Dear reviewer,

We greatly appreciate the comments of the reviewer, please find below our response to the issues raised.

Kind regards,

Margreet van Marle

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 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.

As the reviewer mentioned, there has been substantial progress since the MIP5 reconstruction was produced by Lamarque et al. (2010). Our aim was to make best use of that new information which has most certainly led to improvements in several regions. At the same time, uncertainties remain substantial and we had to made a number of rather arbitrary choices which we have described as good as possible and we included sensitivity studies to estimate the impact of those choices.

Our estimates and the CMIP5 are actually more in line than the reviewer suggests. Both started using GFED but obviously using different version (2 *versus 4s) and both went back in time using other datasets, which to some degree compare reasonably. By expanding the discussion on the newly presented dataset and MIP5 and further clarifying some of the assumptions we hope to have taken away most of the concerns of the reviewer.*

The main assumption of the paper is that "fire models can be used to estimate biomass burning emissions on a global scale"(P4l21-23), 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.
The reviewer is right, and we may have not chosen our wording properly We have now rephrased this to: "fire models are also used to estimate biomass burning emissions on a global scale".

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.
Ideally all the different models would have been evaluated before being used for an exercise like ours. This is actually done in FireMIP but it may take a number of years before those results become available. In the meantime, CMIP6 requires estimates that are based on the best knowledge currently available and that is what we have done. We believe science is an incremental process and we highlighted in several sections that uncertainties are substantial and that future reconstructions may be different, just like ours is different (but based on better science, especially in those areas where new constraints have emerged) than previous ones

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).
We appreciate the suggestions and have added regional comparison in the supplement to better inform the reader about differences between our estimates and previously used fire emissions estimates for CMIP. Reviewer 1 also raised this point. The figure is inserted below as well and we have added the following text to the discussion (P34L24):

“Although the global trends are relatively similar, on a regional scale differences between our estimates and the data used in CMIP5 are more substantial (See Figure D1, with regional comparisons between CMIP5 and CMIP6 estimates in Appendix D), with the largest differences in TENA-E, TENA-W, SHAF and SARC. In Africa, the continent of which half of all carbon emissions stem, we found that emissions were relatively flat while CMIP5 estimates increased over the past decades, at odds with recent findings that agricultural expansion lowers fire activity (Andela and van der Werf, 2014)*. The estimates and trends in EQAS, CEAS BONA-W, BONA-E are very similar, just as the estimates in ARCD, although in our estimates the increase there started a few decades later. While our estimates are for several regions driven by consistent data sources, these substantial discrepancies highlight once more that uncertainties are large”.*

