

Interactive comment on “Driving parameters of biogenic volatile organic compounds and consequences on new particle formation observed at an Eastern Mediterranean background site” by Cécile Debevec et al.

Anonymous Referee #1

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The manuscript by Debevec et al. describe VOC mixing ratios measured at Cyprus Atmospheric Observatory during one month, March 2015. The motivation for the paper is to find out the driving factors for new particle formation. The measurements cover both on-line and off-line measurements, altogether more than 60 compounds were detected. These kind of intensive campaigns are valuable, since there is still a lot of unknown reactive organic compounds in the atmosphere. Unfortunately, the measurements did not cover sesquiterpenes, since they are likely to be very important in new particle formation and there is very little ambient data of sesquiterpene mixing ratios.

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The paper is well written, it includes nice, informative figures and it suits well to be published ACP after minor revisions described below.

1. It is mentioned that ozone was removed in off-line sampling. MnO₂ removes also sesquiterpenes, which is unfortunate, since SQTs are likely to have an important contribution in SOA formation. How about ozone removal in BVOC on-line measurements? How was it removed? In the VOC intercomparison between the used methods on-line measurements showed lower values than off-line measurements. Could this be due to different ozone removal efficiency?
2. It is not quite correct to say that monoterpenes were the most abundant group, when only monoterpenes, isoprene and few oxygenated compounds were measured.
3. Was isoprene measured with PTR-MS only? As mentioned, also other compounds than isoprene can add to m/z 69, and it would be interesting to see a comparison of isoprene measurement in supplement with other comparisons. High night-time isoprene concentrations could be due to other compounds/fragments too. Inomata et al. conducted such a comparison (ACP; doi:10.5194/acp-10-7085-2010) and found that isoprene measurements with PTR-MS were overestimated in comparison with FID.
4. It is not self-evident that MBO is temperature and light dependent in a same way as isoprene as mentioned. At least in boreal forest this could not be proved (Tarvainen et al., ACP doi.org/10.5194/acp-5-989-2005).
5. The chapter 3.2 has a misleading title. The manuscript deals with ambient mixing ratios, not with emissions. When discussing variability of the mixing ratios, the atmospheric mixing is not taken into consideration. There is a lot of discussion about the effect of humidity and rain in the mixing ratios, but these can be due to lower mixing layer height. There is currently also another paper under review in ACPD (Hellén et al., doi.org/10.5194/acp-2018-399) which claims that mixing layer height and temperature are the main factors determining ambient mixing ratios. Is there a way to evaluate mixing layer height at CAO if not measured? This would be extremely valuable and needs

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to be taken into account.

6. Acetaldehyde is a known product of myriad of atmospheric reactions, for example OH radical reactions. OH radicals are produced only in sunlight and therefore acetaldehyde mixing ratios would also peak during daytime. The following sentence is from Millet et al. (ACP, 2010) abstract: "Hydrocarbon oxidation provides the largest acetaldehyde source in the model (128 Tg a⁻¹, a factor of 4 greater than the previous estimate), with alkanes, alkenes, and ethanol the main precursors. There is also a minor source from isoprene oxidation". Why are the authors convinced that VOC oxidation is not the cause for high midday acetaldehyde mixing ratios, but light dependent emissions?

7. Table showing the mean mixing ratios of individual compounds would be helpful.

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