

Interactive comment on “Mega fire emissions in Siberia: potential supply of soluble iron from forests to the ocean” by A. Ito

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The author is grateful to all reviewers for their constructive comments. Below is a detailed answer to the comments.

Comment 1: This paper covers two subjects. The first subject is about CO emissions from boreal forests fires. Model simulations are compared to MOPITT satellite measurements and two main fire periods are studied. The second subject deals with aerosol transport of the fires. Extreme fires in the boreal regions may contribute to the deposition of soluble iron in the North Pacific Ocean. This means that the biological activity of the ocean may be coupled to fire occurrence. Although I think that the paper is interesting and a lot of work has been documented, I also conclude that

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(1) The comparison with MOPITT on different vertical levels is flawed due to the low sensitivity of MOPITT to surface CO.

(2) The deposition of iron to the ocean is not very well framed.

In the following I give some more detailed comments on these two items.

Response: (1) The MOPITT retrievals based on a subset of thermal infrared (TIR) radiances are less reliable at low levels (Deeter et al. 2003). Using both TIR and near infrared (NIR) channels of MOPITT will provide improvements in the measurement ability to resolve CO in the lower troposphere (Worden et al., 2010). In the revised paper, the comparison is shown based on column data only.

(2) Guieu et al. (2005) have concluded that pyrogenic inputs have little impact on the global iron budget since they represent at most 10% of desert dust inputs. On the other hand, there is significant interannual variability in the forest fire emissions, which causes large gaps between the bottom-up and top-down estimates of carbon fluxes (Ito et al., 2008). Thus a new emission data have been developed to compare the iron deposition from biomass burning with that from dust.

Comment 2: Comparison with MOPITT data

Response: First of all, better agreement was achieved by introducing several revisions in the methods:

(1) The anthropogenic emission data are updated to the data set for IPCC AR5 report (Lamarque et al. 2010).

(2) Both the Terra and Aqua MODIS active fire products (Justice et al., 2002) are used to maximize the probability of fire detection against various omission factors such as cloud obscuration or temporal mismatch between peak fire intensity and satellite overpass time.

(3) The soil moisture index of the top soil layer is taken from 3-hour time-averaged

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assimilated meteorological fields from the Goddard Earth Observation System (GEOS) of the NASA Global Modeling and Assimilation Office (GMAO) (Bloom et al., 2005).

(4) The standard simulation was conducted on a horizontal resolution of $2.0^\circ \times 2.5^\circ$ with daily emissions according to MISR plume heights.

Below is a detailed answer to the comment 2.

Comment 2.1: A large part of the paper deals with the comparison between MOPITT and model. The authors are well aware of the fact that MOPITT has a low sensitivity to surface CO. Therefore the authors describe on page 1496 that they apply the MOPITT averaging kernel. With that, they also use the prior profile that was used in the retrieval of MOPITT version 4. Although these prior profiles are based on a model, they generally do not account for extreme biomass burning events that are analyzed in this paper. Since MOPITT has low sensitivity for surface CO, the value provided by MOPITT, and also the shape of the profile, will be largely determined by the prior profile. Moreover, the model results are treated with the same averaging kernel and prior profile which almost by definition implies a good correspondence with the MOPITT profile. For these reasons I think that the comparisons in figures 5 and 6 are flawed. These figures suggest much more height information in MOPITT than is actually present in this kind of measurements.

Response: Same as in response to Comment 1(1). The comparisons of vertical profiles (previous figure 5 and figure 6) are removed.

Comment 2.2: Figure 4 shows a comparison of the total columns, a much more logical step. However, for these figures I would have expected a kind of statistical comparison. How well are the observed patterns actually reproduced by the model? Now the fact that the model is low is blamed to too low anthropogenic emissions and a fair comparison by eye can not be made properly.

