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Interactive comment on "Six Global Biomass Burning Emission Datasets: Inter-comparison and Application in one Global Aerosol Model" *by* Xiaohua Pan et al.

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Response to Referee #1

We really appreciate the constructive comments/suggestions from Referee #1, which will greatly help us to improve this manuscript. Following each of the Referee's suggestions <Referee>, we have provided our responses <Response>.

<Referee> Anonymous Referee #1 Received and published: 16 August 2019 The authors have run the same atmospheric model with 6 different biomass burning (BB) inventories and analysed the differences using AOD and aeronet. These differences

are often substantial and to some degree the authors have pointed to reasons why those differences exist. I feel the paper helps other modellers in understanding where some of the uncertainties in biomass burning emissions originate from but at the same time I feel the reader is left a bit wondering what the main messages are in the end. Ideally one would come up with recommendations about when and where to use a certain dataset, or when and where to avoid those. But given that the dataset to evaluate the results is also used to construct some this may be too much asked. Please find below a number of suggestions to further improve the paper.

<Response> The six BB datasets analyzed in this study differ in various ways and scales across different biomass burning regions and seasons. Hence, it is challenging to come up with comprehensive recommendations about when and where to use or avoid a particular dataset. Nevertheless, we agree that some recommendations, even in general terms, would be beneficial to the community. Thus, we have added the following statement towards the end of the abstract:

"Although model simulations based on QFED2.4 show overall closest agreement with satellite AOD retrievals, we recommend FEER1.0 for aerosol-focused hindcast experiments in the two biomass-burning dominated regions in the southern hemisphere, SHAF and SHSA (as well as in other regions but with lower confidence), mainly because QFED2.4 is tuned with the GEOS model, whereas FEER1.0 is derived in a more model-independent fashion and is more physical-based since its emission coefficients are independently derived at each grid box." Discussion paper

<Referee>: First sentence in introduction is spelled a bit awkward, please break up in two. Likewise for the second paragraph (L79).

<Response> The first sentence in introduction has been modified to:

Biomass burning (BB) is estimated to contribute about 62% of the global particulate organic carbon (OC) and 27% of black carbon (BC) emissions annually (Wiedinmyer et al., 2011). Therefore, biomass burning emissions significantly affect air quality by acting as a major source of particulate matter (PM), and the climate system by modulating solar radiation and cloud properties.

the second paragraph in introduction has been broken up into:

With the advent of satellite remote sensing of active fire and burned area products in the last couple of decades, a number of global BB emission datasets based on these observations have become available (e.g., Ichoku et al., 2012). Six of such major BB datasets will be compared in this study, including three datasets based on burned area approaches, namely, the Fire INventory from NCAR (FINN, Wiedinmyer et al., 2011) and two versions of the Global Fire Emissions Database (GFED, van der Werf et al., 2006, 2010, 2017), and three datasets based on fire radiative power (FRP) approaches, namely, the Global Fire Assimilation System (GFAS, Kaiser et al., 2012) developed in the European Centre for Medium-Range Weather Forecasts (ECMWF), and two National Aeronautics and Space Administration (NASA) products, i.e., the Fire Energetics and Emissions Research algorithm (FEER, Ichoku and Ellison, 2014) and the Quick Fire Emissions Dataset (QFED, Darmenov and da Silva, 2015).

<Referee> 159: Not sure why that small fire paper is cited in the GFED3 description

<Response> We have removed “Randerson et al., 2012”.

<Referee>: 208: Kaiser et al: : :, -> Kaiser et al.,

<Response> This has been corrected.