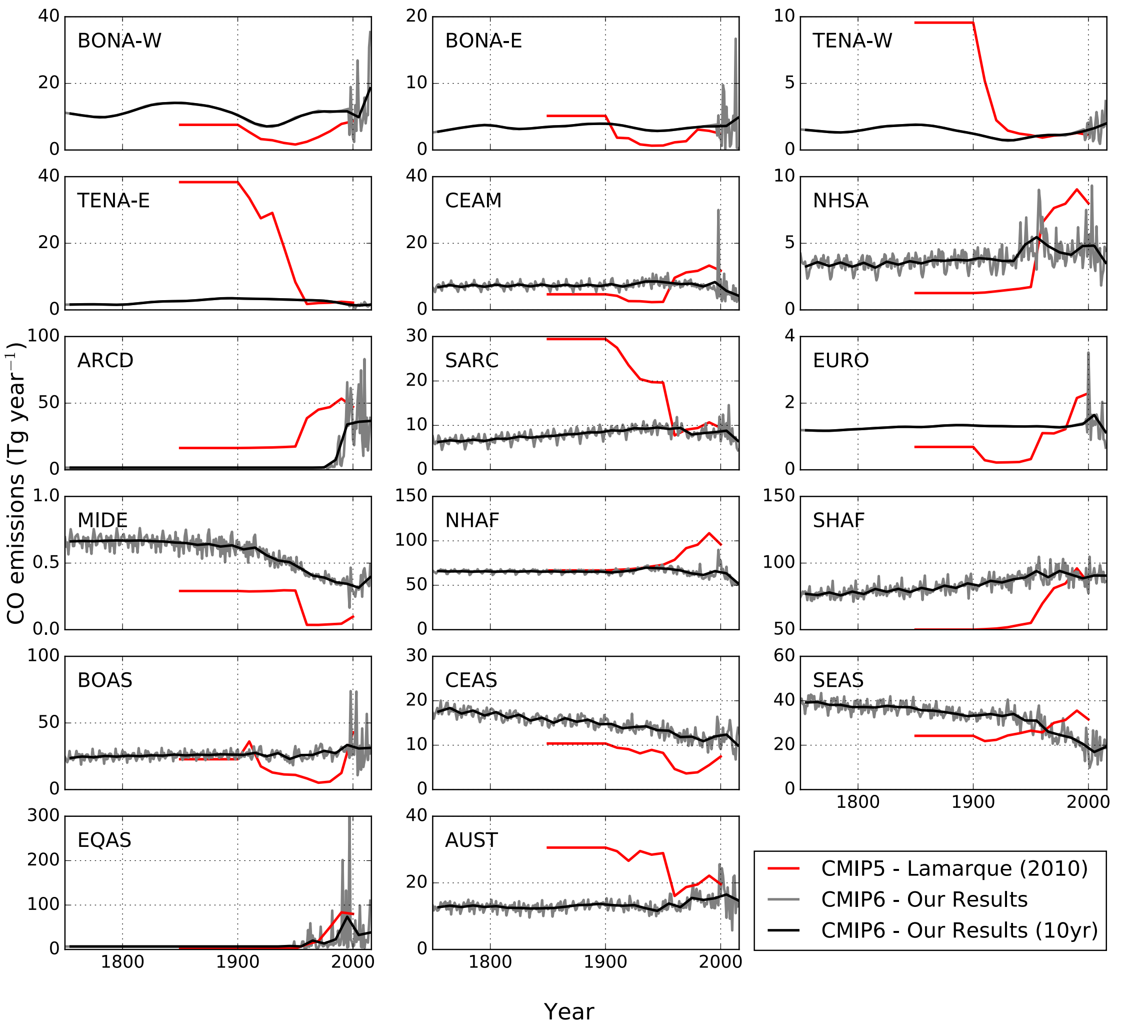

Figure D1 Regional carbon monoxide biomass burning emissions estimated by Lamarque et al. (2010) for CMIP5 and our results (CMIP6) on an annual and decadal time step.

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 reconstructed from visibility. =>How did the authors deal with this other part of the ARCD region, still representing a significant surface?
For this purpose we divided the GFED region 'Southern hemisphere South America' into a region dominated by deforestation (Arc of Deforestation, ARCD) and a region further south where cerrado fires, amongst other, occur. This was labeled South of the Arc of deforestation (SARC). Currently, fire-driven deforestation is the main source of carbon emissions in ARCD and compared to deforestation emissions other fire sources play a small role (Morton et al., 2006, 2008; van der Werf et al., 2010)*. van Marle et al. (2017) showed that fire emissions were low up until the late seventies when deforestation practices started. Before 1973 (when visibility observations became available) we kept emissions at the lowest decadal value and assumed that this baseline corresponds to fires from cerrado burning.*

We agree with the reviewer that these uncertainties could be described in more detail.

Therefore we added to the Methods P16L01:
"In ARCD deforestation emissions dominate the fire emissions, but additional emissions stem from cerrado burning. We assumed that fraction corresponds to our baseline emissions in the 1970s when deforestation was low and was kept constant before that period.'

