The authors would like to thank the two reviewers for their time and effort. We have considered each point carefully and address both reviewers below. Their contributions have hopefully strengthened this paper, and we have made major revisions as advised. Author comments are in blue.

Anonymous Referee #1 Received and published: 2 March 2018 This manuscript reports on the new Rwanda Climate Observatory, an atmospheric trace gas station as part of the established AGAGE network. The station was funded by a collaboration between MIT and the government of Rwanda. The data presented in this report represent the first year and a half of the operation of the atmospheric station and focuses on BC, CO and O3. Season and diurnal plots along with air mass back trajectories are presented. This data set is valuable for the air quality community, however the data in this study is simply reported and lacks a synthesized approach to validate publication in ACP. In addition, there are erroneous claims on ozone production. At the moment, this manuscript does not represent a significant advancement in the science of air quality. I would have to recommend rejection, although I would be C1 ACPD Interactive comment Printer-friendly version Discussion paper happy to review a new submission. I have suggestions to help improve the usefulness of this publication and I outline below my major comments and identify minor issues. Reviewer general comments: The goal of the publication is unclear. What are the others hoping to achieve with this study? The title suggests the discussion of pollutants AND short-lived climate forcers, but climate implication of BC, CO or O3 are not addressed in the paper. BC, CO, and O3 are short-lived climate forcers (see IPCC report). We have added text to discuss this in the intro and the conclusion. The introduction suggests the resolution of the air quality problem in Africa, but the study focuses solely on Rwanda. The introduction has been significantly shortened; we did not mean to suggest a resolution for the entire continent. The BC time series suggest local episodic experiences, but no case studies of highly polluted days are presented. A case study has been added. The ozone diurnal at a remote site is drastically different than at urban sites, but the mechanisms for these differences are not explained. This was explained in the text and we discuss in more detail now (see specific answers to this point below). In addition, it is difficult for a mountain site to inform on the air quality in Kigali and even more so on mitigation efforts within Rwanda. The authors point out that, while a mountain site, Rwanda is highly populated with low urbanization and the mountain site was within an hour walk from a major town and near settlements. These settlements are where the majority of Rwandans live (12 million people, 1 million in Kigali) and thus the air pollution in these areas is relevant and likely somewhat representative. The authors conclude that local reduction in emissions would improve air quality in Rwanda, yet their measurements suggests that the majority of high BC and CO concentrations observed at RCO are regionally impacted. Therefore, the mitigation strategies proposed by the authors wouldn't be so effective in my opinion. Throughout the manuscript, it was acknowledged that biomass burning and regional impacts were greater than local emissions. However, Rwanda can currently only control its own emissions, and enough emissions are Rwanda's that this was suggested with many caveats. We have now added text to discuss regional fires and possible

recommendations for mitigation while softening the local regulations language. Can recommendations for air quality improvement on a regional scale be made based on the presented data? Can the authors show a high pollution period with high frequency data to further support the importance and relevance of high-frequency measurements? High frequency data was used to exclude very local sources, look at diurnal profiles, showcase local spikes in pollution. A case study with the presented data would be valuable. From what I can see from Figure 9 - an interestingly high BC episode in Aug-Sept 2016 would be worth investigating. This period unfortunately does not have ozone measurements. We have chosen a different period where all instruments were working for a case study. Why don't the authors show CH4 and N2O data? Will it be part of a follow-up publication? Yes, this data will be presented in a future publication as it was a graduate student's thesis to model this data. The authors felt it was a natural split to discuss long-lived GHG species (CH4, N2O, CO2) in one paper and air pollutants in a second. Correlation plots are missing to investigate co-transported pollutants at RCO. R2 values were presented in the paper, which were derived from correlation plots. The authors chose not to show correlation plots in an effort to reduce the number of plots. When and how often is RCO within the boundary layer compared to in the free troposphere? This is now added in the methods section, Mugogo description. . An interesting study could involve measuring pollutants in Kigali and correlating them to air mass age once they reach RCO. We did not measure in Kigali and have no measurements there during the presented time period (none existed). This would be a completely separate project beyond the scope of this paper.

Similar case study work has been done by (Gao et al., 2017; Zhang et al., 2015) of which, Zhang et al 2015 the authors already cite (line 433). Higher ozone precursors do not necessarily lead to higher ozone. (erroneous conclusion lines 615-624). Ozone production is not linear. Please familiarize yourselves with ozone chemistry. (examples of review references: Baier et al., 2015; Geddes et al., 2009; Monks et al., 2015)

The authors are familiar with ozone chemistry. It is not linear, no, but more precursors do have an effect on ozone production. The authors did not suggest this was a linear relationship in the text. We have also added that potentially higher solar intensity could contribute.

