

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-124

Anonymous Referee #2

Referee comment on "Measurement report: Source apportionment of volatile organic compounds at the remote high-altitude Maïdo observatory" by Bert Verreyken et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-124-RC2>, 2021

General Comments:

The paper by Verreyken et al. presents a 2-year dataset of selected volatile organic compounds (VOCs) measured using a quadrupole proton transfer reaction mass spectrometer at the Maïdo observatory, which is a remote tropical high-altitude site in the southern hemisphere located on the Reunion island in the south-west Indian Ocean. Measurements of such VOCs over such long periods in such atmospheric environments impacted by oceanic air and local emissions from within the island are rare. Using a source apportionment model (PMF v5.0) and some source tracer VOCs, the main sources affecting chemical composition of the air were investigated both on diurnal and seasonal basis. This is an interesting and valuable study which would be a great addition to the literature. Below please find some points which if I addressed in my opinion can improve the submission further. I recommend the paper to be accepted for publication once the points mentioned below have been addressed.

Specific Comments:

- As is understandable, the air at the remote high altitude is very clean compared to continental sites in Africa and Asia. This often poses challenges to the instrument detection limits and while the authors have generally done a good job in trying to address the issue, more details would be helpful for readers. For example, statistics on out of total number of measurements in each season for the reported compounds, how often were values below the detection limit. This could be provided as a table in the supplement to both inform about QA/QC and the challenges in measuring composition of clean air.
- It is mentioned that calibration experiments were done frequently ..please provide the sensitivity in ncps/ppb of the compounds so that reader can see the drift of the 2 year deployment
- Why were seasonal and diel profiles of six key VOC species analyzed which does not

include acetone and acetaldehyde? This is sub-optimal use of the dataset.

- What could be the reason for artefacts at m/z 93 which is normally well detected as toluene using a PTR-MS? On the other hand attribution of m/z 47 to ethanol and m/z 61 to formic acid and acetic acid is more challenging but no discussion of corrections and justification for these have been provided. What corrections were applied to correct for the HCHO back reaction with and its humidity dependence?
- The calibration gas mixture used (e.g. Apel Riemer) typically also contains trimethyl benzene detected at m/z 121. Was this compound completely absent in ambient air?
- Was a mass scan ever done at some point during the 2 year deployment?
- What was the residence time of air in the inlet and how often were filters changed during the 2 year long deployment?
- Radiation has been used intensively in the analyses so more detailed description of radiation measurements should be provided in the methods section.
- It is mentioned that sugarcane is a major crop cultivated on the island . Is it known whether sugarcane waste is burnt in post-harvest seasons? If so this would be interesting to compare with the BB profile and literature reported emission factors of the measured compounds (see studies from FIREX campaign published in ACP special issue.
- The analyses of isoprene oxidation chemistry could benefit a lot from comparison with other studies from forested sites. What were the NO levels in summer when isoprene and Isoprene oxidation products are highest? It would be very valuable to compare the daytime ratio of MVK+MACR+Isop peroxides to isoprene to that observed from forested sites in Europe, Africa, Asia and South America. Comparing yields reported from laboratory studies such as Liu et al. Liu, Y. J., Herdinger-Blatt, I., McKinney, K. A., and Martin, S. T.: Production of methyl vinyl ketone and methacrolein via the hydroperoxyl pathway of isoprene oxidation, *Atmos. Chem. Phys.*, 13, 5715–5730, <https://doi.org/10.5194/acp-13-5715-2013>, 2013 would be useful too.
- Is there information about the boundary layer height at day and night in different seasons from the site? This would make the discussion about role of emissions and dilution more quantitative. In BB air masses what is the CO/CO₂ mixing ratio?
- Detailed analyses of temperature and radiation regimes associated with highest isoprene emission and formation of photochemically formed compounds (see for e.g. Mishra et al. Emission drivers and variability of ambient isoprene, formaldehyde and acetaldehyde in north-west India during monsoon season, *Environmental Pollution*, Vol. 267, 115538, 2020) would also provide further mechanistic insights.
- Back trajectory modelling description and analyses needs more clarity..can these be compared to local wind direction and wind rose too? Esp PMF factor profile wind roses would be helpful to supplement the other analyses.
- For PMF analyses would have been good to treat NO_x as independent tracer with anthropogenic profile (Fig 11). Also for interpretation of anthropogenic source profile factor shown in Fig 11 would be really helpful to have the boundary layer height variation also in same plot even if from the met model in absence of measurements.
- The conclusion that marine sources do not show up as source factor sounds strange for an island so authors should clarify this is so for the burden of the specific VOCs

Minor points:

L6: Here and elsewhere should be sum of C₈-aromatic compounds...also good to discuss

which ones could be major contributors if speciation info from other studies is available.

L8: on air masses recorded is better replaced by air masses sampled here and elsewhere

L10: does not exhibit consistent diel variability is a not clear... What is consistent diel variability? Authors should clarify

L15: The term secondary biogenic sources is confusing

L19: should be mixing ratios and not concentrations

L53: 2 years may not be adequate for inter-annual variability?

L90: authors mention PBL variability but would be good to add info on measured PBLs between day and night if known

L106: How was m61 corrected for fragmentation effects?

L108: It is stated that m/z137 was used for quantification and 81 was not, although at 136 Td one would expect more than 60% of MT signal to land at 81. What fragmentation ratio was used and is there any information about the speciation of MT, even from other studies perhaps?