Response: The comparison in figure 4 is quantified by the spatial (x, y) and temporal

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(t) variability in the relative model error (RME) on the model horizontal resolution of $2.0^\circ \times 2.5^\circ$ defined as

$$[RME]_{xyt} = 100 \times ([Observed\ CO]_{xyt} - [Modeled\ CO]_{xyt}) / [Observed\ CO]_{xyt} \quad (3)$$

where modeled CO is the monthly mean of the model CO column. According to the above definition, positive values mean that model results are underestimated in reference to the MOPITT retrieved data and vice-versa. As noted above, the modeled CO tends to be lower than the MOPITT data. Over the strong fire regions, the standard simulation results (Exp1) show less errors (less than 20%) relative to the MOPITT, compared to those (more than 40%) for the sensitivity simulation results (Exp2). For the purpose of quantifying the effect of intense fires, sensitivity simulations were performed with 5-year averaged monthly emissions for CO (Exp2). Better agreement with the MOPITT observations is also found downwind regions over the western North Pacific Ocean.

Comment 2.3: More or less the same holds for figure 3. I am not particularly impressed by the model performance, but the author seems to be quite satisfied (results are in reasonably good agreement). A correlation graph would give a more quantitative comparison.

Response: A correlation graph for figure 3 is shown. Anomalies to 5-year mean are also shown as a new figure. The slope, intercept and correlation coefficient are calculated from a least squares fit to the model results (Exp1) versus MOPITT. As for the CO anomaly, the model results exhibit good agreement with the MOPITT data. The correlation coefficient for the anomaly ($R^2 = 0.55$) is larger than that for the monthly mean ($R^2 = 0.37$). These results suggest that the model using emissions estimated in this work is able to describe the interannual changes in CO due to different strengths of forest fires.

Comment 2.4: From figure 3 it seems that the MOPITT column maxima appear systematically later in the year. In conclusion I think that the comparison should be more

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quantitative and based on column data only (due to the low surface sensitivity of MOPITT).

Response: The systematic differences between model results and the MOPITT observations are not seen from the anomalies of CO from the 5-year averages. The anomalies due to the fire emissions are calculated from the differences in CO between the monthly averages for each year and the monthly averages calculated from the 5-year data. The linear correlation coefficient for the anomaly ($r = 0.71$) is larger than that for the monthly mean ($r = 0.61$). These results suggest that the intense fires may not cause the systematic time lag.

Comment 3: Iron deposition What I understand from the paper is that Iron attaches to aerosols and therefore behaves different than CO. It is susceptible to wet and dry deposition. Therefore the simulation of iron would depend critically on emission height and possibly resolution. I find this an interesting topic certainly worth studying. The paper, however, fails to show the difference between surface emission and emission according to MISR plume heights. As presented in the paper, the iron story is a bit detached from the rest of the paper and I do not really see the point of the inclusion. This is a missed opportunity, since it would be interesting to show the different behavior of aerosols compared to gases.

Response: The standard simulation was conducted on a horizontal resolution of $2.0^\circ \times 2.5^\circ$ with daily emissions according to MISR plume heights for CO (Exp1) and iron aerosols (Exp3). The intense forest fire is often neglected in global chemistry-climate models to calculate radiative forcing due to biomass burning (Ito et al., 2007; Naik et al., 2007). For the purpose of quantifying the effect of intense fires, sensitivity simulations were performed with 5-year averaged monthly emissions for CO (Exp2) and iron aerosols (Exp4). The aerosol model results show enhancements in the ratio of iron deposition estimates (Exp4/Exp3) over the North Pacific Ocean by a factor of 1.4–2.6, in contrast to smaller differences in CO column (a factor of 1.0–1.4 over the North Pacific Ocean).

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Comment 4: Minor Remarks Comment 4.1: Page 1498, line 16: The estimate of CO emissions.... Better use "Our estimate of 22 Tg in May 2003 is"

Response: This is done.

Comment 4.2: Page 1498, line 24: The model simulates.....Better would be: "The model captures...". Same on 1499, line 28.

Response: This is done.

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