<Referee> The link on L213 does not work, at least not on my two computers

<Response> They changed the website address recently. Sorry about that. This link in the revised version has been updated as: https://confluence.ecmwf.int/display/CKB/CAMS++Global+Fire+Assimilation+System+%28GFAS%29+data+documentatio

<Referee> L282: I am a bit surprised that BB aerosols are injected near the surface. There is quite a bit of literature showing the importance of injection heights in for ex-

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ample the Boreal region

<Response> We agree that this is a concern. Incidentally, this is one of the current limitations of this model and many other models, such as GEOS-chem (Zhu et al., 2018), due to the lack of observational constraint on plume vertical profiles. We have recently promoted an AeroCom multiple-model initiative to constrain the vertical profile of plume height in a model with the MISR plume height (see more details at the Wiki website: https://wiki.met.no/aerocom/phase3-experiments).

<Referee> L297: So basically, you use the same AOD data that was used to construct one of the BB inventories to evaluate a suite of models. That just doesn't feel right and requires careful explanation why this is done and what the consequences are

<Response> We have replaced MODIS AOD with MISRv23 AOD in the Figure 5-7 as below. In general, the results with MISR AOD are consistent with those with MODIS AOD. We also have changed the text part accordingly in the revised version (but too numerous to put here).

<Referee> L305: I feel this is more useful and scientific sound; evaluate the various inventories with independent data

<Response> Please see our response above.

<Referee> L393: But isn't April outside the main fire season in EQAS? In other words, if emissions are very low then a factor two difference (for example due to the detection of small fires in GFED4) is not that noteworthy I guess

<Response> It is actually in August (not April), the peak of the fire season in EQAS, that GFED4s is a factor of two higher than GFED3.1. Sorry about the confusion. We have corrected it in the revised version. Now it reads like this:

"In particular, it is noteworthy that in EQAS, the annual OC emissions from GFED4s was lower than that of GFED3.1 by 18%, but higher by a factor of two in the month of August when peatland burning is predominant."

<Referee> L402: This is indeed a key question and I doubt we will make much progress as long as we keep using one single dataset to constrain emissions. Broadly speaking, the "gas community" (CO, NO2) has shown that the traditional inventories do reasonably well while the "aerosol community" has shown for over 10 years now that the emissions of those inventories are too low to reconstruct measured AOD. It would be very nice if someone would address why those two communities come to different conclusions.

<Response> It is a good point. We have added the statement below in the introduction part:

Andreae (2019) commented that "In contrast to gaseous compounds, which are chemically well defined, aerosols are complex and variable mixtures of organic and inorganic species and comprise particles across a wide range of sizes. This affects in particular the measurements of organic aerosol, black/elemental carbon, and size fractionated aerosol mass".

In the Section 4.3, we mentioned in the manuscript that many models like GEOS version used in this study did not consider the secondary organic carbon produced from biomass burning emissions".

<Referee> L416 lights -> light

<Response> Changed. Thanks.

<Referee> L419: GOES -> GEOS

<Response> Changed. Thanks. Printer-friendly versionDiscussion paper <Referee>: L452: This is a bit confusing, I don't think emissions peaked in April but you found elevated AOD levels due to burning

<Response> This was due to an oversight on our part. Thank you for pointing it out. We rewrote that paragraph as:

“Being mixed with, and often surpassed by other aerosol types in certain regions, however, the contribution of biomass burning aerosols to the total AOD is hardly distinguishable from those of other sources outside of the peak months, such as April (Fig. 6) in the regions of Southeast Asia (SEAS), Central Asia (CEAS), and Boreal Asia (BOAS). Such complicated situations lead to the difficulties in evaluating the BB emission datasets with the AOD observations.”

<Referee> L467: Given the very large interannual variability, especially in EQAS, this should be avoided. Please scale with active fire detections or so

<Response> We agree that the biomass burning has large interannual variability in certain regions, especially in EQAS, as we have shown in one of our recent publications on Indonesian fires (Pan et al., 2018). Thus, we overlaid the AERONET climatology and MODIS-Aqua and MODIS-Terra to complement AERONET whenever it has missing data in 2008.

<Referee> L529: Now shown -> Not shown (I guess)

<Response> This has been corrected. Thanks.