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 p17l9-10 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.
We agree and rephrased p17 3-4 to in the revised manuscript to:
"The charcoal records were converted to unit less time series, with a range between -1 to 1, with a decadal time step using methods detailed in Power et al. (2010). The decadal time series was linearly interpolated to annual values and subsequently scaled to the output of the modelled data described under 2.2 following Eq. 2:"

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 rephrased p17 3-4 to: "The charcoal records were converted to unit less time series, with a range between -1 to 1, with a decadal time step using methods detailed in Power et al. (2010). The decadal time series was linearly interpolated to annual values and subsequently scaled to the output of the modelled data described under 2.2 following Eq. 2:"

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 charcoal data is available on a decadal time step, with $CC_{FireMIP}$ the scaled charcoal data to the 75^{th} and 25^{th} percentile of FireMIP data. To get the annual values we linearly interpolated the decadal charcoal observations. Taking the average of $CC_{FireMIP}$ over 1997:2003 would imply that we used annual observations and would furthermore not result in different outcomes than using the observed 2000 value.

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? *The charcoal-based time series only provides values on a decadal time step. We decided only to use the FireMIP interannual variability in regions where also the trend is scaled based on FireMIP data. For regions where charcoal records were used we refrain from using the FireMIP interannual varability to clearly emphasize that the underlying charcoal trend is based on decadal data. Also, adding the interannual variability based on the FireMIP data would add additional uncertainty to the estimates in these regions, because this will require additional (rather arbitrary) assumptions on the size of the interannual variability.*

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? –
Following earlier work (Marlon et al., 2016) *we have not weighted the individual records but have strived to make the regions as small as possible. In North America the signals within the original GFED basis regions diverged and there we split those regions. That was not the case in Europe so there was no need to weigh them. In addition, Europe contributes 0.4% to total global emissions so*

even a slightly different reconstruction would have negligible impact on the global signal

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.
We thank the reviewer for this suggestion and agree this might have been a valuable dataset. One of these datasets is the Canadian Fire Database (CNFDB), which nicely extends the satellite era decades back in time. While this dataset provides more spatial detail than our approach, users are also warned by CNFDB that the data in the CNFDB is not complete and not all fires have been mapped and data accuracy varies. The collection only includes data that has been contributed by agencies and completeness and quality may vary among agencies and between years. This makes the dataset less useful for our purpose than initially thought. Adding this dataset would need a thorough regional comparison with the charcoal time series and satellite-based emissions.
We added to the discussion P36L08: "Furthermore, an in-depth comparison between forest fire statistics from the US and Canada, for example the Canadian Fire Database (CNFDB, Stocks et al., 2002) and the charcoal time series may help constrain the uncertainty in boreal and temperate North America."

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.

