Review of "Impact of biomass burning on pollutants surface concentrations in megacities of the Gulf of Guinea" by Menut et al. (2017)

Reviewed by Anne Swank

---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.---

The study of Menut et al. quantifies the relative contribution of biomass burning emissions in Central and South Africa on the surface concentrations of CO, O_3 and PM_{10} in urbanized areas in Southern West Africa. This is done with CHIMERE model simulations of biomass burnings and comparison with satellite and ground measurements data. Several (tracer) model simulations are performed and show that the biomass burning emissions do indeed have an impact on the surface concentrations in urban areas in Southern West Africa. This study contributes significantly to scientific research since it includes the air pollutants in the previously studied air masses transport in Africa, which is an important attribute since (anthropogenic) air pollutants are increasing and have an impact on human's health. The simulation of the model are well tough of and are compared properly to available data. Hence this study fits to the reader's interest of Atmospheric Chemistry and Physics. However, the answer on the main research question about the relative contribution of biomass burning emissions cannot be found in this paper, which can be attributed to the absence of background information and clear explanation between the connection of observed and simulated air pollution values. These quantified relations are sometimes missing and some figures could be shown in another way. Therefore I recommend some major changes in this paper, at least to answer the main research question and on some other aspects, prior to publishing this paper in ACP.

Major arguments:

The paper does not describe clearly how the biomass burning emissions are contributing to the total air pollution in the simulations and observed concentrations, where the research question is how the emissions contribute relatively. The results of the CHIMERE model simulations as illustrated in Figure 14, clearly with the simulation differences, show the maximum increase in surface concentrations of CO, O_3 and PM_{10} of about 150 $\mu g\ m^{-3}$, 10 to 20 $\mu g\ m^{-3}$ and 5 $\mu g\ m^{-3}$ respectively. Concluded from these values is that the exceedance of pollution alerts will not by influenced hereby, but the impact on human exposure is not negligible. In the study of Adon et al. (2016) the concentration of O_3 in West African urban environments ranges from 5.5 to 7.7 ppb (equal to $\mu g/m^3$), which indicates that an increase of 5 $\mu g/m^3$ would have an influence. From the study of Antonel & Chowdhury (2014) the PM_{10} concentration are in range from 60 to 140 $\mu g/m^3$. I believe that the increase in PM_{10} due to biomass burning will therefore not have a large influence on the pollution alert. But, from my view it is necessary to include these reference (background) values of air pollutants in the urban areas in SWA, if necessary include observations form previous WAM years, and concentrations when the air pollution alert will be exceeded. The same accounts for the air pollution concentrations which have an impact on human exposure, when certain air pollutants do have a large impact more attention could be paid to these in the study, serving the purpose of the DACCIWA project. With the reference values included the relative contribution of biomass burning emissions on air pollution and its impact can be determined.

In the explanation for the TRC experiment in paragraph 3.5 is stated that the tracer emission flux is injected constantly without including the diurnal cycle, which produces the results as shown in Figure 7 and 8. However, in the study of Parker et al. (2005) is stated that the diurnal cycle has implications on the mixing of trace gases and aerosols between the surface layer and free troposphere. The vertical mixing occurs to be most efficient at night, which indicates that for the tracer experiment the diurnal cycle should be taken into account. This is confirmed by the study of Gilge et al. (2010), which indicates

that an increase in the vertical mixing within the free troposphere could influence the air pollution levels in the lower free troposphere, with implications to the boundary layer. As consequence different vertical mixing profiles due to the diurnal cycle will probably have an influence on the tracer emission fluxes and transport. However, in section 3.4 of this study is indicated that the two different vertical mixing profiles provide the same results for the transport of the tracer and that only the profile of PR2 will be discussed. Wouldn't the transport be affected by different vertical mixing profiles when the tracer emission experiences a diurnal cycle? It might be good to discuss this in the article with results of the tracer experiment including a diurnal cycle for the two different vertical profiles.

