

Interactive comment on “Historic global biomass burning emissions based on merging satellite observations with proxies and fire models (1750–2015)” by Margreet J. E. van Marle et al.

Anonymous Referee #3

Received and published: 16 March 2017

The paper aims at providing a historical reconstruction of fire emissions from 1750 onward, as the basis for the CMIP6 climate modeling objective. This paper then focusses on updating the 1850-2000 fire emissions proposed for the CMIP5 exercise. To reach this goal, the authors use the GFED4s emissions data as the baseline for 1997 to present period. The backward trend line for the Tropical forest is based on newly delivered papers reconstructing fire emissions since the 1960's based on visibility indices. The global charcoal database is used for boreal and temperate forest of the northern hemisphere where the network of sample is the most significant and from a panel of DGVMs runs for the 1750-present period for all the other areas.

The objectives are timely, and the effort in assembling the state-of-the-art modelling

[Printer-friendly version](#)

[Discussion paper](#)



and charcoal communities deserves congratulations for proposing a synthesis. The strength of the paper in assembling 6 models, and readjusting the non-quantitative charcoal temporal variations to fit the final GFED4s time series, might also be however its main weakness. It is on one side a huge data assemblage, and on the other side a poorly investigated model intercomparison weakening the final message. Despite being well and clearly described, some assumptions remain confusing and potentially misleading. The total absence of link and usage of the MIP5 reconstruction is also frustrating.

The main assumption of the paper is that “fire models can be used to estimate biomass burning emissions on a global scale”(P4121-23) , and this also on a long temporal scale. In this sense, the paper contradicts itself when, in the end, comparing model’s performances to charcoal data on selected regions, and concluding on poor relationships. In absence of any other data, we might understand however to rely on this data resource. I have listed below the questions I am concerned with, which would require major corrections and significant additional information. Unfortunately, I think this approach would really deserve a deeper FIREMIP result understanding before being used for this purpose.

When going through the 3 main methodological tasks used for the reconstruction, I have the following questions:

1. **Visibility:** this interpolation based on two published papers linking visibility to GFED emissions for the period 1997-present and extending backward to 1960’s in south east asia and Amazonia is really convincing, both in terms of temporal trend and interannual annual variability. In this sense, this is a significant update to the MIP5 reconstruction. It would be interesting though to have this comparison with MIP5 for all regions, to clearly understand the added value of this synthesis (as performed in figure 14). I have just a little concern that the Van Marle et al. (2017) paper used for this reconstruction analysed only a portion of the ARCD region showed in figure 2. Peru and Eastern Brazilian (fire-prone cerrado savannas) don’t seem to be included in this temporal trend

[Printer-friendly version](#)[Discussion paper](#)

reconstructed from visibility. =>How did the authors deal with this other part of the ARCD region, still representing a significant surface?

2. charcoal-based reconstruction The authors used the global charcoal database, providing a general trend in historical charcoal deposition in sediments from vegetation fires, with increasing time resolution allowing for decadal understanding of fire history. The authors selected the regions with a significant amount of data, which is a fair assumption. The main weakness of this dataset is the missing quantitative information so the authors had to rescale the Z-scores of the charcoal database to the emissions. The method is described in p17. We get a little confused p17/9-10 with the sentence “the normalized charcoal signal (CCnorm) is the unitless charcoal influx Z-score on a decadal time step normalized per region and year”. This is minor, but decadal and yearly time step sound confusing to the reader. That should be rephrased. When looking at Power et al. 2010 and Marlon et al. 2016 papers, Z scores vary below 0 and above 1, so I guess these values have been reduced to the 0-1 interval. Is that correct? Maybe rephrase as we understand, as written, that Zscores are directly between 0 and 1 in the raw data. To rescale the Z score, the authors then assume that the maximum Z-score corresponds to the 75th percentile of FIREMIP models and the minimum z score to the 25th percentile in equation 2. This assumption is then thoroughly and properly discussed later. We wonder however in Equation 3 p17, why CCscaled is based on CCfireMIP of the year 2000 and not the mean 1997-2003 period as FIREMIPscaled (equation 1)? The output from this rescaling is finally a 10-year smooth average, without any interannual variability (as shown in figure 10 for example). Then why not using the FIREMIP interannual variability to produce this missing variability on the smooth charcoal trend? For the EU region, the charcoal database is used. Samples are distributed across Europe, while burned area is mostly located in the south on the mediterranean part. Are the charcoal sample locations weighted according to present observed burned area for example to give more weight to the Mediterranean? If not how biased could be the result? For north America, The method is clearly described and discussed so that could be convincing. I still wonder here,

however, why the authors did not use the forest fire statistics from US and Canada and reconstructions of burned areas going back in time for almost a century in these regions widely documented to rescale the minimum and maximum emissions? These data have been used in MIP5 and in my opinion would have greatly benefited here to strengthen the decision of this 25th and 75th percentile, and make a link to the previous version.