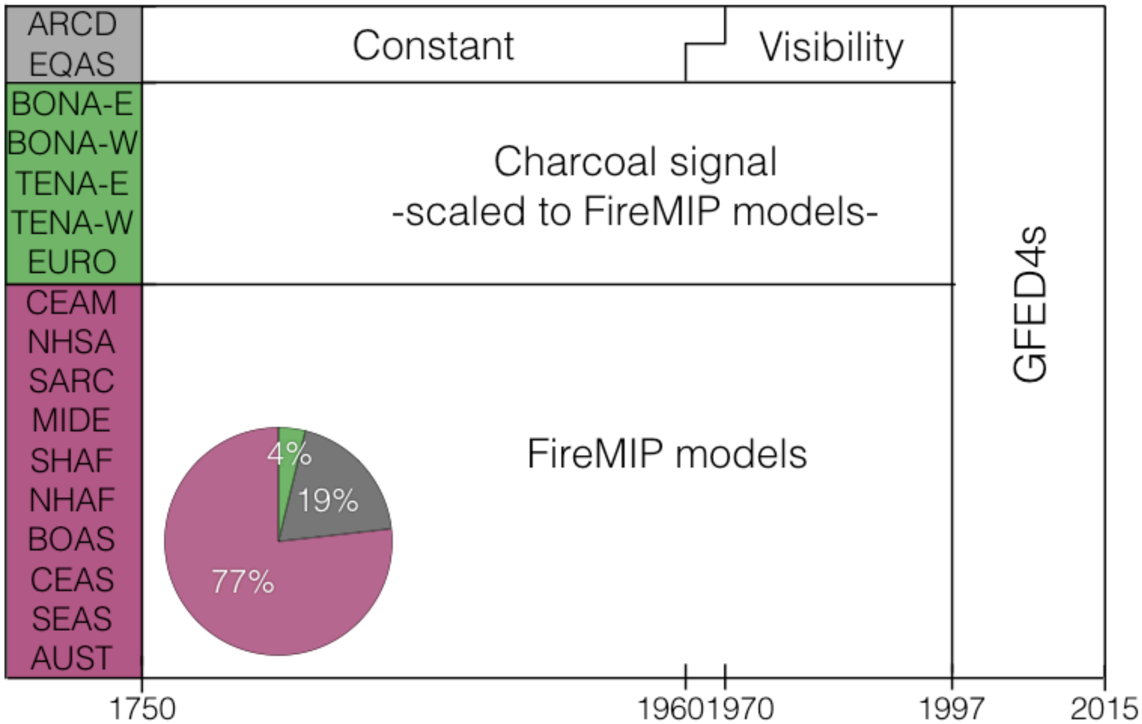

Figure 3: Data sources used for each region. The pie chart represents the contribution of the modelled regions (purple), charcoal regions (green), and visibility-regions (grey) to the GFED totals over 1997 to 2015.

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. –

We agree with the reviewer that for those regions where the fire models were used our results may not be a clear improvement compared to earlier work. This is now mentioned more clearly in the text by adding in the discussion P36 L08: "Future model comparisons pinpointing the reasons why models behave differently would help constrain this uncertainty."

Furthermore we compared the MIP5 and MIP6 results on a regional scale (see comment before) and added to the Discussion P34L24: "Although the global trends are relatively similar, on a regional scale differences between our estimates and the data used in CMIP5 are more substantial (See Figure D1, with regional comparisons between CMIP5 and CMIP6 estimates in Appendix D), with the largest differences in TENA-E, TENA-W, SHAF and SARC. In Africa, the continent of which half of all carbon emissions stem, we found that emissions were relatively flat while CMIP5 estimates increased over the past decades, at odds with recent findings that agricultural expansion lowers fire activity (Andela and van der Werf, 2014)*. The estimates and trends in EQAS, CEAS BONA-W, BONA-E are very similar, just as the estimates in ARCD, although in our estimates the increase there started a few decades later. While our estimates are for several regions driven by consistent data sources, these substantial discrepancies highlight once more that uncertainties are large".*

We also provided sensitivity analyses to the number of models included in the trend derivation. For Africa (~50% of total fire carbon emissions) we do feel we have improved as the multi-model mean is more in line with recent findings about the role of agriculture in suppressing fires (Andela and van der Werf, 2014) *than MIP5 where biomass burning there increased over time (Figure D1).*

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

- P12 l2: 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 overlapping period?
We used the 1997-2003 period as benchmark to scale the modeled data to and we prefer to use GFED data from 1997 onwards. To use the GFED data as benchmark we decided to scale the 2000 value of the modeled data, because there is still some overlap with the GFED time period. We do understand this decision is rather arbitrary, but using the whole overlapping time period with the models would result in a mismatch when stitching the modeled data to GFED, because trends in the time period over 1997-2013 occur. We also rephrased P12 L12-14 to: "We used the average over 1997 to 2003 when combining the various data streams to minimize the impact of interannual variability in the GFED time series, which could result in a mismatch when stitching the FireMIP emissions to the GFED data."

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.

