Review:

Mega fire emissions in siberia: potential supply of soluble iron from forests to the oceanography
by A. Ito

The manuscript introduces a model study, which developed a biomass burning emission inventory for forest fires for the high Northern Hemisphere latitudes using satellite burned area observations in combination with a biogeochemical model. The emissions were applied in a chemical transport model and simulated atmospheric CO concentrations were compared to MOPITT observations. Furthermore the biomass burning emissions were applied in an aerosol model and soluble iron deposition from biomass burning sources to the North Pacific Ocean were compared to those from dust sources. When no atmospheric processing of dust by acidic species is considered, iron from biomass burning emissions are simulated to contribute 10 -6% to the total deposition in the western North Pacific.

The manuscript is very well written and nicely structured. I recommend publication of the manuscript in Biogeosciences after some major revision. In the following I list major concerns the authors may want to address:

1. The title suggests that the manuscript primarily focuses on iron deposition. However, only a short chapter at the end of the manuscript discusses iron deposition. I understand that in order to get to iron deposition estimates many steps have to be made. The present study is clearly outstanding in the respect that all these steps are undertaken within one study, i.e. starting from the development of a biomass burning emission inventory for the high latitudes of the Northern Hemisphere. However, major conclusions of the manuscript are based on the simulated iron deposition, for which a more detailed analysis would be desirable. For example:

- The deposition of iron onto the ocean surface depends besides the biomass burning emission source largely on the simulated aerosol transport. While the emission source if evaluated using MOPITT CO measurements, this does not allow any conclusions about the quality of the simulated iron deposition. The authors state that the comparison with observed AOD data is beyond the scope of this manuscript. However, this would highly contribute to an understanding of how well the aerosol processing is reproduced in the model.

- The simulations showed that iron from biomass burning significantly contributes to the iron deposition in the western North Pacific and conclude that fire play a role as a negative biosphere-climate feedback, due to an increase of atmospheric $CO2$ uptake by the ocean. This only holds when primary production in the western north Pacific is limited by iron . The author should discuss more in detail (including references) the role of iron for the the western north pacific at present and possible future changes.

- The authors argue that the supply of soluble iron from dust could be reduced in the future, when $SO2$ emission are reduced. This is accounted for in the model by eliminating atmospheric processing of dust by acidic species. I was wondering how realistic this is. It is likely that $SO2$ emission will be reduced in the future to improve air quality. However, it is not likely that this will result in zero $SO2$ emissions. Do one needs a large amount of $SO2$ emissions to make dust iron soluble?

2. The basis of this study is the development of a biomass burning emission inventory for the northern high latitudes. The modeling approach for the the biomass burning emission model in partly difficult to

follow:

Page1490/Line 20: “The aboveground (ground-layer) fires are assumed to last 1 day (8days)”. Can you explain more how the length of the fire event enters your calculation and how this then relates to the daily fire count data?
Page 1491/Line 6: “For this study, the fire component embedded in the model is not used for stand-replacement disturbance”. Do you apply the satellite observed instead?
Page 1491/Line16: It is not clear to me how the scaling of the combustion factor is done in this study. Does this depend on the soil moisture index and the size of the fire?
Page 1492/Line3: How do you distinguish between flaming and smoldering burning?
Page 1494/Line1: The fuel consumption model for the surface fires considers ...” Is this not done with the SEIB-DGVM?
Page 1494/Line4: How do you derive EF and CF from NDVI?

Other remarks:

Abstract:
Page1484/Line24: “Without the atmospheric processing by acidic species” This needs more explanation even in the abstract. Why do you ignore atmospheric processing of dust particles by acidic species?

Model approach:
Page1489/Line 23: Point 4/5 are not an improvement of the Seiler and Crutzen approach.
Page1490/Line 20: “The aboveground (ground-layer) fires are assumed to last 1 day (8days)”. Can you explain more how the length of the fire enters your calculation and how this then relates to the daily fire count data?

Page1495/Line19: “When a number of smoke plumes were observed at different altitudes within the same model grid and for the same date, the relative amounts of emissions were calculated by weighting the contribution from each plume by its digitized area” Do you mean relative heights?
Page1496/Line 14:” combusted-generated aerosols as described in Ito and Feng (2010)”. Is this the same as the RETRO emissions mentioned earlier?

Page 1502/Line23: “Compared to the delivery of soluble iron from dust sourced without the atmospheric processing by acidic species....” Did you perform another experiment were this is taken into account?