<Referee>: L624: This could be a place where this paper could make a difference. Given that the emission factors used in the various datasets are not wildly different, the variability stems from variability in dry matter fuel consumption. GFED has been tuned to match measured fuel consumption, how about FEER and QFAS? Are their levels of fuel consumption (per unit burned area that is) similar to literature-based values? I understand that the FRP approach aims to avoid burned area but these datasets are becoming better constrained and by dividing fuel consumption from FEER and QFED with burned area there could be a useful constraint. Right now we compare AOD with AOD-derived datasets and that just does not help us further I am afraid

<Response> We agree that it would be much more useful to the community to go beyond mere comparisons between the different emissions datasets to develop a con-

straint that can eventually lead to a realistic understanding of the reasons for the disagreements and how to account for them, and hopefully improve the emissions. The current paper is the initial step toward that goal, as it helps to understand the high-level relationships/disagreements between the different emissions datasets, at the global, regional, and local scales, based on simulations using the exact same global model. Detailed diagnosis of the issues with the individual dataset and finding appropriate synergistic connections between them can follow from this in a systematic manner. Using laboratory measurements of small fires, Ichoku et al. (2008) showed a relationship between the traditional emission factors (EF) based on the burned-biomass approach and the emission coefficients (Ce) based on the FRP approach. These two factors are related via the combustion factor (Fc) that relates time-integrated FRP and total burned biomass. Such relationships can potentially be applied as a useful constraint for improving emissions, but will need to be pursued in a future study that is more focused on addressing such a question.

<Referee> L731: Actually most of the emission factors are from actual fires, not from lab-based measurements.

<Response> We have added the contribution from field campaigns. The paragraph now reads as follows:

"Emission factor (EF). . . .However, the EFs can have significant uncertainties (Andreae, 2019), because each EF deals with a particular experiment or field campaign. Some EFs are derived from lab-based studies whereby samples of fuels are burned in combustion chambers (e.g., Christian et al., 2003; Freeborn et al., 2008), where the combustion characteristics can be very different from those of large-scale open biomass burning and wildfire; and some EFs are derived from field campaigns, where the measurement locations are often not close enough to the biomass burning source due to personnel safety and other logistic factors (Aurell et al., 2019)."

References: Andreae, M. O.: Emission of trace gases and aerosols from biomass burning – an updated assessment, Atmos. Chem. Phys., 19, 8523–8546, https://doi.org/10.5194/acp-19-8523-2019, 2019.

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Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-475/acp-2019-475-AC1-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-475, 2019.

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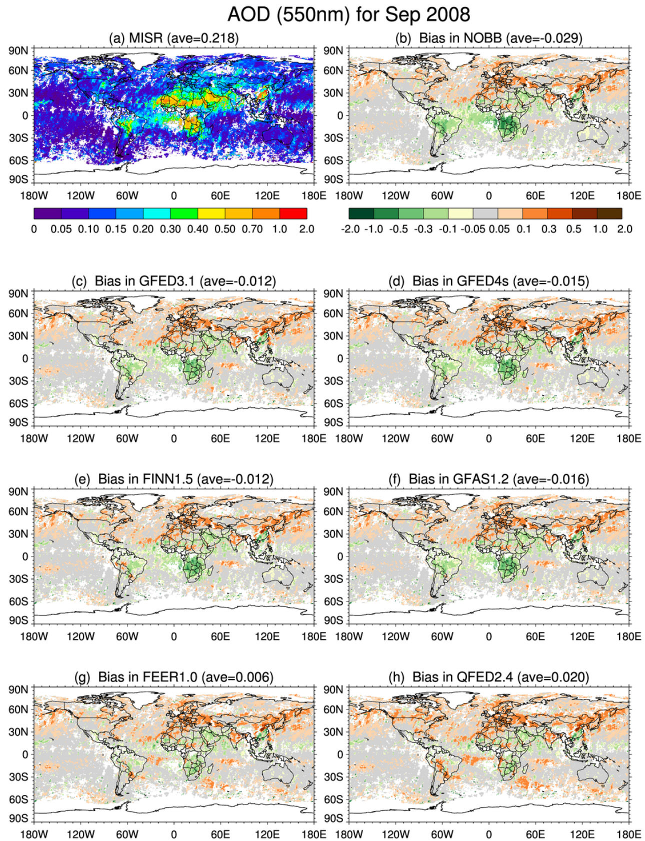