We agree with the reviewer that the 20-year cyclic meteorological forcing should be mentioned more clear in the text. Therefore we added to the discussion P32L02: "Meteorological forcing data was only available for the year 1900 onwards. The interannual variability before 1900 stems from a 20-year repetitive cycle in meteorological forcing (1900-1919)."

Although the IAV in the FIreMIP data is based on a 20-year repetitive cycle for the meteorological forcing before the year 1900, other forcing data such as land-use, population density and CO_2 concentrations were available before 1900.This provides information based on the model output we would like to keep included.

We investigated the effect of taking the 20-year running mean over every modeled time series on the regional and global results. On a global scale the differences are marginal with 0.2%. On a regional scale the differences go up to 7% in NHSA, although this region contributes only 1.4% to the global totals. We prefer to keep our results including IAV, however as described above we will describe the 20-year repetitive cycle more clear in the text.

Table R1 – Carbon emissions based on using the models including interannual variability (IAV), models with a 20-year moving window and the difference relative to the current estimates (IAV-based).

		Average emissions (incl. IAV) (Tg C year^{-1})	Emissions (20-year moving window) (Tg C year^{-1})	Relative difference (%)
BONA-W	Boreal North America – West	41.1	39.7	3.2
BONA-E	Boreal North America – East	12.5	12.1	3.2
TENA-W	Temperate North America - West	8.4	8.4	0.9
TENA-E	Temperate North America – East	14.1	13.7	2.9
CEAM	Central America	44.5	44.0	1.2
NHSA	Northern Hemisphere South America	26.4	28.4	-7.7
ARCD	Arc of Deforestation	57.7	57.7	0
EURO	Europe	7.0	7.1	-1.2
MIDE	Middle East	3.1	3.1	0.6
NHAF	Northern Hemisphere Africa	475.4	475.4	0.01
SHAF	Southern Hemisphere Africa	623.3	615.8	1.2
BOAS	Boreal Asia	101.3	104.7	-3.4
CEAS	Central Asia	78.2	80.6	-3.1
SEAS	South-East Asia	207.3	207.1	0.2
EQAS	Equatorial Asia	47.3	47.3	0
AUST	Australia	97.4	97.2	0.2
SARC	South of Arc of Deforestation	51.3	50.8	0.9
GLOBE	Sum of all regions	1896.4	1893.0	0.17

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.

As described before after 1900 the IAV in the FireMIP data is based on meteorological forcing data. Only before 1900 the IAV in the FireMIP data is based on a 20-year repetitive cycle for the meteorological forcing, although other forcing data such as land-use, population density and CO_2 concentrations were available before 1900.

The sentence written on P12 L12-14 is to explain how we stitched the modeled data to the GFED data. The GFED data has interannual variability and just matching the modeled data to the 2000 value of GFED observations would result in a mismatch, because the models don't exhibit the same inter annual variability. Therefore we used an average over 1997-2003. We feel this comment was the result of a misunderstanding so we rephrased the section to be more precise: "We used the average over 1997 to 2003 when combining the various data streams to minimize the impact of interannual variability in the GFED time series, which could result in a mismatch when stitching the FireMIP emissions to the GFED data."

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) , *All models used identical meteorological forcings as such the emission simulations are timely in phase for each model.*

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 perfomed in this reconstruction. *This comment links to an earlier comment raised by the reviewer. We investigated the effect of taking the running mean over every modeled time series on the regional and global results (Table R1), which shows that the differences are marginal.*

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 Equation 1 we did not scale the modeled data, because they have their own upper and lower limit corresponding to emissions. Charcoal needed the scaling in order to get values corresponding to the Z-scores and the models needed the scaling to match the GFED data. Furthermore we took the median for the modeled regions, which in turn reduces the effect of outliers.*

In conclusion for this modelling chapter, if we can knowledge 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.
We agree with the reviewer this should be highlighted more. Therefore we added to the discussion P32L02: "The interannual variability before 1973 stems from a 20-year repetitive cycle in climate forcing used in the models."
We highlighted the need for future model studies by adding in the discussion P36 L08: "Future model comparisons pinpointing the reasons why models behave differently would help constrain this uncertainty."