Finally, to further improve the manuscript, I recommend that the authors thoroughly revise their manuscript to present the information more precisely and concisely. In particular, the authors should focus on revising the syntax of their sentences. A rule of thumb I can recommend: if the sentence does not add new information, delete it.

The authors have revised the manuscript to be shorter.

I address these issues further in my specific comments. Reviewer specific comments: Title: Much of the manuscript focuses on back trajectories and I think it might be valuable to include that aspect in the title. I would also encourage the authors to specify which "air pollutants and short-lived climate forcers" they studied. Why not simply write O3 and BC? Also, there is no discussion on climatic impacts in the study.

We have discussed climate impacts further in the text.

Abstract: Line 15: The statement "air pollution is largely understudied in sub-Saharan Africa" should be supported. Why is it understudied? Because there is a lack of knowledge and expertise? A lack of funds? A lack of interest? The authors feel as if this is beyond the scope of the paper to speculate on this, as this is not scientific: it is a fact that it is largely understudied and that is now illustrated in Figure 1. Be specific. Line 23: 20% of what? Of the population? Yes, of the population. Lines 26-27: unclear that Rwanda has 4 seasons in one year (or that the two seasons represent the time since the beginning of the measurements). Rwanda does not have 4 seasons: it has two rainy and two dry seasons, as stated. Line 37: name examples of major East African capital cities We have removed this line. It is unclear within the abstract what are the major findings of the study. The authors should include quantitative data in their abstract. We have added more quantitative information as suggested. I

ntroduction: In general, the introduction is ineffective. It is too long and too broad. The introduction could be more effective by focusing on Rwanda's air quality rather than on Africa's air quality. The introduction begins on page 3, and the first time Rwanda is mentioned is on page 6. I recommend that the authors revise those three pages on African air quality into one short paragraph of 5-6 sentences. Furthermore, I recommend introducing the AGAGE network much sooner in the introduction and mention the network in the abstract since it is the first network station in Africa!

The introduction is now much shorter and we have added more details on site selection at the end of the introduction and in the methods section.

Line 50: I would disagree that little scientific research has been performed on air quality in Africa, unless it is in comparison with the Europe and North America (which would need to be specified). I would argue that important work on air quality in Africa has been done since the 80s. (See (Stevens, 1987) as an example) The authors would argue that much has changed in Africa since the 80s. Perhaps it would be more effective for the authors to identify gaps in knowledge, rather than downplaying the existing research. The authors did not mean to offend or downplay pas research, and have tried to include as many past studies as possible. However, the authors maintain that long-term on-ground data is not prevalent on the continent, and particularly in East and equatorial Africa, except in certain countries (like South Africa). Lines 55-56: more recent references can also be included here. This has been removed Line 72: the authors say "past studies" but only reference one single study. Lines 83-85: add SAFARI campaigns (Otter et al., 2002; Swap et al., 2002b, 2002a) and Cape Point GAW station (Brunke and Scheel, 1998) This sentence has been removed as per the reviewer's request to shorten the introduction. Lines 98-101: the authors argue that long-term high-frequency measurements are important and needed, but this study focuses on monthly averages. Did the authors consider showing a case study with high frequency episodes? The authors did do an initial examination of the high frequency spikes in pollution; however, no emerging trends were found. A case study has been added. Discussion paper Lines 112-114: do the authors mean in comparison to Nairobi? Lines 120-121: add reference Lines 127-131: add reference Lines 134-137: unclear. What is meant here? Lines 144-146: missing reference Line 161: is the goal of the study really to understand air pollution in all of Africa? I recommend revising for a Rwandan context. Methods: Section 2.1: As a reader, I would be interested in knowing at the beginning of this section why RCO was chosen as the location for the AGAGE network. Was the intention to capture regional air pollution (as mentioned in the last sentence)? To sample free

