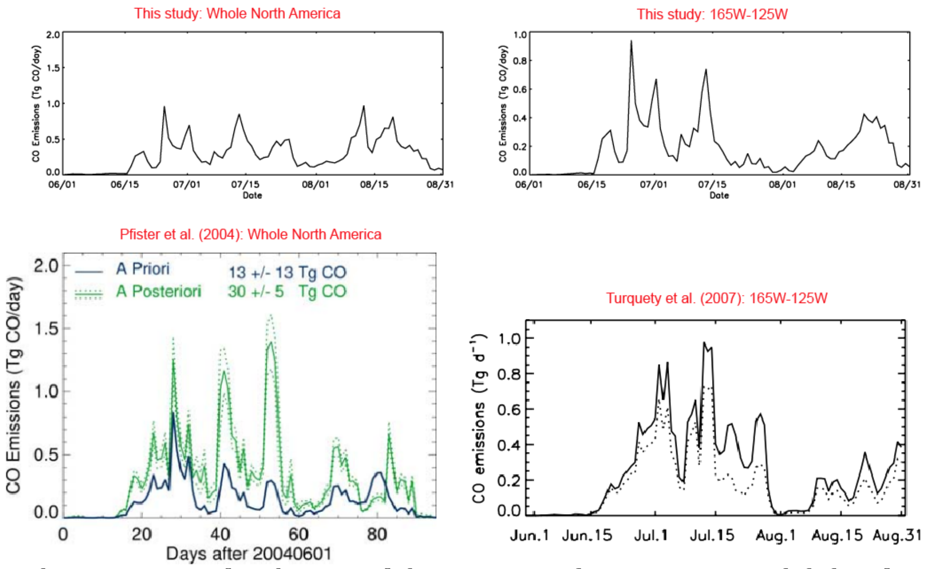
– Yes. We have revised the text to clarify this.

• *Section 2 and 3: you chose here to use the GFEDv2 monthly and weekly inventories, 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?*

– We have added discussions to make it clear how the synoptic variability is implemented.

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)?

– Point well taken. The figure below (not shown in the manuscript) shows the day-to-day variation of fire emissions from the *synoptic* GFEDv2. We used the same biomass burning region as those in Pfister et al. (2004) and Turquety et al. (2007). We have added discussions on these comparisons.



I guess the motivation for the use of the GFEDv2 data was its availability for other time periods. Is that correct?

– Yes.

• *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?*

– We have added more discussion on the uncertainty range of MISR injection height product.

P.11961, L.6: mention work on Calispo by Labonne et al.

– Cited.

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?

– We believe for those low smoke plumes, MISR observations still provide information on their average vertical distribution. Therefore we still apply the PDF from MISR to emissions without direct observation of injection height. We acknowledge that using the same vertical distribution in an 8-day period for those 'high plumes' is not very realistic. The changes in fire energy, emission factor, or meteorology will cause variations in the emission vertical distribution. Detailed exploration of these variabilities is beyond the scope of our study.

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.

– Point well taken. Rephrased.

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

– Discussion on the motivation added.

Section 6.1.1: 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.

– Yes, it would be. However, the uncertainties in both the temporal constraints and plume heights preclude a precise quantification of model uncertainties in response to these parameters.

Is there a difference in flux calculations (table2) with different injection heights?

– No. The mass fluxes were weighted by total emissions in each model grid cell only.

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.

– Further downwind over the Atlantic and Europe, the effect caused by temporal and vertical variability of North American fire emissions becomes smaller, especially when averaged over longer periods (see Fig 4-7). However, stronger impact is likely during individual events of long-range transport of smoke plumes.

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 ?

– See responses above.

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.

– Yes. The level interpolation of model results was done before convolution with MOPITT averaging kernels. We have revised the sentences to make this clear.

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.

– We agree with the excellent pointed made by the reviewer and added more discussion. Larger a priori fractions conceivably lead to larger smoothing errors hence larger retrieval uncertainties.

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 added in section 6.3.

Discussion: Maybe the discussion on the MISR coverage could be mentioned earlier.

–Thanks for the suggestion. We have decided to keep this discussion in the summary part because it points to a future study which can use the more spatially- and temporally-covered MISR smoke plume product.

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).

– Indeed. We are considering a follow on study specifically focusing trans-Atlantic long-range transport events.

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

– Removed.

• *Figure 1: the purple and blue colors are too close; it is difficult to distinguish the diurnal and synoptic lines.*

– Indeed. Changed.

• *Figure 3: add maximum and minimum altitude injection height for the MISRind to give an idea of the range of variability during this period.*

– Added.