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 l1: 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.
We have estimated regional and global carbon emissions based on the data presented and the datasets smoothened (see Table D1) showing that the difference is marginal.

P32 l13-14: "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?

We agree with the reviewer and have eliminated the speculative part: "The multi-model median indicated that Southern Hemisphere Africa (SHAF) had an increasing trend from 1750 until ~1950, after which emissions stabilized."

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.

All models performed differently although the input datasets were similar. The FireMIP community is currently working on detailed intercomparison analyses and benchmarking practices. Although the exact pattern in models is unclear the models provide currently the most continuous datasets and are the sources to rely on especially in regions where little is known about fire history. We do make the assumption that the median is most representative, but until detailed model intercomparison analyses are done we don't know which model performs where best. We agree with the reviewer that this is a limitation of our results. We highlighted this comment more by adding in the discussion P36 L8:
"Future model comparisons pinpointing the reasons why models behave differently would help constrain this uncertainty."

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.

We agree that the Andela and van der Werf (2014) method provides insight into fire behavior in Africa. However their method is solely based on the satellite era. Patterns and causes of fires in Africa might have changed over the century. Our method yields a somewhat different trend. However we do agree this highlights the uncertainty of the global trend, which is for a large part based on the African signal. Therefore we added the following sentence to the discussion P32L25:
"Future research into the drivers of African fires and how these have changed over time could help would improve our estimates."

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. *In previous versions of the dataset we indeed used agriculture as a proxy. After discussions with the fire modelers this was changed mostly because we felt it was inappropriate that over 250 years of fire emissions were a function of only 1 parameter, given that over the same time frame several other crucial parameters (grazing, CO_2 fertilization, other land cover changes) have changed. We totally agree that no model models this perfectly but at least most factors are accounted for including changes in cropland area. Again, this is a subjective decision and the reviewer is right in questioning this, but any other choice would have been subjective as well. By highlighting three different approaches (fire model mean, agriculture as a proxy, and charcoal) to estimate emissions from Africa we have highlighted the uncertainty in this.*

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.
We compared the MIP5 and MIP6 results on a regional scale (see comment before) and added to the Discussion P34L24: "Although the global trends are relatively similar, on a regional scale differences between our estimates and the data used in CMIP5 are more substantial (See Figure D1, with regional comparisons between CMIP5 and CMIP6 estimates in Appendix D), with the largest differences in TENA-E, TENA-W, SHAF and SARC. In Africa, the continent of which half of all carbon emissions stem, we found that emissions were relatively flat while CMIP5 estimates increased over the past decades, at odds with recent findings that agricultural expansion lowers fire activity (Andela and van der Werf, 2014)*. The estimates and trends in EQAS, CEAS BONA-W, BONA-E are very similar, just as the estimates in ARCD, although in our estimates the increase there started a few decades later. While our estimates are for several regions driven by consistent data sources, these substantial discrepancies highlight once more that uncertainties are large".*

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. *We thank the reviewer for the suggestion and added the paper as reference.*

P4l21-23: 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 view. . . maybe review some recent papers trying to compare historical trends (Yue et al., Kloster et al., Yan et al.). *We rephrased this sentence (see earlier comments) to:' "fire models are also used to estimate biomass burning emissions on a global scale"*

P18 l 22: IAV? Does it mean interannual variability? *We defined IAV where it was first introduced at P03L06.*

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*. In Figure 14 the three datasets (Charcoal Z-scores, Mouillot and Field and Our estimates) were normalized and scaled to their 2000-values. The three datasets are for these regions independent of each other and this way it is possible to compare the trends as objective as possible.*

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