tropospheric air masses? This has been added In the methods section. Line 190: what checks are in place at the station to ensure the diesel generator exhaust fumes are not sampled? Very high short-lived spikes in BC were removed, and the generator was 500 m below the station. Table 1: additional columns could include minimum and maximum concentrations observed by each instrument, calibration frequencies, LODs, etc. I recommend that the authors add a data processing section in their methods. Data processing was standard, and we have added a calibration frequency column, and the minimum and maximum can be observed in the graphs. How did they quality control the data? Results and Discussion: Figure 2: why is temperature constant at the beginning of the measurement period? Appears that data quality control is incomplete for temperature and CO trace (dotted lines between gaps in data).Thank you for pointing this out, as it would be confusing to readers, we now realize. This was due to a graphing issue (the graphing program takes no data and draws a line to the next). We have split the data between gaps and graphed it separately, it was not related to the data quality control. Are the lighter colour traces averages, running averages, extrapolations? Specify. This was specified in the figure caption, they are daily averages. Figure 3: Why did the authors choose to use normalized values. Wouldn't absolute values be more meaningful to highlight and air quality problem? The authors have shown absolute values in the previous figure (figure 2). Normalized values are shown to show each pollutant on the same graph for comparisons. The authors must be consistent in their graphing - each graph has different types of error representations. Choose one and use throughout each panel. All averaged graphs have the same error, 95% confidence intervals. The authors think that different types of graphs can have errors represented different ways for clarity of graph. Figure 4: include a graph for RCO to effectively compare the three sites. Explain why data is incomplete for Uganda. Elaborate on the significant different in BC concentrations in DJF between Kampala, Addis and RCO. Nonetheless, comparing two urban sites with RCO is not so meaningful since they are affected by local sources to highly different extents. RCO was not included as it was shown in Figure 3 and is BC, not PM. No PM data exists publically at this time in Rwanda over time. Uganda data is not complete, as the instrument had only been running for the number of months shown, though we have updated it to include more recent months as they have passed. We have elaborated on the differences in Kampala and Addis in the text. (Line 268: the authors could add the WHO's lines to their plots as a graphic reference point. Lines 278-282: I believe the authors are suggesting that local air pollution is more problematic than regional air pollution? The authors agree that regional pollution/biomass burning is problematic and were not trying to suggest that it was more important than regional air pollution. However, there is some local pollution which should be explored. We can see we were not as clear as we would have liked about this issue, so we have reworked this section. Rwanda's local emissions are more controllable at this point so they were more discussed (as regulations would require one government), and these local emissions are what will grow with population increases and development, NOT biomass burning (except for slash and burn agriculture, potentially). However, their data for RCO suggests the opposite, that in fact regional air pollution elevates the background level to such high concentrations that addition cooking fires do not make a significant contribution to concentrations measured at RCO. This result might be difficult and problematic for mitigating air pollution in Rwanda. Figure 5: MODIS data should not be presented in the rainbow color scale. I recommend using a two colour bar so that it is clearer whether the FRP is low or high (like the bluered color bar). We have changed the color. The excellent match between FRP and BC concentrations is highly significant and should be further discussed in the paper. This comparison is

striking! In the caption - do not use short/long to describe the different seasons. They are all of the same length - 3 months. We have removed. Figure 6: I have issues with the meaning of this figure. The comparison is problematic. Rwanda's bar is from RCO, a regional site whereas the comparison to other countries is an average of a number of sites throughout countries. This figure is unfortunately meaningless. BC data could be compared to other background and mountain sites - not between countries. Furthermore, if the authors want to highlight a pollution problem, then a better approach could include highlighting maximum daily pollutant levels (and/or exceedances) instead of averages. The authors believe that yearly averages show long-term pollution concentrations and RCO's context globally is interesting and relevant, but more text has been added to this section to show that these numbers are not directly comparable. The authors do not believe it is meaningless to put the RCO numbers in an international context. Figure 7: be consistent with panel readings (top to bottom) when Figure 2 is bottom up. Figure 8: Diurnal profiles are clearly not influenced by local emissions. Traffic peaks are not observed in the morning, nor in the evening. A discussion on boundary layer breakup is missing from the discussion. Also, I have never heard of a nocturnal boundary layer collapsing in the evening (lines 430-432); this explanation is wrong. This was worded incorrectly and has been removed. We have also added a discussion about boundary layer height. However, as there is a peak in the evening and morning that is distinct (and not a 'u' shape like the ozone) we do believe there is some influence from local emissions, as there is significant cooking in the valley around the station and there is high diesel generator use in the surrounding area. Rwanda is highly densely populated, so even a 'remote' mountain site is near a significant number of households. This shape persists throughout all seasons, so is not just black carbon aloft (as this would decrease in the seasons where there is less biomass burning). Figure 9: Why are running averages shown? What additional information to they provide? Discuss. The running mean is shown to show that AAE seems more seasonally/regionally, and less locally, influenced, as it is very similar to the daily averages. This text is added. Lines 476-482: show graphically, like in a pie chart. However, how important is this Rwandan information if pollution at RCO is regional? Lines 489-491: same issue as above - RCO measures regional air and so source apportionment would need to include surrounding countries' contributions. The authors feel it is important to include this information for future modelers or studies in the region. Rwanda also has good statistics on fuel use, as opposed to other nearby countries. The authors also believe that the numbers are more important than visualization in this case. We have also removed some of this discussion as per your earlier comment. Lines 569-578: show graphically Line 588: rainout is hypothesized as having an impact on the BC:CO ratio. The authors show precipitation data in Figure 9. They therefore have the information to investigate this effect accurately. Yes, we have investigated this and it is a reason. However, there is also non-local precipitation that may be affecting black carbon concentration in other areas, that may then lower the concentration of black carbon transported to the station.

