

## **Comment on “Impact of biomass burning on pollutants surface concentrations in mega cities of the Gulf of Guinea” by Laurent Menut et al.**

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*“This review was prepared as part of graduate program course work at Wageningen University, and has been produced under supervision of Prof Wouter Peters. The review has been posted because of its good quality, and likely usefulness to the authors and editor. This review was not solicited by the journal.”*

Your manuscript investigates the relative contribution of pollutants caused by biomass burning from central and southern Africa on the surface concentrations of aerosols, carbon monoxide and ozone in urban areas in the Guinean Gulf. For this purpose, a large area is modelled using the Weather and Research Forecast model (WRF) and CHIMERE model. Four simulations were done in the months June and July 2014. The first simulation included the releasing of tracers into the atmosphere to see which regions in central Africa are important for the biomass burning influence in the Gulf of Guinea. It turned out that meteorological conditions are favourable for transporting emissions towards the Gulf cities within one week. The other three simulations investigated the atmospheric content with the CHIMERE model, without biomass burning and with biomass burning injected at two different heights into the troposphere. The simulations were validated with the help of observations and products of the MODIS AOD, AERONET, CO and CALIOP. With the last three simulations the effect of the biomass burning on the total emission concentrations could be investigated and quantified. The modelled results showed no effect of different injection heights far from the sources. Furthermore, the effect of biomass burning appeared after a few days and the maximum contribution of the emissions was for CO 150  $\mu\text{g}/\text{m}^3$ , for O<sub>3</sub> 20  $\mu\text{g}/\text{m}^3$  and for PM<sub>10</sub> 5  $\mu\text{g}/\text{m}^3$ .

Your manuscript is important because the particle concentrations are rapidly growing in the past couple of years in southern West Africa and at the moment still barely monitored. Whereas, Mari et al. (2008) showed that even biomass burning plumes from the southern hemisphere could reach the Guinean Gulf. It is therefore crucial to be able to quantify the contribution of several processes to the total particle concentration, including biomass burning. Your research is also new and innovative in a sense that the investigated area is to this extend never modelled before. The used methodology fits nicely in the range of subjects of the Atmospheric Chemistry and Physics journal, because a big part of the manuscript is about atmospheric particles and how to model these and one of the main subject areas of the journal is atmospheric modelling.

In general the manuscript is well written, clear and has a good structure. It is directly clear from the goal what the research question is of the manuscript. The calculations schemes and models (such as the Alfaro and Gomes scheme) are thoroughly investigated and improved before you used them in the manuscript in order to answer the research question. Furthermore, The biggest and most important sections end with a small summary. This makes your manuscript very clear and easy to read. If you would not have done this, the paper would be quite long and complicated, but thanks to these summaries, it is easier to grasp immediately the general idea of the manuscript when you read it for the first time. On top of that, you validated the model with all possible satellite observations

and ground measurements that were available. Because almost all the results showed that the model performed well, the model seems very trustworthy. It is therefore in my opinion no problem that the model is not validated in predicting the composition of the pollutants. However, there are some sections and topics, with regard to the research question answer, definition clarity, the chosen research period, errors and some other more minor things, that should be revised in order to make the manuscript truly publishable.

## Major concerns

My first concern is about the research question. As already said, from the goal it is directly clear what the research question is. In short: investigating the relative contribution of certain pollutants on the surface concentrations in urbanized areas. In your conclusions section you go nicely back to his goal and try to answer this question, but in my opinion you do not completely answer the research question. In short you answer this question by summing up the absolute maximum concentration additions of biomass burning to surface concentrations. This is very nice and interesting but in your goal you promised to give the relative contribution and not the absolute one. This is not only missing in your conclusions section, but you also do not mention it in the results section.

The first question that should be answered is relative to what you want to compare the biomass burning contribution? Relative to the total local air pollution concentrations per substance in the air? Or the contribution of biomass burning per substance to the total concentration of all the substances together emitted by biomass burning? For example Piketh et al. (1999) determines the relative contribution of biomass burning to the total inorganic aerosol concentration. Because you primarily investigate the contribution of the inorganic substances  $O_3$  and CO, you could also use this approach. However, because you focus also on the  $PM_{10}$ , it is in your case more interesting to give the relative contribution of a substance ( $O_3$ ,  $PM_{10}$  or CO) emitted by biomass burning to the total substance concentration ( $O_3$ ,  $PM_{10}$  or CO) in a certain location.

It is not a lot of work to get the outcome of the relative contribution in your manuscript. Figure 14 shows that you have calculated emissions per substance for FIRES and NoFIRE. If you want the maximum relative contribution, this means that you can easily perform the following calculation for the hour where (FIRES – NoFIRE) is the highest:  $(FIRES - NoFIRE) / FIRES * 100\%$ , according to Ott et al. (2013). Thus, please state in your introduction relative to what you calculate the biomass burning contribution, calculate in section 7.1 apart from the absolute contributions also the relative contributions with the formula given above and show this result also in the conclusions section. This would make your manuscript better, because you answer the research question properly and a relative contribution provides more information than just the maximum contribution.