3 DGVMs historical runs In absence of any substantially reliable information, the authors decided to use the FIREMIP runs. The choice is clearly stated in the methods. It then covers a very significant portion of the globe (Africa, south America beside Amazonia, Asia, and Australia) and a large portion of the global burned area. Figure 3 could be rearranged proportionally to burned area, so that the reader clearly visualize that the global burned area reconstruction relies mostly (round 75%) on models. . . I am not against this idea, but in turn, the reader is left a little disappointed and questioned as the paper doesn't analyse at all models assumptions and specificities. The authors give us the huge variability from the models (which is disappointing but actually in the range of uncertainties in climate model projections) and we don't really know what is climate-driven, human-driven and why each model has this trajectory. Analyzing all this would require one full (or even several) papers from this modelling group so they give us further information. and it's a huge task. I might understand the rush to provide CMIP6 data for burned area emissions, but this chapter leaves the reader very frustrated, if not suspicious on the reliability of these data for this purpose. I guess the authors would argue that it's still better than the empirical reconstruction from MIP5 and the linear trend used before 1900. when looking at figure 5 and the 1750-1900 trend, it's not obvious that the authors have achieved a fundamentally innovative trend compared to MIP5.

When going into details on this chapter, I have the following questions:

- P12 I2: FIRE MIP runs DGVMs from 1700 to 2013. GFED from 1997 to 2015. The overlapping period is 1997-2013. Why using 1997-2003 further on (line 5) as an over-

[Printer-friendly version](#)[Discussion paper](#)

lapping period? - timing of interannual variability: I was expecting that, if the trend is not overwhelmingly different from the flat trend of MIP5, we would get the actual interannual variability in time and amplitude from this approach. We also get a little disappointed as all experiments used repeated 1901-1920 forcings from the beginning of the simulation (1750) to 1900. In this sense, figure 5 is misleading and should better be presented as a moving window decadal values with uncertainties (SE or coeff of variation), as the variability is not timely. Also why minimizing interannual variability (P12 L12-L14) on purpose? The authors in additions discuss about the increasing interannual variability but the trend of this variability in figure 5 is all fake. This should not be taken for granted as: a) considering the mean when emission simulations are not timely in phase for each model (figure 7 for example) intrinsically reduces the interannual variability (lower than each model's interannual variability) , b) the charcoal time serie is flat (discussed above). Why do the authors provide this 'fake' interannual variability ? is that a request from the CMIP6? It would be worth, in the introduction for exemple, to present the CMIP6 'wish list' to better understand the choices performed in this reconstruction. We are also questioned that the authors used the 25th and 75th percentiles for charcoal reconstruction using FIREMIP models , so that "outliers did not influence the scaled regional charcoal signal" (P15L15). We then wonder why this was not also done for equation 1. In conclusion for this modelling chapter, if we can knowl-edge the effort of the authors to assemble all this information, the conclusions seem way too overrated and we miss a lot of the understanding of this model intercomparison to fully appreciate the synthesis. The interannual variability is an important point that is completely misrepresented in the final results and misleading for the readers.

Discussion: The discussion is interesting and actually provides more interesting information than the results themselves. However, it also highlights the weakness of the results. P32 I1: we wonder if the visual trend is actual or driven by the "fake" interannual variability. Statistical time series analysis could reinforce this sentence, but with a wrong interannual variability they will be also biased.

[Printer-friendly version](#)[Discussion paper](#)

P32 I13-14: “after which emissions stabilized, probably as a result of increasing CO₂ concentrations and changes in population density as input parameters” This sentence clearly illustrates my comments on the poor analysis of the models functioning. It is very difficult here to understand and have an opinion based on the information provided in the paper (neither by reading hantson et al 2016 and Rabin et al describing the models): why increasing CO₂ would stabilize fire emission? For SAH, different trends are observed in models. . .but all are driven by population (at least ORCHIDEE and LPJ GUESS SPITFIRE are coupled with the same SPTIFIRE but with the most opposite trends. . .). A full model output analysis would be worth being published before this paper, to strengthen the message.

Figure 13 p 33: Using the Andela and van der Werf (2014) hypothesis seems to be a fair option to reconstruct fire history actually for Africa. That’s a nice result. Why not choosing this trend the same way the authors did with charcoal? This would completely reverse the global increasing trend obtained from the FIREMIP into a decreasing trend, and would fit the charcoal Tierney (2010) trend. That sounds convincing. How is cropland area introduced in DGVMs? If not included, there is no reason to value the model hypothesis rather than the Andela paper. This paragraph is again both exciting as the authors seem to have found a smart proxy fitting the charcoal but they don’t use it, but also disappointing as it weakens the model’s approach, that we are not able to fully appreciate due to a lack of deep analysis.

The final discussion chapter on the comparison with MIP5 is welcome (at last!). Too bad it’s partial and only focused on few areas. A final comparison on the MIP5 and MIP6 would be also interesting. . . as the MIP6 seems to be flat before 1900, and it sounds like it would be very similar to MIP5 in the end.

Some few minor additional comments: P3L8 : the varying constraint hypothesis from krawchuk and moritz 2011 would be a better reference in addition or replacement of van der werf 2008. P4I21-23: this is a critical assumption that “fire models can be used to estimate biomass burning emissions on a global scale” on a historical point of

[Printer-friendly version](#)[Discussion paper](#)

view... maybe review some recent papers trying to compare historical trends (Yue et al., Kloster et al., Yan et al.). P18 | 22: IAV? Does it mean interannual variability?

P38: figure 14: just wondering if charcoal Z-scores should be rescaled to the 50 year average of burned area from Mouillot & field and C emissions from your study to better rescale the temporal trend, instead of year 2000.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2017-32, 2017.

GMDD

Interactive
comment

Printer-friendly version

Discussion paper