Reviewer technical corrections: There are important changes that the authors can make to improve the quality of the writing and thus the efficiency of their communication. I would like to point out the following grammar and syntax recurring issues in the manuscript: 1. The word "this" should be followed by a noun. "Despite this," and "This is" is incorrect syntax (ex. Line 50, line 111, line 278 and more). 2. When enumerating a list, all listed items must be the same type of word. Either all nouns, all verbs, etc. a. ex line 59: "are known to increase aerosol and O3 conc and to transport aerosol. . ." b. lines 105-106:

"to increase. . .. And to improve. c. Lines 127-129 rewrite the listed items so they can be correctly enumerated d. Lines 175-179: revise syntax 3. Sentences longer than 2-3 lines of text need to be revised for syntax and conciseness. Specifically: Line 54: replace "certain" with "dry" Lines 56-61: syntax error - split into two sentences. (see point #2) Line 59: rephrase because aerosol fire tracers are molecular Lines 68-72: syntax error - rephrase Lines 158-159: unnecessary sentence; this message is continually repeated. Line 223: specify "regular" Lines 232-236: Move whole paragraph to the caption of the figures. Line 238: delete "it has been known for some time that"

The authors thank the reviewer for these suggestions and have addressed.

Additional references: Baier, B. C., Brune, W. H., Lefer, B. L., Miller, D. O. and Martins, D. K.: Direct ozone production rate measurements and their use in assessing ozone source and receptor regions for Houston in 2013, Atmos. Environ., 114(Journal Article), 83-91, doi:10.1016/j.atmosenv.2015.05.033, 2015. Brunke, E.-G. and Scheel, H. E.: Surface Ozone Measurements at Cape Point, in AtC8 ACPD Interactive comment Printer-friendly version Discussion paper mospheric Ozone: Proceedings of the XVIII Quadrennial Ozone Symposium L'Aquila, Italy, 12-21 September 1996, edited by R. D. Bojkov and G. Visconti, p. 7, Parco Scientifico e Tecnologici d'Abruzzo., 1998. Gao, J., Zhu, B., Xiao, H., Kang, H., Hou, X., Yin, Y., Zhang, L. and Miao, Q.: Diurnal variations and source apportionment of ozone at the summit of Mount Huang, a rural site in Eastern China, Environ. Pollut., 222, 513-522, doi:10.1016/j.envpol.2016.11.031, 2017. Geddes, J. A., Murphy, J. G. and Wang, D. K.: Long term changes in nitrogen oxides and volatile organic compounds in Toronto and the challenges facing local ozone control, Atmos. Environ., 43(21), 3407-3415, doi:10.1016/j.atmosenv.2009.03.053, 2009. Monks, P. S., Archibald, A. T., Colette, A., Cooper, O., Coyle, M., Derwent, R., Fowler, D., Granier, C., Law, K. S., Mills, G. E., Stevenson, D. S., Tarasova, O., Thouret, V., von Schneidemesser, E., Sommariva, R., Wild, O. and Williams, M. L.: Tropospheric ozone and its precursors from the urban to the global scale from air quality to short-lived climate forcer, Atmos Chem Phys, 15(15), 8889-8973, doi:10.5194/acp-15-8889-2015, 2015. Otter, L. B., Scholes, R. J., Dowty, P., Privette, J., Caylor, K., Ringrose, S., Mukelabai, M., Frost, P., Hanan, N., Totolo, O. and Veenendaal, E. M.: The Southern African Regional Science Initiative (SAFARI 2000)âAˇ´r: wet season campaigns, South Afr. J. Sci., 98(3-4), 131-137, 2002. Stevens, C. S.: Ozone formation in the greater Johannesburg region, Atmospheric Environ. 1967, 21(3), 523-530, doi:10.1016/0004-6981(87)90035-7, 1987. Swap, R. J., Annegarn, H. J. and Otter, L.: Southern African Regional Science Initiative (SAFARI 2000)âAˇ´r: summary of science plan, South Afr. J. Sci., 98(3-4), 119-124, 2002a. Swap, R. J., Annegarn, H. J., Suttles, J. T., Haywood, J., Helmlinger, M. C., Hely, C., C9 ACPD Interactive comment Printer-friendly version Discussion paper Hobbs, P. V., Holben, B. N., Ji, J., King, M. D., Landmann, T., Maenhaut, W., Otter, L., Pak, B., Piketh, S. J., Platnick, S., Privette, J., Roy, D., Thompson, A. M., Ward, D. and Yokelson, R.: The Southern African Regional Science Initiative (SAFARI 2000)âAˇ´r: overview of the dry season field campaign, South Afr. J. Sci., 98(3-4), 125-130, 2002b. Zhang, L., Jin, L., Zhao, T., Yin, Y., Zhu, B., Shan, Y., Guo, X., Tan, C., Gao, J. and Wang, H.: Diurnal variation of surface ozone in mountainous areas: Case study of Mt. Huang, East China, Sci. Total Environ., 538, 583-590, doi:10.1016/j.scitotenv.2015.08.096, 2015.

