Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1027-RC4, 2020

Atmospheric Chemistry and Physics Discussions

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Interactive comment on "Seasonal source variability of carbonaceous aerosols at the Rwanda climate Observatory" *by* August Andersson et al.

Anonymous Referee #4

Received and published: 2 January 2020

The study of Andersson et al. reports dual isotope analysis method applied to particulate matter in African Savannah region. The study presents little scientific advancement and the value is only related to relatively scarce data from the region impacted by regional biomass burning. However, even that aspect is somewhat compromised due to observational platform located outside biomass burning region to assess full extent of biomass fires. Most of the connecting trajectories do not overpass fire impacted region and not surprisingly reports relatively low concentrations hardly possessing environmental concern. The site is important remote location for climatic observations, but hardly suitable to assess the impact and extent of regional biomass burning. It is, therefore, important to separate samples collected in fire connecting air masses versus

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Discussion paper

unaffected ones to reveal the extent more convincingly. The applied methods are well suited, but their application and especially discussion needs significant improvement. Dual isotope method is well established, but uncertainties related to C13 are downplayed to suit author's narrative. Given combined contribution to C13 ratio from three competing sources (C3, C4 plants and fossil sources) on top of kinetic fractionation affecting the ultimate ratio, uncertainty analysis needs to be much better elaborated and taken into consideration. Monte Carlo simulation is fine, but there is no information on bench-marking – what was the arbiter for the best solution? How Monte Carlo simulation compared to the observed isotope ratios? The paper can be accepted for publication in ACP, but given little scientific advancement requires much more objective consideration of sources, associated uncertainties and Monte Carlo simulation benchmarking.

Line 235. Out-gassing SO2 emissions can contribute very significantly to regional sulphate levels even without recognised volcanic activity (Ovadnevaite et al. 2009).

Line 246. Authors may also consider that OC/EC ratio is very dependent on the combustion stage - flame fires versus smouldering fires.

Line 321. d13 fossil ratio is very much region dependent and needs to be better constrained or uncertainty increased. It is possible that African liquid fossil fuel isotopic signatures are around -25, but it can be as low as -29 in other regions and, therefore, should be much better constrained or proven or uncertainty increased.

Line 328. Why the uncertainty of of C4 isotopic signature is twice smaller than of C3 plants? It also contradicts Figure 5 where C4 signature is $\sim$16.5+/-2. That seems to be biased low as C4 signatures are more in the region of -16 to -10 (something like -13+-3). C4 signatures need to be much better constrained which will have significant impact on source contribution and associated uncertainty.

Line 342. The result is highly contentious. Why only C4 plants burning are dominant during dry season? While only C3 plants contribute to wet season SOA? The statement contradicts Figure 6 where C4 and C3 contribution is about equal even during dry season. Figure 6 does not fit discussion and seem to be interpreted very subjectively.

Outlook section The authors should not discuss implications beyond presented data. Cloud brightening and broad climatic implications are not supported by the study data and seem to be out of place.

Figure 4. Uncertainty error bars are absent in the Figure.

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