The simulations in this study to quantify the biomass burning emissions are performed with the CHIMERE model. In the article is stated that comparison between satellite data, ground observations and simulations of the CHIMERE model show that the output model results are robust. This is illustrated for instance as time series in Figure 11 for the CO concentrations. What I miss in this article is why the air pollution concentrations are simulated with the use of the CHIMERE instead of another air chemistry model. In the study which Solazzo et al. performed in 2017, but with the CHIMERE model version of 2013, is stated that the CHIMERE model as its performance is studied in Europe, not always simulates the air pollutant concentration correctly. When the ozone lateral boundaries are changed, a shift is visible in the ozone diurnal cycle of the CHIMERE model with significant impact, which could indicate a flaw in the PBL dynamics. A positive bias in the ozone concentration simulated by CHIMERE is also concluded from the study of van Loon et al. (2017). Besides, the error of NO_2 impacts influence the ozone error significantly. These problems do not seem to be solved as I read from the article of Mailler et al. (2017) which describes the 2017 updates of CHIMERE. Due to the flaws in the model on ozone specifically I am not convinced that CHIMERE is the model to use in this study at this moment. An explanation about the simulated ozone concentrations or the relation between the ozone simulations and observations would help me to understand the choice to use CHIMERE. Besides, including some (necessary) correlations between observations and CHIMERE simulations for at least O_3, aerosol subtype and CO would convince the reader of the robustness of the model, similar as Table 2 and 3 for AOD and PM_{10}, next to only the quantitative differences in Figure 11 and 13 for instance.

Minor arguments:
The conclusion from this paper is that the increase in air pollutants is mainly related to PMM and POM, indicating biomass burning. This is stated in part 7.2 of results, however I cannot find (neither in literature) whether this statement can be related to a real life scenario. Quantitative argumentation and references should be included on this matter.

In chapter 3 is mentioned that CHIMERE reads (WRF and) the surface emissions to simulate the chemical concentration, it is not described how these are obtained or can I assume that these are obtained by MODIS data? A brief explanation would be adequate.

In the article the output of the simulation of the CHIMERE model in the urban areas are quantified. However, it is not stated clearly how the values are obtained. Are these point measurements or averages over the whole urban area?

It is a bit unclear to me, whether all fires for the PR2 simulations are estimated on the same Hp or are estimated per individual fire (as described in paragraph 3.4 page 9)? I believe that the magnitude of fires differs and results in different Hp which are influencing the transport of air particles. The explanation could be improved.

Minor issues:

p1, abstract: in the abstract the main goal of the study is framed differently than throughout the paper

p3, Table 1: the caption of Table 1 and elsewhere throughout the manuscript is situated below the table, where it should be placed above

p4, Figure 1: adding a legend to the figure would help the reader to understand it in one glimpse

p9, line 8: probably you mean the 'the transport of fire emissions' instead of 'transport of fires"

p9, line 27: "than" should be replaced by 'as'

p10, line 11: 'complementary information is provided' should be stated here

p10: it would be logical to place Figure 1 below chapter 3 since it elaborates on the different areas

p16, Figure 9: a clearly visible title should be provided for each output figure including the unit of the values and the studied variable. Besides, wouldn't it be clearer to the reader if the simulation deviation between simulated and observed values is visualized?

p19, last line: "And" can be left out

p25, line 14: after "a minor" 'role' should be stated

p25, chapter 7: in the first paragraph it is forgotten to include "(iii)"

p27, line 6: in the introduction of the conclusion PM_{10} is not mentioned

p28, line 18: choose "by" or "via" and include 'the'

Literature list:

Adon, M., Yoboué, V., Galy-Lacaux, C., Liousse, C., Diop, B., Gardrat, E., ... & Jarnot, C. (2016). Measurements of NO 2, SO 2, NH 3, HNO 3 and O 3 in West African urban environments. *Atmospheric Environment*, *135*, 31-40.

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Gilge, S., Plass-Dülmer, C., Fricke, W., Kaiser, A., Ries, L., Buchmann, B., & Steinbacher, M. (2010). Ozone, carbon monoxide and nitrogen oxides time series at four alpine GAW mountain stations in central Europe. *Atmospheric Chemistry and Physics*, *10*(24), 12295-12316.

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Parker, D. J., Burton, R. R., Diongue-Niang, A., Ellis, R. J., Felton, M., Taylor, C. M., ... & Tompkins, A. M. (2005). The diurnal cycle of the West African monsoon circulation. *Quarterly Journal of the Royal Meteorological Society*, *131*(611), 2839-2860.

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