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 analysis 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.

The authors have reorganized the paper as suggested and significantly shortened the introduction. Additionally, we have tried to balance the biomass burning versus local emission discussion to take into account both reviewer's points.

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.

We have added a case study as suggested. We have also added a table (table 2) as suggested.

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.

We are unsure what the reviewer means here. We do not wish to show GHG measurements, as that is in a future paper and we worry it will confuse the discussions shown here (a graduate student has written his thesis on the long-term GHG measurements and we felt it was a natural split between air pollutants and GHGs). We do show met data in Figure 2, and we have added wind direction. HYSPLIT is shown in two figures.

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.

We do not have solar radiation data quality controlled at this time (there were lightning issues) and do not discuss solar radiation data in the methods section (so not sure how the reviewer knew we had solar radiation data). But we have added a reference from Safari and Gasore about solar radiation in Rwanda, as it does appear to increase in JJA vs DJF.

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 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 PM2.5 is reported in Hersey et al. (2015) is during winter and impacted by ground-level sources, and is not during biomass burning period. Figure 3 in Hersey et al. (2015) shows significant biomass burning in the region, however? (JJA, southern hemisphere winter, when there is also a peak in PM concentration). The Rwanda site is certainly impacted by biomass

burning. Discussions about the site and the boundary layer have been added to the methods section.

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. We have moved HYSPLIT and MODIS discussions into the methods. Introducing AAE in the methods would be confusing in the author's opinion, as what it is feeds directly into the discussion of what it means. 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? We have moved this discussion to the end of the paper as suggested, and removed part of the discussion. 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. MAM is not a period of time where BC is high, so the authors were not as concerned with this issue, additionally the FRP overall is low during this time. FRP and HYSPLIT are both inexact things, and the authors wished to convey with this figure that, when BC is high in Rwanda and the weather is dry, transport is from the major burn areas and biomass burning is high. This was not meant to be an exact comparison, as modeling or more detailed satellite measurements, beyond the scope of this paper, would be necessary for more quantitative comparisons (line 303). BC traces the best with FRP, and the authors wanted to reduce the complexity and repetitiveness of the presented graphs. 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. Answered above. Also fire count does not equal emissions or intensity so should not be directly comparable to BC. Line 316, what resolution were the geographical areas re-gridded to? I assume to match the input met resolution, but would state it. Gridded to 1 degree by 1 degree, now stated. Line 335, on the size of the maps in Figure 7, it is hard to see "local sources". The maps of cwt are not at good enough resolution to feel confident in 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. Ozone profiles. 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. We have added a sentence about aging. Aging typically increases AAE, but is not explicitly taken into account in the source apportionment model. 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? These were gaps, we have fixed. 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, t hen 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 are 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? More details have been added to the text. RCO is the only data point, we have made sure to highlight this in the text. 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 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. Ozone has been removed from this figure: ozone is formed in the atmosphere, thus source apportionment not exact. Madagascar has a number of fires, and that is likely the reason for the BC over the ocean. The authors believe that regional fire is impacting Rwanda and equatorial Africa in a significant way, and that the data shows this. Pollution is mixing in that region, especially during major burn periods, so exact identification of sources during wide-spread biomass burning episodes is difficult.
typos Line 82, I believe "later" should be "larger" Line 427, should read "...at RCO is different. . ."
Thank you