

Interactive comment on “Characterisation of Central-African aerosol and trace-gas emissions based on MAX-DOAS measurements and model simulations over Bujumbura, Burundi” by Clio Gielen et al.

Anonymous Referee #2

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The manuscript “Characterization of Central-African aerosol and trace-gas emissions based on MAX-DOAS measurements and model simulations over Bujumbura, Burundi.” by Gielen et al. reports their 2-year MAX-DOAS observations of aerosol, NO₂, and HCHO at a site in Bujumbura, Burundi. The results are compared with co-located AERONET sun-photometer measurements as well as with satellite observations and 3D model simulations. Based on analyzing the entire data set in terms of temporal and spatial variations, the authors conclude that biomass-burning in surrounding regions of Bujumbura contributes significantly to the variation of aerosol and HCHO while NO₂ mainly originates from anthropogenic emissions in Bujumbura city center. Although

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large, various, and valuable data are presented, I feel that the manuscript should be more focusing on analyzing characteristics of aerosol and trace-gas emissions than on explaining observation differences by different instruments. I would recommend the publication of the manuscript only if my following comments are well addressed.

General comments

From the title of the manuscript, I was expecting to see some detailed description on sources, possibly rates, and their variations (temporal, spatial) of aerosol and trace-gas emissions around the measurement sites. Especially, how is the yearly change of the emission, what could be the reason for that, and if possible what would be the atmospheric impacts? The authors spent large efforts on describing figures of measurement results but without much in-depth analysis. For example, Line 11-29 in Page 8, what is the reason for the difference in profiles measured in different seasons, why "the trace gases the H_x values remain nearly constant throughout the year, whereas for the aerosols, higher profile altitudes are seen in MAM and SON"? Moreover, I do not get the idea that the comparison between sun-photometer/satellite and MAX-DOAS observations should be included in the manuscript, because they seems not quite related with characterizing emissions. In stead, I would suggest the authors to put more emphasis on analyzing the model and observation discrepancies, since ground observations are supposed to validate and improve model performance.

Specific comments

Line 9, Page 1: "biogenic emissions" → I did not find specific description on identifying biogenic emissions of aerosol and trace-gases in the manuscript.

Line 14, Page 1: "In contrast ... species (typically 1-2 hours)." → "In contrast, due to its short lifetime, NO₂ is seen to depend mainly on local emissions close to the city."

Line 15–28, Page 2: Readers of this paper are most likely familiar with atmospheric chemistry and know in general sources and sinks of aerosol, NO₂, and HCHO. I think what they might not know is the importance of characterizing emissions of these species in central Africa. What is the impact of African emission on the global scale?

Line 27, Page 3: Although the meteo. station was installed only from 2015, I think it is still necessary to use some historical data from nearby regions to illustrate that the meteorological condition is stable from year to year. Because changes in temperate will have strong influence on biogenic emissions and might lead to different HCHO and aerosol concentrations.

Line 9–10, Page 4: I suggest more description about the MAX-DOAS instrument, e.g., wavelength range, resolution, field of view, time for one duty scan, etc.. Or a reference should be mentioned.

Line 17–19, Page 4: What are the elevation angles?

Line 16–17, Page 5: Change to "... HCHO and NO₂ at 342 nm and 460 nm, respectively."

Line 25, Page 5: Can the temp. and press. profile of USSA represent the central African condition?

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Section 4.1, Page 9–10: What about the year-to-year change of the observed AODs and VCDs? What could be the reason for that?

Line 6–7, Page 10: I estimate the life time of HCHO and NO₂ under overhead sun in summer in the central Africa is around 1.3 h and 1.7 h (@ OH = $1.5 \times 10^7 \text{ cm}^{-3}$, $J_{\text{HCHO}} = 8 \times 10^{-5} \text{ s}^{-1}$), respectively. Therefore, if the local anthropogenic emission can influence NO₂, it also works for HCHO unless this source does not emit HCHO.

Line 14–20, Page 10: Why the offset of the AOD regression between AERONET and MAX-DOAS only exists for 447 nm but not for 360 nm?

Line 21–22, Page 10: Delete "is obtained".

Section 4.1.2, Page 11: I noticed the aerosol profile below 2 km looks quite different between 360 nm and 470 nm in JJA months, i.e., there is a sharp decrease followed by an increase in 477 nm (Fig. 12). What is the reason for this?

Line 26, Page 11: I could not follow the conclusion that "This makes it difficult to investigate the variability in profile height". In principle, both photochemistry and boundary layer development influence the vertical distribution of NO₂ and HCHO. I think it is worthwhile to distinguish between these two factors at here. Probably with the help of aerosol profiles which to some extent can reflect the PBL change.

Line 11–12, Page 12: What could be the reason for the difference of diurnal variation between NO₂ and HCHO?

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Line 13–20, Page 12: The observation at different viewing directions were performed in different years. Would this cause difference in R ? And how to consider this possible year-to-year emission difference in identifying sources of NO_2 and HCHO? The same questions exist for Line 13–20 in this page.

Line 7, Page 13: I suggest to use the same geo-coordinate scale in Fig. 17, so that it is clearer for the statement "For the DJF period, the back-trajectories originate from an area relatively close to the station ...".

Section 4.2.3: If it is possible, I suggest the authors to reformat this section with more emphasis on the diagnostics of the model by MAX-DOAS and satellite observations. As illustrated in Figure 18 the model underestimates all measured species significantly, something must be wrong! How could the model performance been benefit from the measurements in this study?

Line 17, Page 13: I don't understand that why the authors did the model calculation for 2013 but not for 2014 or 2015 when they got most measurement results. This "computational restriction" even confused me when they already showed the model simulation for 2013–2015 in Fig. 7.

Line 19, Page 13: "... underestimate the columns ..." → Does the variability in Line 17 means columns for 2013?

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