Clim. Past Discuss., 8, C2648–C2650, 2012
www.clim-past-discuss.net/8/C2648/2012/

Climate of the Past Discussions

Interactive comment on "What could have caused pre-industrial biomass burning emissions to exceed current rates?" *by* G. R. van der Werf et al.

G. R. van der Werf et al.

guido.vander.werf@vu.nl

Received and published: 4 December 2012

We greatly appreciate the time and effort of the reviewers and editor. We have replied to each reviewer individually but would like to take the opportunity here to give an overview of the main changes we made.
The main criticism was related to the simple model we build to mimic GFED emissions. We build this simple model to 1) be more flexible with regard to changing fire return times, and 2) account for potential differences in NPP and thus fuel build-up between the current landscape where humans have a strong influence and the pre-industrial landscape where urbanization and landscape fragmentation were less important, probably leading to higher NPP in the pre-industrial, especially in areas with high population density now.

Two reviewers commented on this, stating either that the difference between CASA NPP and modeled NPP was large, or that the assumption we made that fuel carries over to the next fire season if there was no fire is wrong for grass-dominated landscapes. Here, all fuel may decompose in case it does not burn. We tested the sensitivity of our results to this by running the simple model with CASA NPP and by running it with fuels that are constant over time. The results are shown in a new Table (Table 4) and discussed in the uncertainty section (4.4).
In short, the two sensitivity runs did change the results but not the main conclusions. The run with CASA NPP indicated CO emissions at SPO were about 8 percent below the standard run for all fire return times. This difference was expected because the model predicted higher NPP values than CASA, which we feel can be explained for a part by lower human pressure on the landscape in pre-industrial times. Replacing NPP with CASA NPP would strengthen our conclusion that it is difficult to match the CO concentration found.
The run without fuel carry-over did not change the maximum amount of CO at SPO that could be due to biomass burning, which was expected because carry-over is not important when fires burn annually. However, it did lower fire emissions when fire return times were longer, up to a third for fire return times now found in savannas. This would deteriorate the agreement with GFED fire emissions to a large degree. However, GFED is also just a modeling framework with uncertainties and we agree with the reviewer that our approach of simulating fuels may be biased to woody savannas and have highlighted this in the methods and uncertainty section. But also this sensitivity analysis strengthened our conclusions with regard to the (I'm)possibility of landscape fires to account for the large increase in CO because as soon as the fire return time was longer than one year, emissions dropped substantially in case there is no carry over.
In addition, we re-assessed the MAP to NPP and MAT to NPP relations and now use the 90th instead of the 95th percentile to estimate NPP. As a consequence, we had to boost the fraction of NPP that is available for litter production from 0.50 to 0.55 to

maintain the fit with GFED.

Another point of concern raised by reviewer Jed Kaplan was that we did not account for charcoal production. We now state more clearly that this source of emissions might have explained part of the elevation found in CO.

There were a number of concerns about our paper that we feel were valid, but in our view of second order importance. The scope of this paper was to address whether with the current available knowledge we could boost biomass burning emissions from landscape fires so that CO emissions increased by a factor of 4, which required a 10-fold increase in savanna emissions. Our approach to find out whether this is possible has a large number of shortcomings just like any other modelling approach, and we have tried to state clearly what the uncertainties are. As the sensitivity analysis already points out, the main findings are robust. This does not mean that we dismiss the critiques, but we do hope the reviewers and editors agree with us that by showing most errors are likely to be relatively small we can be confident about our main conclusions. We have made a substantial effort to highlight these uncertainties throughout the text.

Finally, we have redone all figures as requested, increased the size of the maps, and changed them to the pdf format so they maintain high quality when zooming in. This also required to change Figures 2, 4, and 7 from scatter plots to 2-dimensional histograms to prevent the file size to exceed 10 Mb for each plot because of the large number of observations.

Interactive comment on Clim. Past Discuss., 8, 3159, 2012.