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 #1

Received and published: 9 March 2017

This paper provides a description of the biomass burning emissions that are provided for the upcoming CMIP6 simulations. The authors have done an excellent job of providing in-depth description of the methodologies used to generate the emissions. This was a gargantuan task and the authors should be congratulated to achieving this. I have a small number of minor comments below.

My main complaint is that the emissions are showing fairly significantly different trends from the CMIP5 dataset and it would have been very useful if some model simulations (or at least estimates of radiative forcing) had been performed to understand the consequences of these different trends. I understand that this probably beyond the scope of this paper, but it is still a shortcoming worth mentioning.
This dataset will be used in the CMIP model simulations and presenting the results from that exercise is indeed beyond the scope of this paper. We have, however, added a more general statement to the conclusions: "Our results point towards less variability over time than the fire emissions used in CMIP5 and a smaller difference between pre-industrial and present fire emissions, lowering the impact on changes in atmospheric composition and potentially lowering overall radiative forcing".

Minor comments
Page 2, line 23: CMIP is not part of IPCC. It is part of WCRP (see Eyring et al., GMD, 2016)
We will change this to: 'Will be used in the CMIP6 simulations.'

Page 11, line 13: how large was the scaling when applied? Might be good to mention the scaling algorithm (Eq. 1) at this point. Since 1997 was such a large emission year, has its role been evaluated?
The scaling was done using Eq. 1. To be more specific we have added a reference to this in the sentence the reviewer mentioned: "were scaled (Eq. 1) to GFED4s." Scaling was based on the average of 6 years (1997-2003) as representative for the 2000 value of the models. An average was used to smoothen the effect of regional differences and the effect of interannual

variability over the first years. The reason that 1997 was such a high fire year stemmed mostly from one region (Equatorial Asia where the El Niño induced drought that year led to record high emissions mostly from peat burning). For this region Eq. 1 is not used but is reconstructed using visibility observations. We have added to P12 L09: "where $FireMIP_{scaled}(reg,yr,mod)$ is the scaled regional model output on an annual time step and $FireMIP_{1997:2003}(reg,mod)$ is the average regional estimate for 1997-2003. While this 7-year time period included the highest fire year, 1997, fire emissions in that year stem mostly for peat fires in Equatorial Asia for which Eq. 1 is not used to reconstruct fire emissions (See Sect 2.3)."

Section 2.3: it seems that it would be useful to have more details on the methods used to extract emissions from visibility data? How does this work in anthropogenically polluted areas?
We refer the reader to the papers on which this scaling is based (Field et al., 2009; van Marle et al., 2017) *for more details in the methods. We agree with the reviewer that other sources impact visibility but we found these were of much smaller amplitude and do not influence the seasonal pattern used in our approach. Specifically, in both EQAS and ARCD visibility observations in low fire years at the end of our study period returned to similar levels as low fire years early in the study period indicating that other sources were of secondary importance.*

Page 17, lines 26-27: any suggestions on how models could integrate that recommendation? 'When fire modules are embedded in climate models they may be in a better position to include some spatial and temporal variability based on simulated weather.'
Climate models that include fire models can calculate emissions directly, which may better capture spatial and temporal variability due to, for example, modeled weather patterns. We therefore inserted that sentence. There is no need for integration, because those models will not use our emissions estimates. To avoid confusion, we have rephrased the sentence to: "Those climate models that already have fire modules and calculate emissions directly may be in a better position to include some spatial and temporal variability based on simulated weather."

Page 18: change link to emission factors to an actual description in supplement. Web link will break over time
We added a table with the emission factors used for the different species in the appendix and refer to this in the text.