The second thing that concerns me is the chosen model period. From the manuscript it becomes clear that the simulations are performed for the period May to July 2014. Reasons to choose this period are that the simulations were a preparation for the fieldwork in June/July 2016 and the onset of the West African Monsoon (WAM) in that period. However, I do not understand why you did not choose for June-August instead of May-July. Several sources (Mari et al. (2008), Williams et al. (2010)) say that the highest concentrations measured due to biomass burning are during August. On top of that, according to Williams et al. (2010) the WAM is from June to August. Furthermore, for the

southern hemisphere, where the tracers are released, the biomass burning season is from June to August (Mari et al (2008)).

For the fieldwork and the tracers you could have modelled August, because if you chose June to August, the model results could still be used for the fieldwork June/July. In the manuscript, you start with releasing the tracer only on 15 June and most of the figures you show are the concentrations at the end of the period: the end of July, because you claim that the concentrations are then the highest. Why are you so sure that concentrations are then the highest?

It would really strengthen the manuscript if you could model the atmospheric composition for August as well in chapter 7, because especially the graphs in figure 14 shows still a positive trend at the end of July. This means that the biomass burning contribution to several substances may be higher in August than at the end of your modelled period. Thus, your estimated maximum contribution values per substances are also too low. These estimated values are very important, because this is the answer to your research question and this answer would not make sense, if the maximum estimated values are in reality much higher a month later. In my opinion it is not necessary to validate the model again for August before you use the model to calculate the atmospheric composition for that month, because figure 6, 10, 11 and 12 show no evidence to assume that the model will perform worse after July 31.

Another concern is that the introduction starts with *“The concentrations of gases and particles are rapidly growing in southern West Africa (SWA), driven by the constant increase of anthropogenic atmospheric emissions”*. Whereas, you are going to investigate biomass burning which is a partly naturally caused phenomenon. There are two things in these statements that are not clear for me. First of all, why do you start your introduction with a problem that you do not tackle directly. If you start your introduction like this, I would expect that you try to quantify anthropogenic emission sources for example. This could have been the case if you focussed only on human induced fires, such as agricultural fires and deforestation, which occur in Central Africa (Buccini et al. (2002) , van der Werf et al. (2010)). However with the sentence *“In addition to this anthropogenic regional pollution, the region is impacted by other important sources especially in the summer, with high emissions of mineral dust from the Sahara and Sahel to the north and vegetation fires from Central and southern Africa.”*, you implicate that you focus on the natural occurrence of biomass burning. Please make in the introduction clear whether you focus only on anthropogenic induced biomass burning or on naturally induced biomass burning and if you focus only on the latter one, you should make clear in the introduction why we have to investigate biomass burning now, while anthropogenic emissions are growing.

I am also a bit concerned about your (lack of) error propagation in the manuscript. When choosing the values for several model parameters, several calculation schemes and models are used. There are also errors in these, but it is not possible to compare these with the real values, because they are often unknown. It is still very important to be transparent about uncertainties in this stage. For example in the APIFLAME model to calculate the emission fluxes of biomass burning. There are uncertainties in using this model, that you do not mention, such as the fact that Turquety et al. (2014) states that a lot of parameters, such as information about the biomass density are primarily developed for Europe and are more uncertain for the rest of the world (including the Gulf of Guinea). Please be transparent about these kind of uncertainties.

In the next step when you have your model with its parameters, you test the model thoroughly for several aspects and most of the time you conclude that the model is sufficient. However, the correlations between model and observations as shown in table 2 and 3 are on average quite low and often not even 0.3. In my opinion that is too low for a correlation. You tackle this problem for both tables in your text and explain what the reason is for this low correlation (wrong location measurement stations or large temporal variability for example) and why the model is still good enough to use. Still there are errors in the model and in this stage you are able to quantify them. You could do this quite simple and straightforward by calculating an average error percentage per location: divide the showed bias value by the model value. Average these for all the available model-observation comparisons. In the end you have a bias percentage per location for the model.

With the described simple method to determine the bias percentage per location you can include uncertainty bands in your final answers of the conclusions section, even if you did not compare these values with the observations. Now, you just give the averaged values as if it is a fact, but if I regard the previous uncertainty in the modelling which the text clearly explained, there must be uncertainty in these atmospheric composition answers as well. Therefore, you should show the values in the form of  $\text{CO} \approx 150 \pm \dots \mu\text{g}/\text{m}^3$ .

## Minor concerns

**General structure remark:** Until figure 7, the figures are not closely placed to the text where they are explained. Please put the figures close to their text part.

**Page 1, line 16:** There is no reference after this sentence or small section. There are several sources which say that southern West Africa has a big air pollution problem (Knippertz et al. (2015), Lioussé et al. (2014) and De Longueville et al. (2010)), but I could not find a source that states that especially the mentioned areas have the biggest problems. The second part of the sentence about the atmospheric boundary layer is from Real et al. (2010), which is mentioned some sentences later. Can you provide the correct references after this sentence?

