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

Anonymous Referee #1

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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

- The comparison with MOPITT on different vertical levels is flawed due to the low sensitivity of MOPITT to surface CO

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Interactive Discussion

Discussion Paper

• 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.

1 Comparison with MOPITT data

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 MO-PITT 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 withe 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.

Figure 4 show 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. 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. 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 quantitative and based on column data only (due to the low surface sensitivity of MOPITT).

2 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 intresting to show the different behavior of aerosols compared to gases.

3 Minor Remarks

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

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

Interactive comment on Biogeosciences Discuss., 8, 1483, 2011.