

Interactive comment on “Seasonal and diurnal variability in air pollutants and short-lived climate forcers measured at the Rwanda Climate Observatory” by H. Langley DeWitt et al.

Anonymous Referee #2

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This manuscript presents over a year worth of measurements of air pollutants from the Rwanda Climate Observatory. This is an important dataset in an area where there are few long-term measurements. Thus, this manuscript does add important new information to our understanding of atmospheric composition in Africa and is appropriate for publishing in ACP. I would recommend major revisions before it is accepted.

In general, I believe that the manuscript focuses too much on biomass burning impacts on the site and does so too early. The site and its data are important contributions to the scientific literature, however, this gets lost in the current structure of the manuscript. I think because the data and site weren't first fully explained and characterized, the anal-

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ysis of the data that follows is confusing to me in parts. I believe to improve this, the manuscript does need to be reorganized and edited, and that is why I am recommending major revisions.

I would recommend that the paper is re-focused to firstly be on presenting the site and its measurements. As written now, the data are presented, but then quickly the focus moves to other sites and back trajectories. I would recommend that the authors first present these measurements fully and fully characterize the site. To assist with the former, I would recommend adding a table with the values of pollutants measured (e.g. annual average, seasonal averages, etc.). A case study of polluted or non-polluted events (or both) could also be helpful to understand the drivers of pollution at the site as well. In order to help characterize the site, I would recommend showing all the data including the met data (including local wind direction and speed) in addition to the air mass history through hysplit and the other GHG measurements for completeness. I would recommend adding graphs of temperature, rain and solar radiation to Figure 3. Solar radiation could be important to explaining ozone, and so would be helpful to present.

One thing that is not clear to me is if this site is generally within the boundary layer or not. This is key to the explanation of the diurnal cycle (e.g. ozone analysis in section 3.2.1), however it is not clear to me what/where the site is sampling. This is also important in understanding if the site is impacted by biomass burning. For example, in the SAFARI campaigns in southern Africa (and other campaigns) it was found that biomass burning pollution is found aloft. This was confirmed again in Hersey et al. (2015), when the seasonal profile of ground-based PM is not the same as column PM, as the former is impacted by ground-based sources and the latter by biomass burning (which is aloft). Thus, it is a question of at what altitudes the biomass burning emissions are transported from these distance sources to this site, and would that align with where this station is measuring. This disconnect that occurs between pollution concentrations aloft and ground-based concentrations during biomass burning period,

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and the potential impact on the measurements of this site, is not clear to me in the current biomass burning discussion in the manuscript. For example, on line 272, this peak in PM_{2.5} is reported in Hersey et al. (2015) is during winter and impacted by ground-level sources, and is not during biomass burning period.

Line-by-line recommendations

I would recommend adding all the methods applied to the methods section. This includes details on Hysplit, MODIS, calculation of AAE, etc. These are currently in results as the ideas are introduced, however, I would recommend they should be in the methods section instead.

Starting line 251. Data from other sites are discussed in more detail before the data from the main site. I found this to be very confusing. I can see that there are few measurements in the area, so these could be helpful for comparison. However, I would recommend that they are then moved after the full presentation and analysis of the RCO data and are used to provide context. Information on the sites should be added to the methods as well. In addition, the back trajectories are discussed in 257 and not shown. It is suggested that transport occurs from southern Africa and Madagascar to Ethiopia – have others seen this?

Line 303 and Figure 5, this analysis is very interesting. For Figure 5, is the picture any different if you plot FRP only of the direction of the back trajectories? The point that main air flow changes during the seasons and, unfortunately for AQ, follows the biomass burning source region is very interesting. This can be seen in maps, but are the fires outside of the back trajectory direction (e.g. the Western African fires in MAM) artificially impacting the FRP Figure 5b? Also, I would recommend adding O₃ and CO to Figure 5b to see their trends as well. Line 303 states that MODIS is used qualitatively and not quantitatively, however FRP is quantified through the MODIS fire count data, so this seems to be contradicting the statement in 303.

Line 316, what resolution were the geographical areas re-gridded to? I assume to

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match the input met resolution, but would state it.

Line 335, on the size of the maps in Figure 7, it is hard to see “local sources”.

In the discussion of ozone, would meteorology play a role in the seasonal differences of ozone? For example, does solar radiation change dramatically? Does the boundary layer change?

Line 440, which profiles are flatter in the figure? As they are on the same scale, it is harder to see which has a relatively flatter shape.

Line 552, for the aethalometer model does it take aging into account with the apportionment? If these are biomass burning aerosols that have been transported very far, they would be aged and would look different than local BC from burning.

Figure 2, I would recommend adding shaded bars to the figure to denote rainy and dry season.

Figure 2, The light green line is easily seen online, but not in the printed version. Perhaps white or yellow may stand out better? Also, what are the dots in the CO measurements? Gaps or zeros?

Figure 6, I do like the comparison to other sites, however I would find additional information on the sites helpful. Are these just one site each or an average of sites. If it is the former, then I would recommend adding the name. If it is the latter, then I would recommend adding how many sites per country were used. Are they all the sites from those countries in the EPA report? Also, for the top graph, does this have the same x-axis? Where then were the Rwandan urban measurements taken?

Figure 7, looking at the maps, many of the concentration-weighted trajectories appear to me to be in the ocean (e.g. JJA (ozone esp. shows this), SON), though that is not the conclusion in the text. It is not clear to me why the highest concentrations would be over the ocean in this analysis. Also, in southern Africa, burning moves south as the season goes on (as shown in Figure 5). So the trajectories for JJA (in Figure 7) if they

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are coming from quite far south, then moving over the ocean to come back to land and that is where they have the highest concentrations, don't seem like they would cross the main burning areas to me.

typos

Line 82, I believe "later" should be "larger"

Line 427, should read "...at RCO is different. . ."

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