**Page 1, Abstract/Introduction general:** Page 15 to 25 of the manuscript, which is almost 1/3 of the total content, is about comparing the model runs with observations. Thus, the model is validated on several levels. It is very good that the model is thoroughly tested, before using it without observations, but from the introduction and abstract it is not clear that such a big part of the manuscript is about validating. There is only one sentence that states shortly that the meteorological model ability and chemical species concentration prediction ability will be compared with observations. Please state clearly in your introduction and abstract that the model is thoroughly validated, with the help of the observations of space-born platforms and ground based stations, such as IASI, AERONET, CALIOP... This fits nicely with the next section which gives a more detailed description of the used observations.

**Page 1, line 18:** You give Real et al. (2010) as a reference. In my opinion Real et al. (2010) only shows that biomass burning pollutants can reach the Gulf area from Central Africa, but says nothing about Sahara sand or how important this source is. Thus, by the given reference I am certainly not convinced. Mari et al. (2008), to which you refer in line 2, states clearly that the region in general is impacted by biomass burning. You should refer to Mari et al. (2008) instead of Real et al. (2010). Still, none of these sources says something about mineral dust. De Longueville et al. (2010) states that

especially in West Africa mineral dust is a very important factor affecting the local air quality, thus you should also refer to De Longueville et al. (2010) instead of Real et al. (2010).

**Page 2, line 6:** If you are as a reader not familiar with the DACCIWA project it is entirely unclear what the link is between the project and this paper. Is the manuscript financially supported by this project? From which organisation is this project? This is now only clear if you read the Acknowledgements in the end.

**Page 4, figure 1:** Figure should be a little bit bigger to make it easier to read. The disadvantage of having the figure already on page 4 is that it gives also an overview of the CALIOP and IASI locations, whereas at that point you do not know what the purpose is of CALIOP and IASI. A sentence about this in the text of the manuscript would be nice.

**Page 6, line 22:** You use updated data about the erodibility provided by Beegum et al (2016). However, you say nothing about using updated data for the roughness length, whereas this information can also be provided by Beegum et al. (2016). It is therefore not clear for me whether you used the updated information about roughness length in the manuscript. Can you make this clear?

**Page 18, table 2:** Several aspects of this table are not completely clear. Can you explain why there is for almost all the locations so little data available and how this does not influence the validation? Furthermore, if you scan the table for the first time it is definitely not clear that 'Obs' and 'Model' are just the real measured or simulated values, because there is nothing about that in the caption and you give no units. Please give units for these values. On top of that, it is not directly clear that  $R_t$  is the correlation where you are talking about in the text, why do you call it temporal correlation in the tables? Could you explain this in the text? These last two points apply also to table 3.

**Page 25, section 6.6:** This section concludes that the model performed quite good in all the tests. However some observation-model comparisons showed that there were uncertainties and prediction errors. Can you summarize these model uncertainties as well?

## Minor issues

**Page 1, line 14:** "linked to" instead of "linked with".

**Page 2, line 27:** "Ground-based" instead of "groud-based".

**Page 6, figure 2:** The purpose of this figure is completely unclear for me. It shows the modelled anthropogenic  $\text{NO}_2$  fluxes for a week day. It is interesting to see output of the CHIMERE model, but the modelled flux is not investigated in the paper, so why would you show it? In my opinion, this figure does not add value to the manuscript and can be deleted.

**Page 7, line 11:** "Of the daily ..." Of the daily what? I think the word "fluxes" is missing.

**Page 8, line 12:** "A homogeneous way" instead of "an homogeneous way".

**Page 9, line 18:** "Consist of" instead of "consist in".

**Page 10, section 4:** This section comes a little bit out of the blue here. In essence you compare the model with the observations. Therefore, I think that this section fits better as a new section 6.1 in section 6. Because in section 6 you compare the model with observations and in the small introduction of section 6 you come back to the results of section 4: it is better to provide this results directly in section 6.

**Page 12, figure 6:** Can be deleted. You do not refer in the text to figure 6 and it is actually unnecessary, because figure 5 shows the same idea, but then on a spatial scale.

**Page 17, line 10:** “Correctly enough” instead of “enough correctly”.

**Page 20, figure 11:** This figure shows the comparison between model and observations at different locations. Per location there is a graph for east and west. In my opinion there is barely difference between east and west for every location. It would therefore be better the say in the text that there is no difference between east and west and just show one graph per location (south, north central).

**Page 21, line 13:** “Where the studied areas are located” instead of “where are located the studied areas”.

**Page 21, line 14:** “Have” instead of “has”.

**Page 22, table 3:** Caption claims that RMSE, the root mean square error, can also be seen in the table. This is not the case. You should delete this from the caption or include indeed the RMSE.

**Page 24, line 27:** “But” instead of “bit”.

**Page 28, line 12:** “Period in” instead of “period of”.

## References

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