Comparison with CMIP5: it would be greatly helpful if regional comparisons were also shown, maybe simply in the supplemental material
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. 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".*

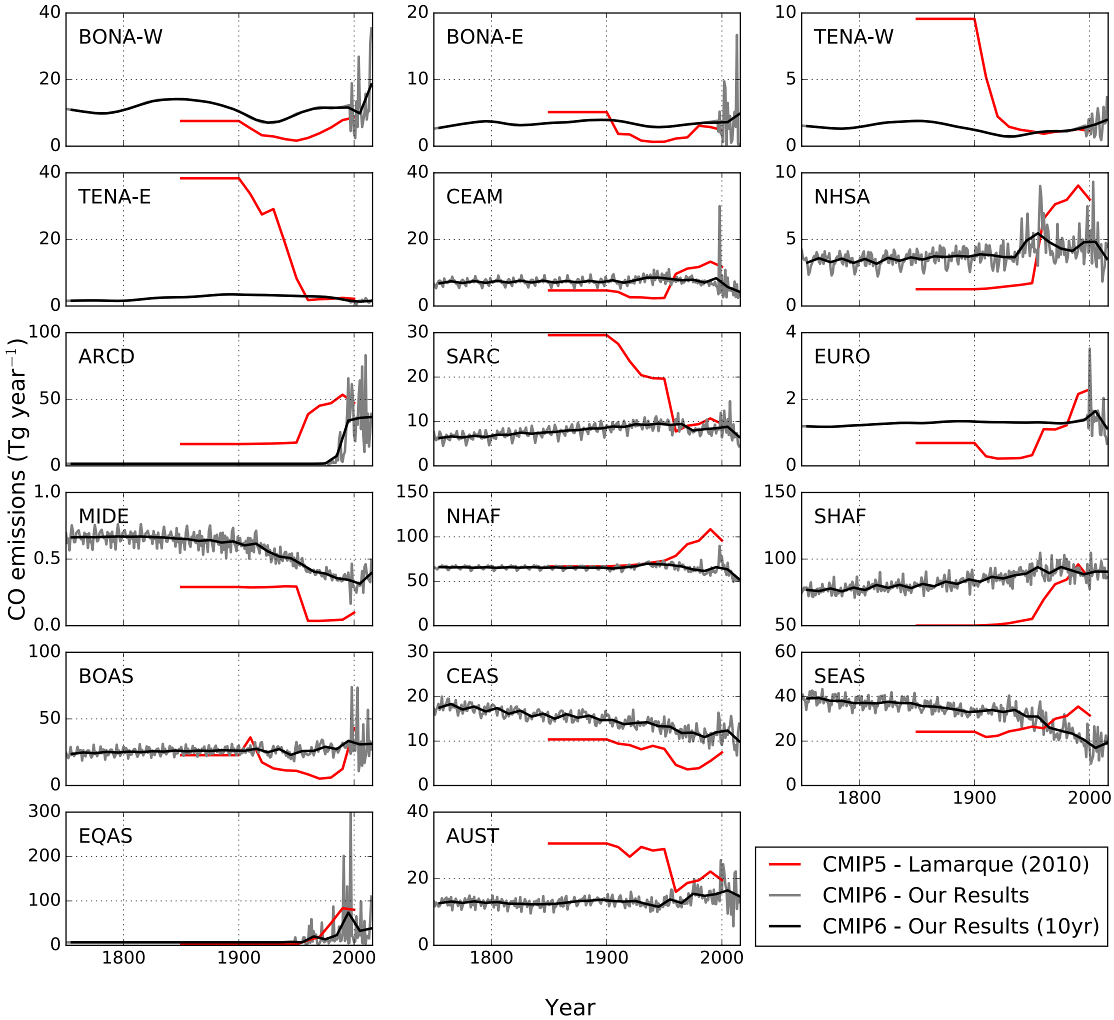

Figure D1 Regional carbon monoxide biomass burning emissions estimated by Lamarque et al. (2010) for CMIP5 and our results (CMIP6) on an annual and decadal time step.

References:

Andela, N. and van der Werf, G. R.: Recent trends in African fires driven by cropland expansion and El Niño to La Niña transition, Nat. Clim. Chang., 1–5, doi:10.1038/nclimate2313, 2014.

Field, R. D., van der Werf, G. R. and Shen, S. S. P.: Human amplification of drought-induced biomass burning in Indonesia since 1960, Nat. Geosci., 2,

185–188, doi:10.1038/ngeo443, 2009.
van Marle, M. J. E., Field, R. D., van der Werf, G. R., Estrada de Wagt, I. A., Houghton, R. A., Rizzo, L. V., Artaxo, P. and Tsigaridis, K.: Fire and deforestation dynamics in Amazonia (1973-2014), Global Biogeochem. Cycles, 31, 24–38, doi:10.1002/2016GB005445, 2017.