Fig. 1. Figure 5. (a) The spatial distribution of monthly mean AOD at 558nm for September 2008 from MISR with the white color representing missing value. The global averaged value (ave) is shown in the parent

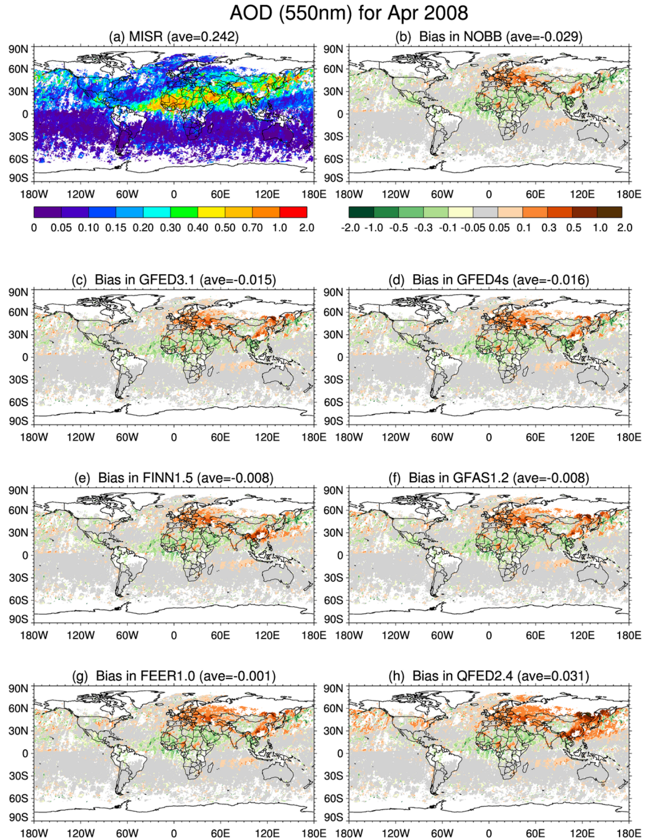

Fig. 2. Figure 6. Same as Figure 5 except for April 2008.

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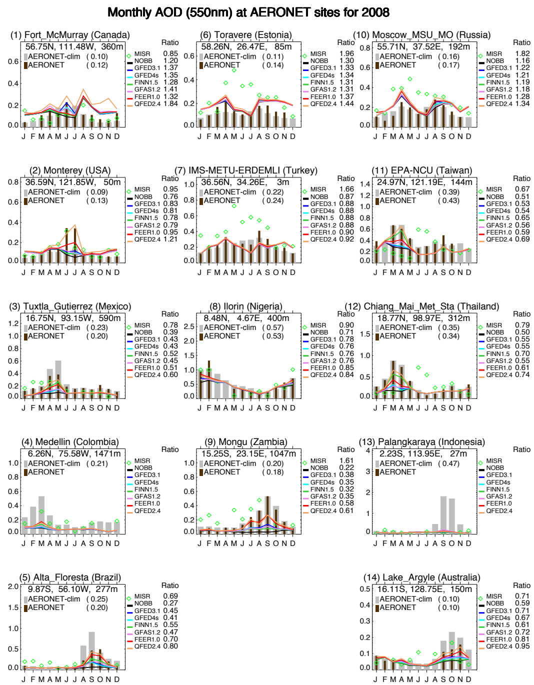

Fig. 3. Figure 7. Monthly variation of AOD (at 550nm wavelength) for 2008 over 14 AERONET sites selected from their respective regions (with its country indicated in parentheses).

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